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# Bulletin of the History of Medicine

THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

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# Bulletin of the History of Medicine

THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

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Founded by HENRY E. SIGERIST
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THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
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VOLUME 52 SPRING 1978 NUMBER 1

# THE HENRY E. SIGERIST SUPPLEMENTS

In June 1941, in a Preface to Volume X of the *Bulletin,*[1] Professor Sigerist, the founding editor, confirmed what he had announced in January —"I cannot accept long papers, but must limit myself to the publication of shorter papers which have no other way of reaching the public than through a journal"—but acknowledged that this policy had created a new problem, that of the essay too long for a journal and too short for a book.

"And yet there is no doubt that many valuable studies are made that have just that unpopular length. In the last few months, much to my regret, I was forced to refuse three such manuscripts, and I think that a way must be found to make that kind of study available to the public."

Although Sigerist had hoped to begin a series of Supplements in 1942, it was not until 1943 that No. 1 appeared—the famous monograph by Ludwig Edelstein, *The Hippocratic Oath: Text, Translation and Interpretation,* to be followed in 1944 by Walter Pagel's *The Religious and Philosophical Aspects of van Helmont's Science and Medicine* (no. 2) and by other splendid examples, such as Erwin H. Ackerknecht's *Malaria in the Upper Mississippi Valley 1760-1900* (1945) and *Fees and Fee Bills: Some Economic Aspects of Medical Practice in Nineteenth Century America* (1946) by George Rosen. Beginning with No. 11 the Supplements were edited by Owsei Temkin, who had taken over editorship of the *Bulletin,* but unfortunately financial considerations forced the discontinuance of the series with No. 13, *Caelius Aurelianus, Gynaecia: Fragments of a Latin version of Soranus' Gynaecia from a thirteenth century manuscript,* edited by Miriam F. Drabkin and Israel E. Drabkin. From classical antiquity to modern America, from medical ethics to medical fee bills, the Supplements had ranged widely during their short course but the standard of scholarship and the level of interest had remained high.

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[1] This was the second volume for that year, two volumes per annum having been issued from 1941 to 1946

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Now a new series begins. As long ago as 1968-69, when Saul Jarcho of New York was President of the American Association for the History of Medicine, a stimulus was given to the idea and subsequently, in the regimes of Whitfield J. Bell, Jr., and John B. Blake, it was by no means forgotten by President or Council in their discussions of publication subsidy. In 1973 the Editorial Committee chaired by James H. Cassedy during the Blake presidency, recommended that the "annual amount budgeted for additional publication support . . . should not be parcelled out in small portions. Rather, it should be allocated to single substantial projects. . . . The Committee believes that the type of publication most needing and most meriting further support at this time is the short monograph. . . ." At the annual meeting in 1974 the Council approved the report and voted unanimously to recommend action to the membership. At the subsequent annual business meeting (4 May 1974) Dr. Lester King moved that an appropriation be made for the purpose and the motion carried unanimously. Thus was a new series of Supplements launched. The report so adopted included the following sentence: "We suggest that manuscripts be reviewed by the Editorial Committee as well as by the Editor of the *Bulletin* prior to awarding funds for any Supplement though, once selected, control over the manuscript would, of course, be exercised by the Editor of the *Bulletin,*" and this section of the report was remarked on by the Council when it voted its approval.[2]

The first Editorial Committee to take action to this end in consort with the Editor was chaired by Professor Charles Rosenberg, and the manuscript selected to lead off the new series is a treatise entitled *Almost Persuaded: The First American Debate over Compulsory Health Insurance, 1912-1920,* by Ronald L. Numbers, Associate Professor, Department of the History of

Facing page. *This portrait of Henry E. Sigerist appeared on the cover of* Time, *January 30, 1939. The issue also featured an article about Sigerist and his work entitled "History in a Tea Wagon" (pp. 51-53)—the title referred to a portable file in which he kept at hand notes of his most recent research. The* Time *cover is reproduced here with the permission of* Time *Magazine. The portrait of Sigerist is the work of Holmes I. Mettee (1884-1947), founder of a Baltimore photographic firm. The* Bulletin *wishes to express its appreciation to Mettee Photographers for furnishing a print of the photograph with permission for its reproduction.*

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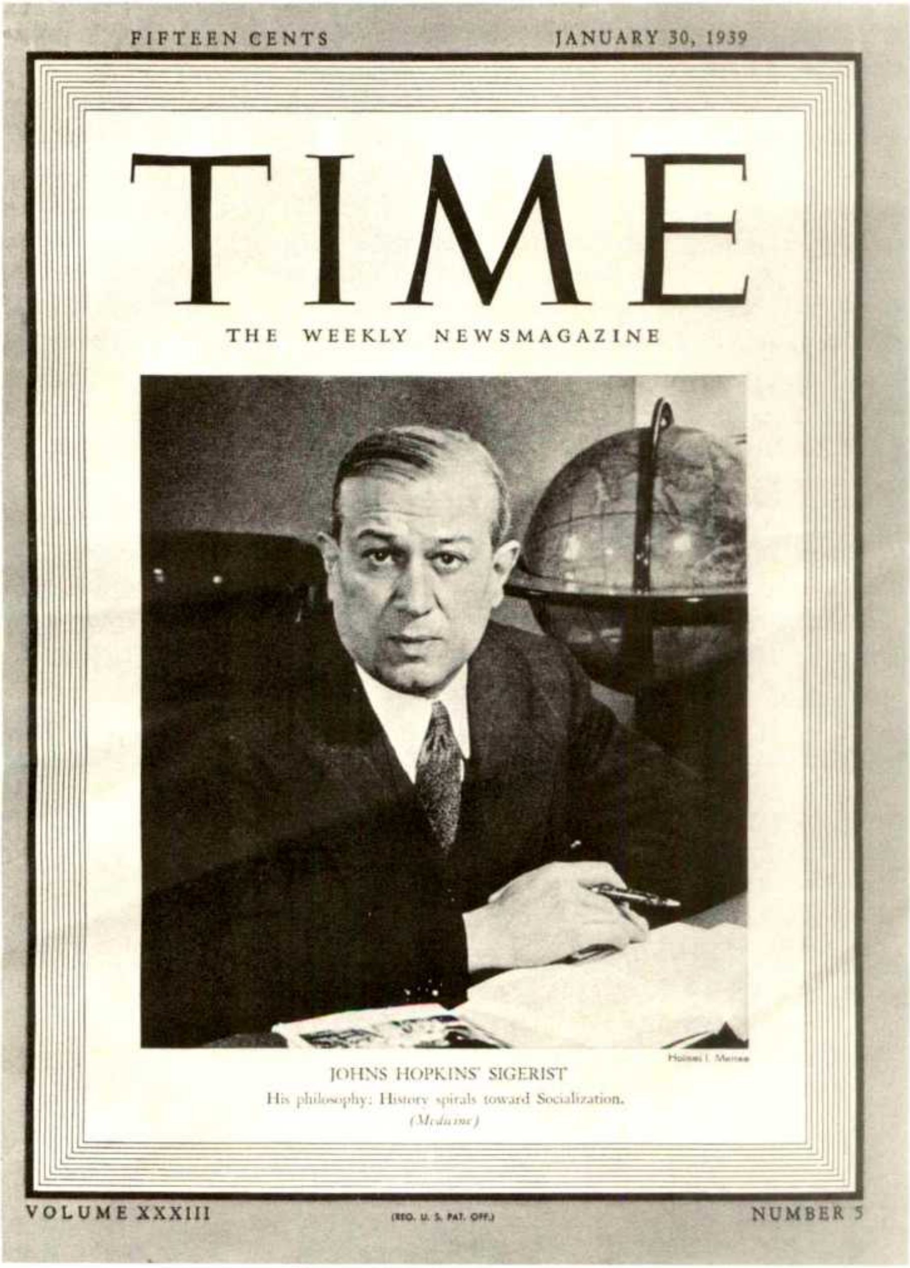
[2] American Association for the History of Medicine: Report of the Forty-Seventh Annual Meeting, Charleston, South Carolina, May 1-4, 1974 *Bull. Hist Med.,* 1975, *49:* 258-269



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THE WEEKLY NEWSMAGAZINE

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JOHNS HOPKINS' SIGERIST

His philosophy: History spirals toward Socialization.

*(Medicine)*

VOLUME XXXIII (REG. U. S. PAT. OFF.) NUMBER 5

"History spirals toward Socialization."
Henry E. Sigerist's message to *Time* readers in 1939.

Medicine, University of Wisconsin. It will be published by the Johns Hopkins University Press in the spring of 1978. The theme is one that would have been of very special interest to Professor Sigerist. What could be a more appropriate tribute to the memory of the man who founded both the *Bulletin* and the first series of Supplements than that the new series should bear his name, appearing from this time forth as the Henry E. Sigerist Supplements? That this should be done was recently determined by President John Duffy and the Council, rounding out the work of Saul Jarcho, Whitfield Bell, John Blake, Lester King and the members of the previous councils who have moved in the direction of reestablishing this valuable augmentation to the *Bulletin;* they have given new life to a dream first dreamed by Sigerist a generation ago. Thirty-five years after the first Supplement and twenty-seven years after the last, the founder's idea is reborn.

It was Sigerist's resolve that the *Bulletin* should provide the organ not only of the Johns Hopkins Institute but also of the American Association, and to this day it subserves both functions. For this reason, too, it is eminently suitable that the new series should bear the founder's name and perpetuate his scholarly interests. These interests were so broad and so varied that the Supplements will recognize no narrow limits in range of theme and will publish historical essays of greater scope that the *Bulletin* itself can accommodate. It is not too much to hope that in time the Henry E. Sigerist Supplements will help to extend the purview of medical history.

LLOYD G. STEVENSON

# THE SUGAR CONNECTION: A NEW PERSPECTIVE ON THE HISTORY OF YELLOW FEVER

JAMES D. GOODYEAR

The history of yellow fever is an enigma. It is uncertain whether the disease first appeared in West Africa, the islands of the Caribbean, or Central America. Also unexplained is the spontaneous disappearance of yellow fever from locales—such as North American seaports—where it had been a major health hazard for centuries. A third mystery is the non-appearance ever of yellow fever in Asia or Australasia.[1] In an effort to supply a hypothesis that may open these puzzles, this essay offers a new historical perspective on the epidemiology of yellow fever. There is an apparent connection in time and place between epidemic yellow fever and the presence of sugar cane cultivation, milling, refining, or shipping. My intention is to discuss characteristics of the cane sugar business, and their potential influence on the epidemiology of yellow fever. It is hoped that this "ecological approach" toward yellow fever may shed light on the history of the disease.[2]

## *Background: Epidemiological and Historical*

Epidemics of yellow fever follow two separate patterns. The classic variety is urban yellow fever in which the virus is transmitted from man to man by the domestic mosquito *Aedes aegypti.* In Africa other members of the genus *Aedes* may serve as vectors in this urban cycle but *A. aegypti* predominates. In the Americas *A. aegypti* is the only mosquito proven to participate in the urban form of the disease.[3] The second epidemiological cycle is sylvan (or jungle) yellow fever. This form was discovered in the

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[1] All sources dealing with the history of yellow fever acknowledge uncertainty about their subject For a recent summary of some epidemiological enigmas surrounding the disease see James S Ward *Yellow Fever in Latin America A Geographical Study,* Centre for Latin American Studies. Monograph Series No. 3 (Liverpool. University of Liverpool, 1972), pp. 1-9, also Richard M Taylor. "Epidemiology," in *Yellow Fever,* ed. George K. Strode (New York· McGraw-Hill Book Company, 1951), pp. 529-533

[2] Concepts underlying an ecological approach to human disease are well stated in Sir MacFarlane Burnet and David O. White, *Natural History of Infectious Disease,* 4th ed (Cambridge Cambridge University Press, 1972), pp. 1-21

[3] Loring Whitman, "The Arthropod Vectors of Yellow Fever," in *Yellow Fever,* ed Strode, p 294.

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1930s when varieties of wild (i.e. non-domestic) mosquitoes were found to transmit yellow fever virus among small forest animals. In the Western Hemisphere, the most prominent of these mosquitoes are of the genus *Haemagogus,* and include species *H. capricornii, H. spegazzinii,* and *H. spegazzinii falco.* They live in the forest canopy feeding on, and thereby infecting, monkeys which inhabit the same locale. In non-urban Africa wild varieties of *Aedes* act as vectors in the same fashion.[4] Thus sylvan yellow fever is usually epizootic; man is involved in the cycle only by accident.[5] Exactly how the urban and sylvan cycles overlap or intersect confuses the history of the disease.

No clinical distinction can be made between these two forms. Both involve the same virus, and both demonstrate the same signs and symptoms among similar populations. These include: sudden onset, severe headache, fever, jaundice, albuminuria, and signs of haemorrhaging, such as bleeding gums, melaena, petechiae, or blood vomit. The course of the disease is characterized by (1) high fever for one to three days, (2) general lassitude without fever for a short number of days, (3) increasing nausea and jaundice, (4) death within ten days, with a clustering around the sixth day after the onset of fever. Mortality is generally high, running from a tenth to a half of those infected. Women survive the disease better than men. Infected persons under fourteen usually escape death.[6]

The course of the disease and its symptoms are not unique to yellow fever. Even today laboratory analysis of liver tissue is often necessary to distinguish a case of yellow fever from a number of other diseases.[7] Misdiagnosis is even a greater problem in interpreting accounts of pestilence in past centuries. Such clinical or epidemiological descriptions of illnesses more often than not are insufficient in detail or are too imprecise in language to identify an epidemic positively as yellow fever. For purposes of historical identification the most indicative characteristics are blood vomit and high mortality. However, these alone cannot confirm the presence of the virus, and so it is questionable which past epidemics can be ascribed to yellow fever.

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[4] *Ibid.*, pp 262-289.

[5] Fred L Soper, *Building the Health Bridge, Selections from the Works of Fred L. Soper, M.D.*, ed. J. Austin Kerr (Bloomington and London Indiana University Press, 1970), p 159

[6] J. Austin Kerr, "The Clinical Aspects and Diagnosis of Yellow Fever," in *Yellow Fever*, ed. Strode, pp. 391-399.

[7] Epidemic diseases which have been confused with yellow fever because of epidemiological or symptomatological likeness include: malaria, scurvy, typhus, dengue, plague, and relapsing fever, see Henry Rose Carter, *Yellow Fever, An Epidemiological and Historical Study of its Place of Origin* (Baltimore Williams and Wilkins, 1931), pp. 49-77.

Armed with a knowledge of the symptoms and epidemiology of the disease, investigators have long argued about the original home of yellow fever. The debate, prior to the discovery of the sylvan cycle, included a number of fascinating speculations about the disease.[8] However, two basic positions predominated. One favored an African origin; the other insisted that yellow fever was indigenous only to the Americas.[9]

The proponents of the former position cited entymological and epidemiological evidence in support of their argument. *A. aegypti* exists in Africa in three genetically controlled habitats—feral, peridomestic, and domestic.[10] In the Americas there is only a domestic *A. aegypti* population. In terms of evolution the feral population of the species is considered to be the oldest, which argues heavily for *A. aegypti* being native to Africa, and of fairly recent importation to the Americas.[11] Because this species of mosquito plays such a prominent role in the epidemiology of yellow fever, its natural history favors Africa as the home of the disease.

Epidemiological evidence supporting Africa as the first locale of the disease is more tenuous. A tendency has been noted for Africans or persons of African descent either to be immune to the virus or, if infected, to suffer a milder attack than Europeans, Native Americans, or Asians.[12] Such observations, although they do not supply conclusive proof, suggest: (1) Africans through a longer association with the disease developed a genetic tolerance making them less susceptible, and/or (2) they commonly were infected in childhood when the disease is less likely to be fatal, and acquired life-long immunity which statistically would have shown a lower incidence of infection for them in locales outside of Africa.[13] In either case, the association of yellow fever and Africa appears extensive and of long duration.

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[8] George Augustin, *History of Yellow Fever* (New Orleans. Searcy and Pfaff, 1909), pp. 85-130 The author of this massive study cites four theories on the "cradle" of yellow fever, including the "Gulf Stream Theory" of origin.

[9] Carter, one of the greatest of yellow fever epidemiologists, is the best spokesman for an African origin The most renowned proponent for the theory of American origin is Carlos Finlay who first hypothesized the critical role played by *A aegypti* in the epidemiology of yellow fever. For a succinct summary of Finlay's position see the work by his son, Carlos E Finlay, *Carlos Finlay and Yellow Fever* (New York· Oxford University Press, 1940), pp. 180-210.

[10] Milan Trpis and Walter Hausermann, "Demonstration of differential domesticity of *Aedes aegypti* (L ) (Diptera, Culicidae) in Africa by mark-release-recapture," *Bull Entomol. Res.*, 1975, *65*. 199-208.

[11] Sir S. Rickard Christophers, *Aedes Aegypti (L.) The Yellow Fever Mosquito, Its Life History, Bionomics and Structure* (Cambridge Cambridge University Press, 1960), pp 43-44

[12] Many have commented on this phenomenon. See John Duffy, *Epidemics in Colonial America* (Baton Rouge: Louisiana State University Press, 1953), pp. 142, 152, John H. Powell, *Bring Out Your Dead, The Great Plague in Philadelphia in 1793* (Philadelphia University of Pennsylvania Press, 1949), p 95, Edward Nathanial Bancroft, *An essay on the disease called yellow fever; with observations concerning febrile contagion, typhus fever, dysentery, and the plague, partly delivered as the Gulstonian Lectures before the College of Physicians, in the years 1806 and 1807* (Baltimore: Cushing and Jewett, 1821), pp 100-105, and the statistics of the 1853 epidemic at Rio de Janeiro in Francisco de Paulo Candido, *Relatorio sobre a salubridade da cidade do Rio de Janeiro em geral, e a febre amarella em particular* (Rio de Janeiro· Typografia Nacional, 1853).

Early proponents for the Americas' being the original location of the disease based this position on their interpretation of the historical record of suspicious epidemics. Contingents of European explorers and colonists in the New World had high mortality rates which some investigators have attributed wholesale to yellow fever.[14] Specific documentation about medical causes of these catastrophes is generally meagre. It is unclear whether indeed it was yellow fever which plagued the early settlers. However, it is now generally accepted that the first confirmed description of a yellow fever epidemic comes from the Yucatán. In 1648 Diego López de Cogolludo, a Franciscan friar, witnessed a disease in which "patients [suffered] severe and intense headache [and] vomiting of blood . . . ; the majority died on the fifth [day] and very few reached the seventh . . . ; although a great many women were taken sick the disease was less severe in them than in men [and] of the young children who were attacked by the *peste* only a few died as compared with the adults."[15] The disease was concentrated among Spaniards in the towns of Mérida and Campeche. Mayan chroniclers of the period called this epidemic *xekik*, and explained it as divine retribution against the colonists. Copies of chronicles relating events in pre-Columbian Yucatán also refer to a pestilence as *xekik*. That disease, said to be highly fatal and characterized by blood vomit, took place in "houses within the fortress" sometime in the 1480s.[16] Because the copies of these documents that exist today post-date the 1648 epidemic, it is uncertain if a knowledge of the later pestilence influenced those scribes who copied the earlier records.[17] This raises doubt as to whether or not the two diseases termed *xekik* were in fact identical.

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[13] Carter, *Yellow Fever. An Epidemiological and Historical Study*, p. 81. It has also been noted that under experimental conditons African monkeys survive the disease more readily than Asian or New World primates, Taylor, "Epidemiology," p 530.

[14] Augustin, *History of Yellow Fever*, pp. 87-99, 639-649, and L. J. B Bérenger-Féraud, *Traité Theorique & Clinique de la Fièvre Jaune* (Paris Octave Doin, 1890) pp 23-27.

[15] The quotation is in Finlay, *Carlos Finlay*, pp 206-208, for a fuller exposition see Carter, *Yellow Fever An Epidemiological and Historical Study*, pp 147-151. The original source is Fr Diego López de Cogolludo, *Los tres siglos de la dominación española en Yucatán o sea historia de esta provincia*, II (Graz, Austria. Akademische Druck-und Verlagsanstalt, 1971), pp 633-650

[16] Diego de Landa, *Landa's relación de las cosas de Yucatán*, ed Alfred M. Tozzer. Papers of the Peabody Museum of American Archaelogy and Ethnology Vol XVIII (Cambridge Harvard University, 1941), p 41 Because of a debate on interpreting the Mayan calendar the exact year is uncertain, but all agree the reference is to a date about ten years prior to Columbus' initial New World voyage.

The recorded history of yellow fever in Africa does not disprove the theory of American origin, but does raise questions. Diseases with symptoms similar to those characterizing yellow fever beset sixteenth and seventeenth century European fleets at Santiago in the Cape Verdes and at the island of São Thomé.[18] It has been claimed that the first clinical description (1623) of yellow fever was by Aleixo de Abreu, a Portuguese army surgeon who witnessed, suffered, and treated this disease in Luanda between 1594 and 1606.[19] But the first recorded epidemic of yellow fever in Africa confirmed by modern epidemiologists dates only from 1778, or 130 years after its American counterpart.[20]

The discovery of sylvan yellow fever has modified this debate. One theory holds that the disease, in both its epidemiological cycles, is indigenous to Africa. During the slave trade infected mosquitoes were imported to the Americas where they passed the disease among men and, inadvertently, on to monkeys which led to the sylvan cycle on the western side of the Atlantic.[21] A second theory is that sylvan yellow fever is indigenous to both Africa and the Americas, but that the urban variety is indigenous only to Africa. Some time after 1492 Europeans imported *A. aegypti* to the New World. The eventual result was the "urbanization of yellow fever" at climatically appropriate times and cities throughout the Western Hemisphere.[22] This theory is attractive because it seemingly accomodates both Mayan history and entymological data on *A. aegypti*. It fails to explain how the Yucatán epidemic of the 1480s, occuring within the urban setting of houses inside a fort, could have been yellow fever without the presence of the vector, *A. aegypti*, which had yet to be imported.

[17] Ralph Roys, *The Book of Chilam Balam of Chumayel* (Washington, D C Carnegie Institution, 1933), pp 5, 120, 133. On the sources for Mayan medicine see Francisco Guerra, "Maya Medicine," *Medical History*, 1964, *8*: 31-43

[18] Carter, *Yellow Fever An Epidemiological and Historical Study*, pp 229-235, notes that fleets under Sir Francis Drake in 1585 at Santiago, Admiral Van der Does in 1599 at São Thomé, and Admiral Jols in 1641 at São Thomé encountered diseases characteristic of yellow fever.

[19] Francisco Guerra, "Aleixo de Abreu (1568-1630), author of the earliest book on tropical medicine describing amoebiasis, malaria, typhoid fever, scurvy, yellow fever, dracontiasis, trichuriasis, and tungiasis in 1623," *J Trop Med & Hyg*, 1968, *71* 51-69.

[20] Carter, *Yellow Fever An Epidemiological and Historical Study*, p. 69, credits this first record to J P Schotte who witnessed "a severe epidemic among British troops at St Louis de Senegal."

[21] Taylor, "Epidemiology," p 531

[22] Ward, *Yellow Fever in Latin America*, p. 5 and fig 1

Here the debate falters. One side must still reconcile favorable entymological evidence with a fairly negative historical record of the disease early in African history. The other side faces the twin problems of ambiguous documentation, and the recent importation of *A. aegypti* to the Americas. It is possible that unknown sources may be uncovered that may resolve the issue. Lacking these, a new interpretation of the *confirmed* history of the disease may offer a new perspective on the mysteries surrounding the origin of yellow fever.

## *Associative Aspects of Sugar and Yellow Fever*

### I

Epidemiological requirements for urban yellow fever include presence of the virus, susceptibles, and *Aedes aegypti.* A ready supply of these mosquitoes is mandatory because they serve both as vectors *and* reservoir of the virus. That *A. aegypti* crossed the Atlantic on slave ships explains how this African mosquito promoted pestilence in the Americas. It does not explain why the first documented outbreak of yellow fever occurred well over one hundred years after the start of the slave trade. It has been theorized that the first appearance of the disease was due to increased buccaneering. Growing numbers of pirates in Caribbean waters increased the percentage of susceptibles in the total population. This permitted a widespread yellow fever epidemic.[23] However, the scenario ignores the role of *A. aegypti.*

Any epidemic of yellow fever requires a "critical number" of *A. aegypti,* i.e., a certain number of potential vectors relative to the susceptible population.[24] This suggests that the history of the disease focuses on the yellow fever mosquito. The question may then be asked: what environmental conditions existed in new locales which permitted *A. aegypti* to proliferate to the critical numbers required to initiate and sustain epidemics of yellow fever? Because this mosquito in the Americas lives in close association with man, the answer to this question may lie in finding a pattern of human occupation common to the areas in which the disease occurred. When we look at Caribbean epidemics of the 1640s one socio-economic activity repeatedly appears—the cane sugar business. The proximity of sugar growing, processing, and shipping to areas of perennial yellow fever in the seventeenth and eighteenth centuries suggests that the sugar industry significantly changed the ecology of some Caribbean islands in such a way as to promote the proliferation of *A. aegypti*. If so, sugar has played an important role in the epidemiological history of this infamous disease.

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[23] H Harold Scott, *A History of Tropical Medicine,* I (London Edward Arnold, 1939), p. 287.

[24] Carter, *Yellow Fever An Epidemiological and Historical Study,* pp. 18-19. It is not necessary to exterminate every potential vector of yellow fever in a given locale to eradicate the disease. "In dealing with aegypti-transmitted yellow fever experience shows that the critical index [i.e number] has been reached when a careful house-to-house inspection reveals the aquatic forms of the vector on not more than 5 per cent of the premises visited." Soper, *Health Bridge,* p. 306.

The character of the early sugar industry would have been compatible with the bionomics of *A. aegypti*. This mosquito must take a blood meal in order to acquire or transmit the virus, but *A. aegypti* do not live by blood alone. They are especially attracted to sweet fluids, the sweeter the more attractive.[25] Males take nourishment from fruits, honey, flowers and sugar.[26] Females have been observed eating honey for the first two days of their adult lives before choosing to switch to a blood diet.[27] In the absence of blood meals, females easily live on a regimen of sugar or honey. Experimenters have found that females fed strictly on a diet of honey live well over two months and often over three, with 154 days being the longevity record.[28] There is speculation that females will survive longer if fed only sugary foods as opposed to blood meals.[29] A diet of sucrose supplemented with blood does not impair egg production.[30] There are also reports of oviposition when strictly peptone and sugar, or milk and sugar is given as food.[31] This "autogenous" behavior could have important consequence for supporting a mosquito population in the temporary absence of blood meals.[32]

The seventeenth-century sugar plantation made sucrose available in some form the year round. The planting and harvesting of cane was staggered in accordance with the seasons and the twelve to eighteen months growing time required for cane to ripen. This meant that cane was growing throughout the year. Harvesting and milling were concentrated in the dry months, while the processing activities of boiling down fresh cane juice, reboiling molasses, distilling, and curing clayed (white) or *muscavado* (brown) sugars took place over a twelve month period. Sucrose would have been readily available in waste stalks (*bagasse*) that were discarded after being pressed. It would have been especially abundant in the cooling vats that held the boiled product, and in the run-off from the pots that held the sugary syrup while it was crystalizing and being clarified.[33]

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[25] Calvin Lang and Robert Wallis, "An artificial feeding procedure for *Aedes aegypti* using sucrose as a stimulant," *Am J. Trop Med. & Hyg*, 1956, *5* 915-920.

[26] Christophers, *Aedes Aegypti (L.)*, pp 170, 468.

[27] J W. Scott Macfie, "Observations on the bionomics of *Stegomyia fasciata*," *Bull. Entol Res.*, 1915, *6*. 205-229. R. M. Gordon, "Notes on the bionomics of *Stegomyia calopus*, Meigen, in Brazil. Part II," *Ann Trop. Med. & Parasit.*, 1922, *16*· 425-439.

[28] Leland O Howard, Harrison G Dyar and Frederick Knab, *The Mosquitoes of North and Central America and the West Indies*, vol. I (Washington. Carnegie Institution, 1912), p. 270.

[29] The experimenter in question is E A. Goeldi, *Os mosquitos no Pará* (Paris· Wiegandt, 1905), cited in Howard et al., *Mosquitoes*, p. 270.

[30] Lang and Wallis, "Artificial feeding."

[31] On the relationship of nutrition and egg laying in *A aegypti*, see J. W Fielding, "Notes on the bionomics of *Stegomyia fasciata* Fabr.," *Ann Trop Med & Parasitol*, 1919, *13* 259-296.

[32] Autogeny is the capacity to reproduce "without feeding on vertebrate blood" Milan Trpis, "Autogeny in diverse populations of *Aedes aegypti* from East Africa," *Tropenmed & Parasitol.*, 1977, *28*: 77-82

The character of sugar processing created favorable breeding areas for *A. aegypti*. These mosquitoes prefer to breed in small catches of relatively clean water. They find ideal various artificial containers such as gutters, water barrels, cans, jars, or those many little niches found in such places as junkyards,[34] which are common to areas other than sugar plantations. But the manufacture of sugar in the Caribbean did involve an artificial container that was extremely common and may have served this domestic mosquito as a breeding locale. This is the crude undecorated clay pot in which the crystalized product was clarified. The mass of boiled cane juice, once cooled, was ladled into these conical pots. Molasses drained out through a hole in the bottom of the pot, either into a trough or a second jar. To further purify the sugar a layer of moist clay was placed on top of the pot and water percolated through the crystals dissolving the remaining molasses. This watered-down molasses drained into a third form, and was kept separate from the unadulterated molasses. Both types of run-off were collected and distilled or reboiled. The sugar remained in the pots for about four months in order to dry. Then it was knocked out of the forms and briefly sun dried before shipping.[35]

Each mill making clayed sugars used these pots in great quantities. A sugar works in central Mexico in 1589 had on inventory over six thousand of these forms which were enough for only one-half of its annual production.

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[33] A valuable summary of sugar growing and processing techniques is provided by Ward Barrett, "Caribbean Sugar-Production Standards in the Seventeenth and Eighteenth Centuries," in John Parker (ed.), *Merchants and Scholars Essays in the History of Exploration and Trade* (Minneapolis University of Minnesota Press, 1965), pp. 145-169.

[34] Christophers, *Aedes Aegypti* (L ), pp. 57-59; ingenious experimentation on breeding habits of non-laboratory mosquito populations is found in Milan Trpis, "Dry season survival of *Aedes aegypti* eggs in various breeding sites in the Dar es Salaam area, Tanzania," *Bull. W. H O.*, 1972, *47*: 433-437.

[35] Richard Ligon, *A True and Exact History of the Island of Barbadoes* (London: Frank Cass, 1976), pp. 87-92, gives a thorough description of this process. Upon comparing a number of production manuals from the period, Barrett claims that curing procedures exhibited "uniform technology and the same standard operations" throughout the Caribbean, "Caribbean sugar-production standards," pp. 149-150.

The same works in the eighteenth century employed five full-time potters, each producing forty forms a day.[36] A seventeenth-century Jamaican inventory lists twelve hundred pots valued at a total of £90. A nineteenth-century Jamaican plantation had 11,100 pots in use during a normal year. Considering that even small West Indian planters needed "several hundred pots" and that there were many hundreds of mills throughout the Caribbean, these pots were quite abundant.[37] It is unknown what mill owners did with the forms when they broke, and they must have broken fairly regularly on account of the nature of the material and how they were treated.[38] But if the shards were discarded like used cans in a junkyard they would have provided a perfect breeding area for *A. aegypti.*

The above data establish the possibility that the characteristics of sugar and the sugar business, by providing feeding opportunities and breeding locales, promoted the survival of *A. aegypti* in the Caribbean. This raises the question of how congruent were sugar agriculture and yellow fever in that region between 1635 and 1655.

In these two decades well-identified outbreaks occurred in five separate Caribbean locales. The first one was the island of Guadeloupe, a member of the Leewards in the Lesser Antilles. In the seventeenth century, before European colonization, the island was occupied by Carib Indians. They had imported cane from elsewhere in the region—sugar cane being an Old World crop first brought to the Americas by Columbus. Thomas Gage, an English traveler and priest, saw it growing on Guadeloupe in 1625.[39] In 1635 five hundred and fifty French colonists established the first permanent settlement of Europeans on the island. They began growing cane and producing sugar by the 1640s.[40] During the first year of occupation the colonists suffered widespread mortality that may or may not have been in part due to yellow fever.[41] However, another epidemic in July 1648 certainly was yellow fever. This clearly occurred after the Europeans began producing sugar at Guadeloupe. And if the earlier epidemic on the island was yellow fever, the presence of sugar cane preceded it by at least ten years.

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[36] Ward Barrett, *The Sugar Hacienda of the Marqueses del Valle* (Minneapolis University of Minnesota Press, 1970), p 59 and fig 13

[37] Richard S. Dunn, *Sugar and Slaves The Rise of the English Planter Class in the English West Indies, 1624-1713* (New York· W. W. Norton, 1973), pp. 197-98.

[38] For passing comments on breakage see João Antonio Andreoni, *Cultura e Opulência do Brasil* 2nd ed. (São Paulo· Companhia Editora Nacional, 1970), p. 210, and Manuel Moreno-Fraginals, *The Sugarmill, The Socioeconomic Complex of Sugar in Cuba 1760-1860,* trans. Cedric Belfrauge (New York and London Monthly Review Press, 1976), p 40

[39] The fundamental work on all historical aspects of sugar and sugar cane is Noel Deerr, *The History of Sugar* 2 vols (London· Chapman and Hall, 1949), and the reference to Gage is vol. 1, p 18.

[40] *Ibid* , pp. 154, 230

[41] Carter, *Yellow Fever An Epidemiological and Historical Study*, pp. 181-183, details why he believes this mortality was caused primarily by famine Ward, *Yellow Fever in Latin America*, p 6, cites this case as "undoubtedly" yellow fever.

A second case involves Barbados, a Windward member of the Lesser Antilles. It was uninhabited when the British arrived in 1627. The early settlers attempted tobacco cultivation, but their product was inferior to that produced in Virginia and the enterprise failed.[42] In 1640 sugar cane was introduced and it flourished immediately. Richard Ligon, a visitor to the island in 1647, found cane agriculture and processing well established. On arrival he witnessed a rampaging epidemic—Ligon himself contracted the disease but survived. The pestilence lasted through the following year causing great mortality.[43] This same malady remained an endemic health hazard in the island, and became recognized as yellow fever.[44] Thus Barbados offers a second example of yellow fever being preceded in a locale by sugar agriculture.

Yellow fever appeared concurrently at St. Kitts, a Leeward member of the same archipelago. In June 1648 the island suffered its first siege of the disease. The epidemic raged intermittently for eighteen months, killing one-third of the island's population. It is claimed that yellow fever was carried from St. Kitts aboard the vessel *Le Boeuf* to Guadeloupe in July 1648.[45] St. Kitts, colonized in 1624, had been cultivating cane and making sugar since 1644 and 1646 respectively.[46] Therefore, in this colony the presence of sugar agriculture also antedates the first outbreak of yellow fever.

The fourth Caribbean area involved in the 1648 yellow fever epidemic was the Yucatán, where Spain had begun pacification and colonization in the 1500s. During that century sugar was introduced with initial encouragement from the crown.[47] Gage, who traveled in the peninsula in the 1620s, cites cane as a local product.[48] Dominican friars in southern Yucatán also recorded that resident Maya cultivated sugar cane in the 1630s.[49] Throughout the remainder of the colonial period sugar was produced for local consumption.[50] Rudimentary claying techniques most probably were in use then, and were still being practiced in 1925 by relatively isolated groups of Maya in the interior.[51] Though sugar seems never to have been produced for export in colonial Yucatán, it was being grown and crudely refined at the time of the first documented epidemic of yellow fever.

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[42] Dunn, *Sugar and Slaves*, pp 49-59

[43] Ligon, *Barbadoes*, pp 21, 118

[44] Scott, *Tropical Medicine*, p 297

[45] Carter, *Yellow Fever An Epidemiological and Historical Study*, p 153

[46] Deerr, *Sugar*, vol 1, p. 237

[47] *Ibid* , p 137

[48] Thomas Gage, *Thomas Gage's Travels in the New World*, ed. J. Eric Thompson (Norman University of Oklahoma Press, 1958), p 93

[49] J Eric Thompson, "Sixteenth and seventeenth century reports on the Chol Mayas," *American Anthropologist*, n s , 1938, *40* 597

[50] Nelson Reed, *The Caste War of Yucatan* (Stanford· Stanford University Press, 1964), p 8.

Cuba is the fifth locale pertinent to the association of yellow fever with sugar. Cane was introduced to western Cuba early in the sixteenth century. The first production records show three mills in operation near Havana in 1576. At the start of the seventeenth century the industry began a mild expansion near Bayamo and Santiago de Cuba in the eastern part of the island. By 1617 thirty-seven mills were in existence and expansion then ceased. For over a century Cuban sugar production leveled off between three hundred and five hundred tons yearly.[52] Production began to rise in the mid 1700s, and was especially stimulated by the importation of ten thousand African slaves during the British occupation of Havana (1762-1763). Between 1760 and 1796 sugar production leaped from 2,550 tons per year to over 25,000 tons annually, which resulted in Cuba becoming a leading sugar exporter.

It is generally agreed that Cuba was free from any epidemic of yellow fever from 1511, the year of the first Spanish settlement, until 1761, with one exception.[53] This was a six-year period from 1649 to 1655. The virus, circulating among the Lesser Antilles, arrived at Havana (1649) and gradually spread eastward to strike Bayamo in 1653. By 1655 the disease had become extinct island-wide. It has been argued that the disappearance was due to a decrease in the number of susceptibles, either through death or acquired immunity.[54] However, Havana traditionally had been a center for ship building, and a major *entrepôt* for the yearly convoys plying between Spain and the Indies.[55] The city continued these activities in the years following its bout with yellow fever; that no further outbreaks of yellow fever occurred there until the 1760s was not caused by lack of susceptibles. The cause of this absence may have been a lack of suitable vectors. It is arguable that the *A. aegypti* population declined below the critical level required for an epidemic because yellow fever did not recur for over one hundred years in spite of a continual influx of susceptibles. When it did strike again, its reappearance and subsequent endemicity followed closely upon the incipient revolution in the Cuban sugar industry.

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[51] The technique is described in Robert Redfield and Alfonso Villa, *Chan Kom, A Maya Village* (Washington, D C · Carnegie Institution, 1934), p 29

[52] Deerr, *Sugar,* vol 1, pp. 126-131; cf Franklin W Knight. *Slave Society in Cuba during the Nineteenth Century* (Madison. University of Wisconsin Press, 1970), pp. 3-24, and Moreno-Fraginals, *The Sugarmill, passim*

[53] Carlos J Finlay, *Trabajos Selectos* (Havana Secretaría de Sanidad y Beneficiencia, 1912), p 212 Carter concurs with this opinion, see *Yellow Fever An Epidemiological and Historical Study,* pp 187, 190-191

[54] Carter, *Yellow Fever An Epidemiological and Historical Study,* p 18.

[55] Charles Gibson, *Spain in America* (New York Harper and Row, 1966), pp 103, 123, Hugh Thomas, *Cuba The Pursuit of Freedom* (New York. Harper and Row, 1971), pp 12-16.

## II

Of course, yellow fever epidemics did not occur only in the Caribbean. The general linkage of yellow fever and seaports is well documented. Sailing ships provided mosquitoes with adequate breeding areas, which explains how eggs and larvae endured transit between ports.[56] However, in order to introduce the virus into a non-infected area, adult *A. aegypti* must have occasionally survived an ocean voyage.[57] It is arguable that the survival of infected mosquitoes would have been significantly influenced by an abundant supply of appropriate non-blood meals. Hypothetically, a ship with a susceptible crew, infected mosquitoes, and a cargo of fruit or sugar would contain the optimum conditions for the proliferation of yellow fever. Such seems to have been exactly the case regarding the 1861 epidemic at St. Nazaire, France.

In mid-June of that year the *Anne-Marie* sailed out of Havana bound for St. Nazaire. She "carried barrels of sugar, between the tiers of which layers of sugar cane were placed, to properly dress the cargo."[58] *En route* there were three cases of yellow fever, two of which were fatal. On arrival there were no ill persons, so the crew was discharged. The sugar was unloaded by local longshoremen. Within a few days yellow fever had developed among these stevedores and workmen on adjacent ships. Forty-two cases were reported and twenty-five deaths, including a cooper "who had spent several days in the hold . . . repairing sugar barrels."[59] Never before or since did yellow fever occur in this port; and in 1918 a search for *A. aegypti* in the vicinity proved negative.[60] Lack of previous or subsequent epidemics and the absence of the proper vector at St. Nazaire indicate that conditions aboard the *Anne-Marie* could have been extremely conducive to the survival of infected mosquitoes. It is clear that the only time yellow fever ever invaded this French seaport was when it arrived aboard a sugar ship.[61]

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[56] Henry Carter, "Are vessels infected with yellow fever," *Medical Record*, 1902, *61*: 441-444; Soper, *Health Bridge*, p. 66; also see Lloyd Stevenson, "Putting disease on the map. The early use of spot maps in the study of yellow fever," *J. Med Hist. & All Sci.*, 1965, *20*: 226-261.

[57] The virus is non-inheritable so an adult mosquito must acquire it when taking a blood meal from an infected individual

[58] Augustin, *History of Yellow Fever*, p. 401

[59] *Ibid* , pp. 402-403

[60] Carter, *Yellow Fever. An Epidemiological and Historical Study*, p. 26

An epidemic of yellow fever at New York City in August and September of 1822 occurred under similar auspices. Because of its latitude, New York could not sustain a year-round mosquito population, but its summertime climate has traditionally permitted temporary infestation. As a result, and because of regular contact with ships from Caribbean ports, New York had a long familiarity with yellow fever. In colonial times four epidemics of varying severity invaded the city.[62] After U.S. independence, sporadic cases appeared almost yearly. A major outbreak in 1822 was traced to four ships recently arrived from Havana. Each carried a cargo of sugar and one had over six hundred boxes. An eyewitness claimed "there never perhaps had accumulated before, so large a mass of cargo from Havana vessels, at that particular season, and in so small a space."[63] The boxes were of rough pine and porous. When off-loaded they were stored in warehouses to which *A aegypti* in the neighborhood would certainly have had access. Imported sugars, by providing an exceptional feeding opportunity, may have helped temporarily to bolster the mosquito population in an area climatically unsuited to year-round infestation, and thereby contributed to an epidemic of yellow fever in a non-sugar growing locale. This suggests that the epidemiology of yellow fever in the United States may have been considerably influenced by the nature of the sugar trade.

## III

Importation of sugar to North American ports never lagged during the eighteenth century, and it grew precipitously after the Revolutionary War.[64] Prices increased and demand for this commodity kept pace.[65] To take advantage of these circumstances businessmen built refineries at major seaports: New York, Philadelphia, Boston, and Baltimore. There were approximately seven refineries in the colonies, but the number rose to seventeen in 1795 and to thirty-three by 1810.[66] The four decades between 1790 and 1830 coincided with the worst yellow fever epidemics on the eastern seaboard.[67]

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[61] Cf Howard, et al., *Mosquitoes*, p. 295, where it is stated "ships, particularly the ships loaded with sugar, have transported the terrible malady [i.e. yellow fever] to Africa" from the Caribbean

[62] Duffy, *Epidemics*, pp 146-157.

[63] Peter S Townsend, *An Account of the Yellow Fever, as it prevailed in the City of New York in the Summer and Autumn of 1822* (New York: O Halsted, 1823), p 25 The amount of sugar contained in a "box" varied. A survey of sugar imports to Baltimore in 1804 shows an average of 463 lbs of sugar per box If this figure is used to calculate the tonnage of 600 boxes it gives an estimate of 138 9 tons. Cargo data is located in *National Archives*, "Impost books for Baltimore," E 1149 and E 1174, 1803-1804.

[64] Richard B Sheridan, *Sugar and Slavery An Economic History of the British West Indies 1623-1775* (Baltimore The Johns Hopkins University Press, 1974), pp 314-316.

[65] Anne Bezanson, Robert D Gray, and Miriam Hussey, *Wholesale Prices in Philadelphia 1784-1861*, 2 vols (Philadelphia. University of Pennsylvania Press, 1936), vol 1, p. 334, and vol. 2, pp 146-147, 222-230

Refineries imported imperfectly cured or "raw" sugar which required further clarification to be saleable. It may be that raw sugar had greater consequence than clayed whites for the history of yellow fever in the United States. In the Caribbean islands raw sugar was cured rudimentarily in hogsheads. Each had a hole in the bottom out of which the molasses emptied. The time allowed for drainage, thirty days, was short compared to the fourth months allotted for claying.[68] Upon export, clarification was incomplete so the partially crystalized mass continued to drain during the voyage. Raw sugar was packed in a variety of wooden containers: hogsheads, tierces, barrels, half barrels, boxes, casks, etc. None was perfectly sealed to begin with and shifting cargoes subjected them to damage. Seepage of molasses was common, and a hogshead of raw sugar would arrive weighing less than it did on departure.[69] This run-off—in the absence of blood meals—could readily have sustained infected *A. aegypti* trapped in the storage holds at the time of embarkation. Upon unloading the sweet cargo, surviving mosquitoes would be able to escape and spread the virus while taking blood meals.

Philadelphia is an example of a sugar depot where yellow fever occurred repeatedly. Between 1699 and 1762 the city had four known outbreaks of the disease.[70] All were attributed to contact with the West Indies, especially the 1762 epidemic which was traced to "a West India ship coming to Sugar House Wharf below South Street."[71] Because Philadelphia was a major trading port for the thirteen colonies it had long been a distributing center for imported sugars and molasses. Later in the eighteenth century it became an important sugar refining center as well. Its first refinery was built in 1777 and three more were in operation before the close of the century.[72] This industry demanded larger imports of raw sugar than ever before for "the bulk of sugar consumed in this country before 1789 was from Philadelphia refineries."[73] It was during the 1790s that yellow fever returned to the city. The 1793 epidemic was a disaster in which ten percent of Philadelphia's total population perished.[74] It was followed by less severe epidemics in 1794, 1796, 1798, and into the first two decades of the nineteenth century. Never during the colonial period had this city endured such a sustained onslaught of the disease. Not until after the introduction of sugar refining did yellow fever become a major health hazard in Philadelphia.

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[66] Deerr, *History of Sugar*, pp. 461-462.

[67] An exception is Charleston, S C., which experienced its worst encounters with the disease early in the eighteenth century; see John Duffy, "Yellow Fever in Colonial Charleston," *S.C. Hist & Gen Mag.*, 1951, 52· 189-197.

[68] Dunn, *Sugar and Slaves*, pp. 195-196 especially fn 11

[69] Byron Edwards, *The History Civil and Commercial of the British Colonies in the West Indies. To which is added, An Historical Survey of the French Colony in the Island of St Domingo* (London B Crosby, 1798), p 299

[70] Duffy, *Epidemics*, pp 138-163

[71] Powell, *Bring Out Your Dead*, pp 14-15

[72] Deerr, *History of Sugar*, vol 2, p 461.

The history of sugar and yellow fever in Baltimore parallels that of Philadelphia. Its involvement with the West Indies sugar trade grew during the Revolution, after the capture of Charleston by the British in 1780. Following the war the trade continued to such an extent that "because of the cargoes of raw sugar to this port, a sugar refinery was built in Baltimore in 1784 and when commerce was re-established with Europe, this sugar supplemented the exports of Maryland."[75] A second refinery was in operation prior to 1795, and the trade continued to prosper. By 1810 seven refineries in the city and county were in operation, and produced 755,879 pounds of sugar. In 1823 the number of refineries had grown to eleven. Twelve years later production at the two largest plants was up to one thousand barrels a day.[76] The early processing plants were located in the wharf area known as Fell's Point.[77] It was here in 1793 that the first epidemic of yellow fever struck the city. The disease reappeared often over the next thirty years, the worst outbreaks coming in 1794, 1801, 1812, and 1819. In each instance mortality was highest in the area of "the Point."[78] Thus the worst epidemics ever to plague Baltimore were congruent in time and place with great expansion of the sugar refining industry.

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[73] Paul L. Vogt, *The Sugar Refining Industry in the United States. Its Development and Present Condition* Publications of the University of Pennsylvania Series in Political Economy and Public Law No 21 (Philadelphia University of Pennsylvania Press, 1908), p. 7

[74] Powell, *Bring Out Your Dead*, p. 281 The city's population at this time was about 55,000

[75] James E Hancock, "The Baltimore Clipper and the story of an old Baltimore shipbuilder," *Md Hist Mag.*, 1935, *30*: 143; Rhoda Dorsey. "The pattern of Baltimore commerce during the confederation period," *Md Hist Mag.*, 1967, *62*. 119-134

[76] Ralph J Robinson, "The story of sugar refining in Baltimore reaches back 150 years," *Balt Mag* , 1941, *34*· p 35.

[77] Thompson and Walker, *Baltimore and Fell's Point Directory, Containing the Names, Occupations and Places of Abode of the Inhabitants* (Baltimore. Pechin and Co , 1796), p. 29, J Thomas Scharf, *History of Baltimore City and County from the Earliest Period to the Present Day* (Philadelphia. Louis H. Everts, 1881). p 418, and Franklin R. Mullay, "The battle of Baltimore," *Md Hist Mag* , 1959, *54*· 65.

[78] Augustin, *History of Yellow Fever*, pp. 917, 920. Regarding the 1793 epidemic see Fillmore Norfleet, "Baltimore as seen by Moreau de Saint-Méry in 1794," *Md. Hist Mag.*, 1940, *35*. 229, and Frank F White Jr , "Edward Thorton looks at Baltimore," *Md. Hist Mag* , 1955, *50*: 70. For the 1797 epidemic, see "Extracts from the diary of William Faris of Annapolis, Maryland, January 4th, 1792-August 9th, 1804," *Md Hist. Mag* , 1933, *27*: 226. Apropos of the affinity of yellow fever for Baltimore's wharf area is a series of

A final example suggestive of a sugar/yellow fever association is found in the history of Louisiana. Early in the nineteenth century the center of the sugar industry in the United States began to shift away from the East Coast to the environs of New Orleans. Though cane had been grown there since the early eighteenth century, and was widely produced for local consumption by the 1780s, it became a profitable enterprise only about 1800.[79] Farmers quickly understood the potential of the industry and readily became sugar planters. Annual production grew from 2,500 tons in 1801 to 6,000 in 1812.[80] But the period of greatest expansion came several decades later. In 1830 there were 691 processing mills producing about 27,000 tons of sugar; by 1853 there were 1,550, producing 255,000 tons.[81] Not only had the number of milling works increased, so had the output efficiency per mill. It was during these years of enormous growth that yellow fever hit Louisiana, especially New Orleans, hard and often. The earliest epidemic dates back to 1796, and from then until the Civil War the virus appeared perennially.[82] The peak years of the disease were betwen 1837 and 1858 when severe epidemics struck eleven times. Each cost 500 to 2,500 lives, excepting the most severe ones in 1853 and 1858 which took 8,000 and 5,000 persons respectively.[83] Once again the worst yellow fever years followed closely upon an intense expansion in the sugar industry.

## *Summary*

The above ten examples document a chronological and geographical correlation between cane sugar enterprises and epidemic urban yellow fever. The cases of Guadeloupe, Barbados, St. Kitts, and the Yucatán demonstrate

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experiments done by Henry Carter in August 1903. He attempted to breed *A. aegypti* at various locales throughout the city. It is of interest that he was successful in raising "*aegypti* from cans exposed on the fruit wharves in Baltimore and from no other places in town." *Yellow Fever. An Epidemiological and Historical Study*, p. 43.

[79] J. Carlyle Sitterson, *Sugar Country. The Cane Sugar Industry in the South, 1753-1950* (Louisville University of Kentucky Press, 1953), pp 1-12

[80] *Ibid.*, p. 12, also, John Leander Bishop, *A History of American Manufactures from 1608-1860*, vol. 2 (Philadelphia: Edward Young, 1864), p. 185.

[81] Sitterson, *Sugar Country*, pp. 28-31

[82] Between 1796 and 1837 there were major epidemics in 1817, 1819, 1825 and 1833, see Jo Ann Carrigan, *The Saffron Scourge A History of Yellow Fever in Louisiana, 1796-1905* (Ph.D. diss , Louisiana State University, 1961), p 92.

[83] *Ibid* , pp. 93, 125. cf. John Duffy, *The Sword of Pestilence The New Orleans Yellow Fever Epidemic of 1853* (Baton Rouge· Louisiana State University Press, 1966), p 7

the presence of sugar cane growing and milling prior to the appearance of the first confirmed outbreaks of yellow fever in medical history. The epidemics at New York (1822) and St. Nazaire (1861) connect sugar ladened ships with the occurrence of yellow fever in climates rarely hospitable to dense infestation of *A. aegypti*. The cases of Cuba, Louisiana, and Philadelphia reveal similar epidemiological patterns over time. These three locales had some contact with yellow fever and even suffered major outbreaks in the nascent years of their respective sugar industries. But the onset of the disease as a major health hazard in each area was contemporaneous with a profound florescence of the sugar business. The final example, Baltimore, resembles that of Barbados, except that the nature of the sugar related industry was manufacturing rather than agriculture. Only after the establishment of a number of refineries and the increased importation of raw sugar did yellow fever plague Baltimore.

These cases strengthen the proposition that presence of sugar cane, processing equipment and procedures, and trading characteristics created conditions ecologically suitable to the survival and proliferation of *A. aegypti* in the Americas. This supports the hypothesis that the sugar industry from the seventeenth century through the nineteenth century had a marked impact on the epidemiology and history of yellow fever. If so, the "sugar connection" offers a new perspective for investigating the many unanswered questions still surrounding this enigmatic disease.[84]

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[84] A further research step to demonstrate the associative aspects of sugar and yellow fever would be a complete tabulation of all recorded yellow fever outbreaks with indications of the type of "sugar connections" associated with them

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# MEDIEVAL CANON LAW ON MEDICAL AND SURGICAL PRACTICE BY THE CLERGY*

DARREL W. AMUNDSEN

Were the medieval clergy ever forbidden to practise medicine or surgery? If an answer to this question is sought in secondary sources, the inquirer will soon find himself sinking into a quagmire of often conflicting and confusing statements usually alluding to but almost never citing, much less quoting, primary documents. The searcher may turn to a survey of medieval history and find the assertion that "at the beginning of the twelfth century, the medicine of the soul was declared incompatible with that of the body. The Church forbad monks to become professional doctors, a ban renewed in 1219 and extended at the same time to . . . cover the secular clergy."[1] Or he may look in a recent survey of the history of medicine and read, "By the late centuries of the Middle Ages, medicine . . . passed entirely into the hands of the laity: Pope Honorius III had in fact forbidden clergy to practise."[2] He may wish for more detailed information and turn to other medical history texts and find statements such as these:

> Notably under Pope Innocent II edicts were given forth (Council of Clermont 1130, of Rheims 1131, Lateran Council 1139) against medical practice by ecclesiastics.[3]
>
> . . . the practice, and in fact the study and teaching, of medicine, were forbidden to the higher clergy by the Council of Rheims 1131, the Lateran 1139, Montpellier 1162, Tours 1163, Paris 1212, and the second Lateran 1215. Finally the lower monks were also restricted, and especially by the Council of Le Mans 1247 all burning and cutting (surgery) were forbidden them, on the principle "The Church shuns bloodshed."[4]
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> At the Councils of Rheims (1131), Montpellier (1162), Tours (1163), Paris (1212), at those of the Lateran (1139 and 1215) and also by the decretals of the Popes Alexander

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* Read in abridged form under the title "Medicine, Surgery and the Medieval Church Councils—A Reinterpretation," at the 51st Annual Meeting of the American Association for the History of Medicine, Galveston, Texas, May 13, 1977.

[1] Robert S. Lopez, *The Birth of Europe* (New York: Evans-Lippincott, 1967), p. 373.

[2] Roberto Margotta, *The Story of Medicine* (New York: Golden Press, 1968), p. 146.

[3] Max Neuburger, *History of Medicine*, translated by E. Playfair, vol. II, pt 1 (London: Oxford University Press, 1925), p 40.

[4] J H. Baas, *Outlines of the History of Medicine and the Medical Profession*, translated by H. E Handerson (New York J. H. Vail, 1889), p. 253.

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III (1180) and Honorius III (1219) medical practice and especially surgery were forbidden to priests.[5]

> . . . we find the Church instituting that long series of edicts, which, in the first instance, were aimed not so much at medicine as at its malpractice by monks. These were the decrees of the councils of Clermont (1130), Rheims (1131), the second Lateran (1139), Montpellier (1162), Tours (1163), Paris (1212), the Fourth Lateran (1215) and Le Mans (1247) The general effect was, unfortunately, not only to stop the monks from practising, but to extend the special odium of these decrees to the whole medical profession.[6]

If the searcher stops at this point, he will have a fairly consistent list of Church councils, the mention of decrees of popes and the impression that the Church had forbidden the practice of both medicine and surgery to monks, and/or to priests or, perhaps, both to "the higher clergy" and to monks.

The inquirer may then turn to more specialized works on medical history and see a statement such as this:

> Ecclesiastic law is exemplified by the action of the Fourth Lateran Council which, in 993, prohibited the regular clergy from performing any surgical operation involving the shedding of blood, thus leaving such operation to seculars and clerks. However, the priest-physicians were not prohibited from practicing medicine until 1131, at which time the Seventh Lateran Council forbade the monks and regular cannons from pursuing the study of medicine. Later still, in 1163, the Council of Tours positively interdicted all surgical operations by the clergy.[7]

The dates and the names of some councils are now different and new types of clergy introduced unto the scene. If the searcher after truth has not yet become discouraged, he may look further into the secondary literature in the hope of some clarification, and read that "those in Orders were discouraged or forbidden by the Church from the practice of surgery, involving as it did the shedding of blood. In the practice of medicine the cleric had to compete with the experienced lay physician."[8] A new twist is now added: Clerics "in Orders" were forbidden to practise surgery, but clerics in general were allowed to pursue medicine.

The investigator may then wish to look further into the matter of the prohibition of the practice of surgery. He will find that "one of the main objections developed in the Middle Ages against anatomical studies was the maxim that 'the Church abhors the shedding of blood.' On this ground, in 1248, the Council of Le Mans forbade surgery to monks. Many other councils did the same . . . So deeply was this idea rooted in the mind of the universal Church that for over a thousand years surgery was considered dishonourable . . ."[9] Seeking to confirm that monks had been forbidden to practise surgery as of 1248, he may turn to a noted history of the relations of medicine and surgery and see that "the Rheims (A.D. 1125) and Lateran (A.D. 1139) Councils restricted the surgery of the clerical or, in other words, of the educated class, and no doubt feudal ideas did no less to abase its services; it was at Tours however that the sinister and perfidious 'ecclesia abhorret a sanguine' was first pronounced."[10]

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[5] Theodor Puschmann, *A History of Medical Education*, translated by E H Hare, 1891 (reprinted, New York: Hafner, 1966), p. 280.

[6] Fielding H Garrison, *An Introduction to the History of Medicine*, 4th ed (Philadelphia W B Saunders, 1929), p. 168

[7] Benjamin Spector, "The growth of medicine and the letter of the law," *Bull Hist Med* , 1952, *26* 512 f

[8] Charles Singer, "Thirteenth-century miniatures illustrating medical practice," *Proceedings of the Royal Society of Medicine* (Section of the History of Medicine), 1915-16, *9* 35.

If the conflict over the council responsible for the prohibition of surgery as well as the question of whether only monks or all clergy were affected stimulates the searcher to dig further into the problem, he will find that Tours of 1163 is more frequently mentioned than any other council in this regard and that the prohibition appears to have applied to all clerics. Additionally it may have been this opprobrious action of the Church that caused the great split between medicine and surgery in the late Middle Ages:

> Another practical obstacle to the advance of medicine at this time was the prohibition formulated at the Council of Tours against the shedding of blood by ecclesiastics. Since the doctor until well into the Renaissance was generally also a priest, he was henceforward prohibited from the practice of surgery. This resulted in him having to send his servant, a lay-brother, who would generally be the barber who kept his tonsure shaved, to attend to those of this clients who wished to be bled or have a tooth drawn. As a result of this he completely abandoned his status as a craftsman and merged himself into the authoritarian scholastic hierarchy. . . .[11]

If in doubt that this was how the barber-surgeons originated, the investigator might seek confirmation: "Surgery . . . was considered an unseemly practice for Monks. In 1163 the canon of Tours by its edict, Ecclesia abhorret a sanguine, had ordained that Monks should wholly abstain from manual operations of surgery; as a consequence this deserted profession fell into the hands of barbers and surgeons."[12]

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[9] Andrew Dickson White, *A History of the Warfare of Science with Theology in Christendom*, 1896 (reprinted, New York. Dover, 1960) vol. II, pp. 31 f.

[10] T C Allbutt, *The Historical Relations of Medicine and Surgery to the End of the Sixteenth Century* (London: Macmillan, 1905), p 21.

[11] W. S. C. Copeman, *Doctors and Disease in Tudor Times* (London: Dawson's of Pall Mall, 1960), p. 6. Copeman makes essentially the same assertion in "The evolution of anatomy and surgery under the Tudors" *Annals of the Royal College of Surgeons of England*, 1963, *32* (1): 11

[12] John F Eustace, "The Dublin Guild of Barber Surgeons," *Industrial Medicine and Surgery*, 1963, *32* 449.

Tours of 1163 remains the source; now however, it only affects monks again. That it caused the separation of medicine and surgery remains clear, at least until the investigator reads further:

> Innocent III . . . at the Lateran Council of 1215 . . . forbade all clerics in higher orders, that is, subdeacons, deacons and priests, to carry out surgical operations which involved cutting or burning. It was to be left to laymen. This decree has been constantly brought forward to show that from this point onwards surgery was thrown into the hands of the illiterate and that it led to the separation of medicine and surgery . . . . The separation of surgery from medicine was not an effect of ecclesiastical legislation: it was due not to the prohibition of medical practice to clerics. . . .[13]

Yet a different council and date and different clergy were affected; and the Church is no longer held responsible for separating medicine and surgery. In the hope that the author last cited said more on the subject elsewhere, the diligent inquirer will find him maintaining that "after the decree of the Lateran Council of 1215, when clerics in major Orders were forbidden to practise surgery and cautery, it became clear that physicians and surgeons could not be housed (at least in ecclesiastically dominated universities) under one and the same academical roof."[14] So, perhaps the Church was partially responsible after all.

If the investigator stops here, pondering on the degree of the Church's guilt in relegating surgery to barbers, or at least expelling it from the universities, will he at any rate have found an answer to the original question: were the medieval clergy ever forbidden to practise medicine or surgery? He certainly will have a variety of answers from which to choose. If he wishes to step back through the looking glass into the whirlwind of confusion of more secondary sources, the variations on the theme will continue to grow.[15] Nearly all that he reads on the subject will contain at least a small kernel of truth. Some will be essentially accurate in certain details. But he will probably look in vain for a study that will assure him that there has indeed been much inaccuracy and confusion on the question in secondary literature and that will then discuss systematically the relevant action of various Church councils and, more importantly, the regulations in medieval canon law governing the involvement of the clergy in medicine and surgery.

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[13] C H. Talbot, *Medicine in Medieval England* (London Oldbourne, 1967), pp. 51 ff.

[14] C H. Talbot, "Medical Education in the Middle Ages," in *The History of Medical Education*, edited by C D. O'Malley (Berkeley: University of California Press, 1970), p. 80.

[15] The more the researcher turns to secondary literature, the more he will become confused on the question The contradictions *between* the secondary sources quoted above or cited here are extreme, individually, many of these are correct in certain details but their authors apparently were not aware of the larger context of the development and relations of the various primary documents. See, for example, E Withington, "Roger Bacon and Medicine," in *Roger Bacon Essays*, edited by A. G Little (Oxford. Clarendon Press, 1914), p. 339; Stephen D'Irsay, "On the original connection between medicine and the university," *Johns Hopkins Hosp Bull* , 1930, *46*: 120 f.; Henry E Sigerist, "The physician's profession through the ages," originally published in 1933 and reprinted in *Henry E Sigerist on the History of Medicine*, edited by F Marti-Ibañez (New York: M D Publications, 1960), p. 9; David Riesman, *The Story of Medicine in the Middle Ages* (New York: Hoeber, 1936), pp. 197 f.; Edward F. Hartung, "Medical education in the twelfth century," *Medical Life*, 1934, *41* 21, G. G Coulton, *Medieval Panorama: The English Scene from Conquest to Reformation* (New York: Macmillan, 1938), p. 445; Gertrude M Engbring, "Saint Hildegard, twelfth century physician," *Bull. Hist Med.*, 1940, *8*: 778, Edward F. McLaughlin, "The guilds and medicine," *Annals Med. Hist.*, 1941, *3*: 389 f , Mary Niven

It is my purpose in this paper to present and discuss the primary documents in order to demonstrate that the prohibitions were not nearly as restrictive as they usually appear in the secondary literature and that such broad statements as "clerics were forbidden to practise medicine or surgery in the late Middle Ages" are generally inaccurate and consequently misleading.

The primary documents employed will be the canons of various Church councils, both regional and general, and the official collections of canon law. In regard to Church councils, it is important to be aware that the enactments of regional councils applied only to their immediate area of jurisdiction and that those of general councils applied to all Christendom. If one looks only to the provisions of Church councils in order to determine the law of the medieval Church, the result will be distorted. In addition to the canons of Church councils there are also papal rescripts or decretals, both of a universal and of a limited application, and the writings of the Church fathers. From the fifth century on, many attempts were made to form collections of regulations from these materials. Thus, although the collections contained canons of Church councils both general and local, they also included selections from letters of

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Alston, "The attitude of the Church towards dissection before 1500," *Bull. Hist. Med.*, 1944, *16*: 231; Pearl Kibre, "The Faculty of Medicine at Paris, charlatanism, and unlicensed medical practices in the later Middle Ages," *Bull Hist Med.*, 1953, *27*. 4; Monique Berry, *Les professions civiles dans le Décret de Gratien*, unpublished dissertation, Faculté de Droit de Paris, 1956, pp 96 ff., H M. Karn, "The history of medical training and licensure," *New Zealand Med. J*, 1957, *56* 328; Morris Fishbein, "The Barber Surgeons and the liberation of surgery," *J Intern College of Surgeons*, 1957, *27* 772, B L. Gordon, "Lay medicine during the early Middle Ages," *J Michigan State Med. Soc.*, 1958, *57*: 1006; E. A Hammond, "Physicians in Medieval English religious houses," *Bull. Hist. Med*, 1958, *33* 119 f., "Incomes of Medieval English doctors," *J Hist Med. & Allied Sci*, 1960, *15*: 167, and "The Westminster Abbey Infirmarers' Rolls as a source of medical history," *Bull Hist Med*, 1965, *39* 262; Vern L Bullough, "Status and Medieval medicine," *J Health & Human Behavior*, 1961, *2*: 206, n 17; *The Development of Medicine as a Profession: The Contribution of the Medieval University to Modern Medicine* (New York: Hafner, 1966), p. 109; Jacques Le Goff, "Métiers licites et métiers illicites dans l'Occident médiéval," *École des hautes études, annals, études historiques*, 1963, *5*. 42 f and 48, David Knowles, *The Monastic Order in England*, 2nd ed. (Cambridge Cambridge University Press, 1963), p. 518, George Clark, *A History of the Royal College of Physicians* (Oxford. Clarendon Press, 1964), vol 1, pp 17 and 22; Alessandro Simili, "Considerazioni storico-critiche sulla medicina monastica," *Minerva Medica*, 1974, *65*: 3106. Examples such as these could be increased substantially Special mention should be made of Paul Diepgen, *Die Theologie und der arztliche Stand* (Berlin-Grunwald: W Rothschild, 1922). Diepgen's discussion of the problem (pp. 16-19), although excellent for what it covers, is cursory in its selection of primary documents popes and other bishops and from patristic literature. These, the first canonical collections, were unofficial in nature, though they provided regulations for those dioceses in which they were used. Vast and disorganized, this array of conflicting sources provided no systematic treatment of the law of the Church. The most spectacular effort at bringing order out of this chaos was the *Decretum* of Gratian, completed about 1140. Its original title, *Concordia Discordantium Canonum* ("a harmony of disharmonious canons"), graphically portrays Gratian's purpose. The method he employed was first to state a specific problem, introduce authorities on both sides of the question, and then propose a solution that would bring the apparently conflicting positions into harmony or show the greater acceptability of one position. Although a monument of scholastic method, and of enduring significance, this dialectical treatise on the law of the Church was entirely of an unofficial nature in the sense that it was never promulgated as an "authentic" source of canon law. The *Decretum* was not, however, only of academic value, but was used by canonical courts in reaching decisions and presumably guided bishops and other ecclesiastical officials in their administration. After Gratian completed his *Decretum*, many new laws were promulgated by general Church councils and many decretals having a universal application were issued by popes. These were collected by a variety of efforts, most being unofficial, i.e., not having been promulgated as authentic collections. The first official collection was the *Compilatio Tertia*,[16] comprised of decretals issued during the first twelve years of Innocent III's pontificate, and promulgated in 1210. The second official collection was the *Compilatio Quinta*, promulgated in 1226 by Honorius III and comprised of decretals issued between 1216 and 1226. The most important collection having the force of law is the *Decretales* of Gregory IX, promulgated in 1234. This, the first official collection of a universal character not limited to decretals issued during restricted periods of time, rendered all previous collections, whether of an official or unofficial nature, obsolete[17] and is part of what became known as the *Corpus Iuris Canonici*.[18]

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[16] This is one of the *Quinque Compilationes Antiquae*, the five most famous decretal collections composed between c. 1187 and 1226. The actual sequence of composition was *prima*, *tertia*, *secunda*, *quarta*, and *quinta* These were edited by E Friedberg, 1882 (reprinted, Graz. Akademische Druck- und Verlagsanstalt, 1956)

[17] Excepting the *Decretum* of Gratian, which the official collections did not supplant but rather supplemented

[18] The title *Corpus Iuris Canonici* was first used in 1580 by Gregory XIII and refers to the *Decretum* of Gratian (c. 1140), the *Decretales* of Gregory IX (1234), the *Liber Sextus* of Boniface VIII (1298), the *Clementinae* (named after Clement V—1317), the *Extravagantes* of John XXII (1325) and the *Extravagantes Communes* (1500 and 1503) These were edited by E Friedberg, 2 vols., 1879 (reprinted, Graz: Akademische Druck- und Verlagsanstalt, 1959).

In discussing the regulations bearing first on the practice of medicine and then on the practice of surgery by clerics, we shall look at the relevant canons of regional and general Church councils dated before the promulgation of the *Decretales* of Gregory IX. We shall then turn to the *Decretales,* followed by a brief discussion of later decretal and conciliar developments.

Before pursuing the primary documents, some terminological distinctions should be made on the different classifications of clergy. On what may be called a horizontal plane, one must distinguish between regular and secular clergy. Some clerics lived under a rule and were called regular clergy, or religious. Monks and canons regular are in this category. The majority of clerics, however, were secular clergy who did not live under a rule but functioned within secular society. On a vertical plane, there were seven diffrent orders: four minor orders (porters, acolytes, exorcists and lectors) and three major orders (subdeacons, deacons and priests). The major orders are sometimes referred to as holy orders or sacred orders.

Canon 5 of the Council of Clermont[19] of 1130, canon 6 of the Council of Rheims[20] of 1131, both regional councils, and canon 9 of the Second Lateran Council[21] of 1139, a general council under Innocent II, are all identical, with minor textual variations. Their common rubric reads: "Monks and canons regular are not to study jurisprudence and medicine for the sake of temporal gain."

> An evil and detestable custom, we understand, has grown up in the form that monks and canons regular, after having received the habit and made profession, despite the rule of the holy masters Benedict and Augustine, study jurisprudence and medicine for the sake of temporal gain. Instead of devoting themselves to psalmody and hymns, they are led by the impulses of avarice to make themselves defenders of causes and, confiding in the support of a splendid voice, confuse by the variety of their statements what is just and unjust, right and wrong. The imperial constitutions, however, testify that it is absurd and disgraceful for clerics to seek to become experts in forensic disputations. We decree, therefore, in virtue of our Apostolic authority, that offenders of this kind be severely punished. Moreover, the care of souls being neglected and the purposes of their order being set aside, they promise health in return for detestable money and thus make themselves physicians of human bodies. Since an impure eye is the messenger of an impure heart, those things about which good people blush to speak, religion ought not to treat. Therefore, that the monastic order as well as the order of canons may be pleasing to God and be conserved inviolate in their holy purposes, we forbid in virtue of our Apostolic authority that this be done in the future.

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[19] *Sacrorum conciliorum nova et amplissima collectio*, edited by J. D. Mansi, et al, new edition (Florence and Venice, 1759-98), vol 21, col 438 f. (Hereafter cited as Mansi.)

[20] Mansi, vol 21, col. 459

[21] *Conciliorum oecumenicorum decreta,* edited by J Alberigo, et al., third edition (Bologna. Istituto per le Scienze Religiose, 1973), pp. 198 f. (Hereafter cited as Alberigo.)

Bishops, abbots, and priors consenting to such outrageous practice and not correcting it, shall be deprived of their honors and cut off from the Church.[22]

Although there is a degree of uneasiness that these religious who practise medicine would encounter things that they ought not to see, the central matter of concern in this canon is the avaricious motivation behind entering into the study and, by extension, the practice of medicine and secular law. This was a time of great social and economic change, a period marked by the rise of commercial and professional activities of a wide variety. The Church was engaged in an internal, ideological struggle regarding the spiritual aspects of commercial activity and the attitudes of canonists and the stipulations of Church councils evidence an attempt at adaptation designed to cope with socio-economic change. This is reflected in the fact that, while in the *Decretum* of Gratian all commercial profit seems to have been condemned, even his earliest commentators did not sustain his views on merchant's activities.[23] It is particularly during the twelfth century that there was a "shift in values within the traditional scheme of the cardinal vices."[24] The primacy of pride as the supreme vice was being displaced by the sin of avarice in theological and popular conceptions. This had its effect on the secular activities of the clergy. "In practice, clerics had engaged in secular pursuits from the time of the early Church onwards, and gradually, in theory, the canonists came to apply one criterion, i.e., of motive, whether such work was undertaken from a genuine need (*necessitas*) or selfish gain (*turpe lucrum*)."[25]

It is essential to note that the prohibitions contained in the canon common

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[22] I have used the translation made by R. J. Schroeder, *Disciplinary Decrees of the General Councils* (St Louis. Herder, 1957), pp. 201 f. The Latin text in Alberigo reads: Prava autem consuetudo, prout accepimus, et detestabilis inolevit, quoniam monachi et regulares canonici post susceptum habitum et professionem factam, spreta beatorum magistrorum Benedicti et Augustini regula, leges temporales et medicinam gratia lucri temporalis addiscunt. Avaritiae namque flammis accensi, se patronos causarum faciunt; et cum psalmodiae et hymnis vacare debeant, gloriosae vocis confisi munimine, allegationum suarum varietate iustum et iniustum, fas nefasque confundunt. Attestantur vero imperiales constitutiones, absurdum immo et opprobrium esse clericis, si peritos se velint disceptationum esse forensium. Huiusmodi temeratores graviter feriendos, apostolica auctoritate decernimus. Ipsi quoque, neglecta animarum cura, ordinis sui propositum nullatenus attendentes, pro detestanda pecunia sanitatem pollicentes, humanorum curatores se faciunt corporum. Cumque impudicus oculus impudici cordis sit nuntius, illa de quibus loqui erubescit honestas, non debet religio pertractare. Ut ergo ordo monasticus et canonicus Deo placens in sancto proposito inviolabiliter conservetur, ne hoc ulterius praesumatur, apostolica auctoritate interdicimus. Episcopi autem, abbates et priores tantae enormitati consentientes et non corrigentes, propriis honoribus spolientur et ab ecclesiae liminibus arceantur.

[23] The whole subject is very complex. A discussion of the economic doctrines of the canonists can be found in J. Gilchrist, *The Church and Economic Activity in the Middle Ages* (London: Macmillan, 1969), pp. 53 ff.

[24] L. K. Little, "Pride goes before avarice: social change and the vices in Latin Christendom," *Amer. Hist. Rev.*, 1971, *76*: 16.

[25] Gilchrist (supra n. 23), p. 55.

to the Councils of Clermont, Rheims and Lateran II extended only to monks and canons regular, those who were professed religious, who had withdrawn from secular affairs. The study and practice of medicine and secular law are here not considered appropriate for those who should be devoted exclusively to a religious life, if the motivation behind medical or legal activity was financial gain. The specific prohibition against the study and practice of medicine did not apply to a sizable segment of the clergy and is hardly a wholesale condemnation of the practice of medicine by clerics. Even though it is also a canon of a general Church council, its provisions were of little lasting significance. It simply was never included in any official collection of canon law. We shall find no further mention of any prohibition of the *practice* of medicine by any clerics in the canons of general Church councils or in decretals.

The charge is made with some frequency that it was necessary for the Church to repeat nearly incessantly its prohibition of the practice of medicine by clerics.[26] If all the Church councils that issued canons regarding clerical involvement in medicine or surgery were simply listed chronologically, it would then appear that these were frustrated repetitions of previous legislation that had been ignored by obstreperous and recalcitrant clergy. This is a theme to which we shall return later in this paper. For now, let us consider whether the charge holds true in the case of the first three relevant Church councils. In 1130 a *regional* council (Clermont) issued the canon discussed above. Its provisions would then have applied only to the specific region represented by the council. The same canon was, in the following year, adopted by another *regional* council (Rheims—1131), still having only regional force. Both Clermont and Rheims were regional but also papal councils, two of four regional councils over which Innocent II presided.[27] Eight years later the canon was adopted by a general council (Lateran II—1139), also under Innocent II, thus applying its provisions to all Christendom, at least until such time as it was superceded by other general Church legislation. In fact, one-fourth of the canons adopted by Lateran II simply repeat verbatim canons enacted by regional but papal councils presided over by Innocent II between 1130 and 1135. This is hardly a case of frantic repetition of ineffective legislation.

In 1162, at the Council of Montpellier, a regional and papal council presided over by Alexander III, a canon appears to have been enacted pertaining to the

[26] See, e.g., Puschmann (supra n. 5), pp. 280 f., Neuburger (supra n. 3), p. 40, Hammond (supra n. 15 [". . . Religious Houses"]), p. 120; Talbot (supra n. 13), p. 51.

[27] The other two were Piacenza (1132) and Pisa (1135).

study of medicine and secular law by monks, canons regular or other religious. The records of this council are not extant but in a later council in Montpellier (1195) reference was made to the earlier council:

> He [sc. Alexander III] also prohibited under every severity of ecclesiastical discipline any monk or canon regular or other religious to depart in order to study secular laws or physic. Moreover, in consequence of the decree promulgated under Lord Alexander in the council at Montpellier and of Tours in accord with this article, let them be punished canonically by the diocesan bishops.[28]

The canon enacted by Alexander III at Montpellier in 1162 is linked with and may be identical to that of Tours. In 1163, at the regional and papal Council of Tours, Alexander III enacted a canon that had an immediate and enduring impact:

> The ancient enemy's hatred does not strive with much effort to ruin the weak members of the Church, but he sends his hand against her more desirable members and attempts to trip up the elect, for the scripture says, "The elect are his food." Therefore he thinks that he has caused the fall of many when he has drawn away a more precious member of the Church by his cunning. And, of course, it is then that, by his usual habit, he entices away from their cloisters certain regulars for the study of law and for pondering medical concoctions[29] under the pretext of aiding the bodies of their ill brothers and performing ecclesiastical affairs more proficiently. On this account, so that, by the pursuit of knowledge, spiritual men be not entangled again in the affairs of this world and be not lacking in things of the soul, believing themselves thereby to provide for others in external matters, we decree with the consent of the present council, that no one at all is permitted to depart for the purpose of studying[30] medicine or secular law

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[28] Mansi, vol. 21, col 1160, and vol. 22, col. 670. Prohibuit praeterea sub omni severitate ecclesiasticae disciplinae, ne quis monachus, vel canonicus regularis, aut alius religiosus, ad saeculares leges vel physicam legendas accedat Alioquin iuxta decretum sub domino Alexandro in concilio apud Montempessulanum et Turonis super hoc articulo promulgatum, a dioecesanis episcopis canonice puniantur

[29] This is a strange periphrasis which could also be translated " . for weighing medical confections " See the comments of William D Sharpe, *Isidore of Seville The Medical Writings*, Philadelphia. Transactions of the American Philosophical Society (n s 54, 2), 1964, p 17, n 64

[30] "Legendas" here means "studying," not "teaching," as some scholars misread it Le Goff (supra n 15), for example, makes this general statement on the supposed action of the medieval Church against the practice of medicine by clerics "Il est clair qu'interdire une profession à un clerc dans une société religieuse et 'cléricale,' comme celle de l'Occident médiéval, n'est pas une recommandation pour cette profession, mais lui vaut au contraire un discrédit qui rejaillit sur les laics qui l'exercent" (p 42) Later (p 48), when discussing the provisions of the Council of Tours, he states that the prohibition against "clergy" in respect to medicine and law not only reduced the clergy's profit potential but also reduced their prestige His inconsistency can perhaps be explained by his mistranslation of the phrase "ad physicam legesve mundanas legendas" as "pour enseigner la medécine ou le droit civil " "Legendas" in the context of this canon surely does not mean teaching but, rather, learning The canon states that under the pretext of caring for the bodies of their ill brothers and handling ecclesiastical affairs more proficiently, certain regulars are lured out of their cloisters "ad legendas leges, et confectiones physicales ponderandas " For this to imply teaching rather than learning would be an utter *non sequitur*

> after having taken the vow and profession of religion in any religious place. If he does depart and does not return to his cloister within the space of two months, let him be avoided by all as if excommunicate, and in no case, if he dares to present a defense, should he be heard. When he has returned, however, let him always be the last of the brothers in the choir, in the chapter, at table, and in the rest, and let him lose hope of every advancement, unless, perhaps, by the pity of the Apostolic See.[31]

This canon, although not of a general council, quickly was included in a variety of twelfth-century decretal collections. It is, for example, found in these major collections: *Appendix Consilii Lateranensis,*[32] *Collectio Canonum Lipsiensis,*[33] *Collectio Canonum Bambergensis,*[34] *Collectio Canonum Casselana,*[35] and *Compilatio Prima,*[36] and also in several unedited English collections.[37] It is also the first document printed in the cartularies of the University of Paris.[38] Most importantly, it was included in the first officially promulgated major decretal collection, the *Decretales* of Gregory IX (1234).[39]

[31] Canon 8 Mansi, vol. 21, col 1179 Non magnopere antiqui hostis invidia infirma membra ecclesiae praecipitare laborat· sed manum mittit ad desiderabilia eius: et electos quoque nititur supplantare, dicente scriptura. Escae eius electae. Multorum si quidem causam operari se reputat, ubi pretiosius aliquod membrum ecclesiae fuerit aliqua calliditate detractum Inde nimirum est, quod se in angelum lucis more solito transfigurans, sub obtentu languentium fratrum consulendi corporibus, et ecclesiastica negotia fidelius pertractandi, regulares quosdam ad legendas leges, et confectiones physicales ponderandas, de claustris suis educit Unde ne sub occasione scientiae spirituales viri mundanis rursum actionibus involvantur, et in interioribus eo ipso deficiant, et quo se aliis putant in exterioribus providere: de praesentis concilii assensu, huic malo obviantes, statuimus, ut nullus omnino post votum religionis, post factam in aliquo religioso loco professionem, ad physicam, legesve mundanas legendas permittatur exire. Si vero exierit, et ad claustrum sum infra duorum mensium spatium non redierit: sicut excommunicatus ab omnibus evitetur, et in nulla causa, si patrocinium praestare praesumpserit, aut tentaverit, audiatur. Reversus autem ad claustrum, in choro, capitulo, mensa, et ceteris, ultimus fratrum semper existat: et nisi, ex misericordia forsan sedis apostolicae, totius spem promotionis amittat.

[32] 27, 2 Mansi, vol 22, col 373. The text there is identical except for the addition of a final sentence "Episcopi vero, abbates, priores, tantae enormitati consentientes, et non corrigentes, spolientur propriis honoribus, et ab ecclesiae liminibus arceantur." ("Bishops, abbots, and priors consenting to such outrageous practice and not correcting it, shall be deprived of their honors and cut off from the Church.") This is, of course, with minor variations, the concluding sentence of Clarmont 5, Rheims 6, and Second Lateran 9.

[33] 14, 2 Edited with the *Quinque* (supra n 16)

[34] 14, 2 *Die Canonessammlungen zwischen Gratian und Bernhard von Pavia,* edited by E Friedberg (Leipzig. Tauchnitz, 1897).

[35] 24, 2 *Ibid*

[36] Supra n. 16.

[37] *Belverensis*, "Durham Collection," and "Fountains Collection " On these see Charles Duggan, *Twelfth-Century Decretal Collections and their Importance in English History* (London. University of London Press, 1963), pp. 72 and 79 ff

[38] *Chartularium Universitatis Parisiensis sub Auspicus Consilii Generalis Facultatum Parisiensium,* edited by H. Denifle and A Chatelain, vol. I, 1899 (reprinted, Brussels· Culture et Civilisation, 1964).

[39] 3, 50, 3. The text is essentially the same as that given above (n. 31), with some omissions and verbal alterations· Non magnopere antiqui hostus invidia [ ] inde nimirum est, quod in angelum lucis se more solito

The section of the *Decretales* in which this canon of the Council of Tours is placed is entitled "Clerics and monks are not to involve themselves in secular affairs." The major emphasis of this section is that some acts are forbidden to all Christians, some are permitted to all; others, although permitted to laymen, are prohibited to all clergy; and others still are forbidden only to certain groups of clergy. The canon under consideration falls into the last category. The study of medicine and secular law is here prohibited for the same groups as were included in the canon from Lateran II. Canons regular and monks were religious rather than secular clergy. They lived together under a rule and were bound by a vow of stability, an obligation of residence. It is the leaving of their cloisters for secular, rather than purely religious purposes, that was condemned, not the study of medicine or secular law. That these two disciplines are again mentioned together is interesting: the canon lawyers themselves were required to receive extensive training in secular law before advancing to canon law. The study of secular law obviously was not being condemned *eo ipso*. Nor was the study of medicine. Significantly the canon did not forbid all members of the clergy to study medicine or secular law. The majority of clerics would not have been affected. Nor did it prohibit the *practice* of these arts even by monks and canons regular.

In 1213[40] the regional Council of Paris adopted the following canon:

> Since certain regulars (to use the words of the Lateran Council), under the pretext of caring for the bodies of their sick brothers and conducting ecclesiastical affairs more proficiently, do not fear to leave their cloisters for the purpose of studying secular law and pondering medical concoctions in order to attend to jurisprudence and medicine, and are *eo ipso* deficient in things of the soul, although they believe themselves to be providing for others in exterior matters: following in the footsteps of that council, we order that, unless they return to their cloisters within the space of two months, even if they had the permission of their abbot (since he was not allowed to give such permission), let them be excommunicate and avoided by all, and in no case, if they wish to present a defense, are they to be heard.[41]

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transfigurans, sub obtentu languentium fratrum consulendi corporibus et ecclesiastica negotia fidelius pertractandi, regulares quosdam ad legendas leges et confectiones physicales ponderandas de claustris suis educit Unde, ne [. . ] occasione scientiae spirituales viri mundanis rursus actionibus involvantur, [ ] statuimus, ut nulli omnino post votum religionis, et post factam in aliquo loco religioso professionem ad physicam legesve mundanas legendas permittantur exire. Si vero exierint, et ad claustrum suum infra duorum mensium spatium non redierint, sicut excommunicati ab omnibus evitentur, et in nulla causa, si patrocinium praestare voluerint, audiantur. Reversi autem in choro, capitulo, mensa et ceteris ultimi fratrum [. . ] exsistant, et, nisi forte ex misericordia sedis apostolicae, totius spem promotionis amittant

[40] Mansi gives 1212

[41] Canon 20. Mansi, vol 22, col 831 Cum quidam regularium, ut verbis concilii Lateranensis utamur, sub obtentu languentium fratrum consulendi corporibus, et ecclesiastica negotia fidelius pertractandi, ad leges mundanas legendas, et confectiones physicas ponderandas, ut jurisprudentiae et medicinae dent operam, de

Note that, although the council cited is the Lateran, the canon that is paraphrased is that of Tours. This may be simply a *lapsus linguae* but it is equally possible that the reference is to Lateran III of 1179. (It obviously is not to Lateran II.) Reference was made above[42] to the inclusion of Tours canon 8 in *Appendix Consilii Lateranensis,* a collection of canons that is found associated with the canons of Lateran III. Canon 8 of the Council of Tours on that account may have been mistaken for a canon of Lateran III which would have given it additional weight, as Lateran III was a general council while Tours was a regional, although papal, council.[43]

In 1219 Honorius III issued a lengthy rescript, usually called the *Super specula* (or *Super speculam*), which was designed to promote the study of theology.[44] The steps taken in this rescript were (1) to provide that teachers of theology (as long as they were teaching) and theological students (for a period up to five years) could receive the income of their prebends or benefices; (2) to repeat and extend the provisions of canon 8 of the Council of Tours; and (3) to forbid the teaching and study of civil (i.e., Roman) law at the University of Paris. The sections of the *Super specula* dealing with the above three matters were extracted and included in the *Collectio Quinta,*[45] the second official decretal collection, which was promulgated in 1226 by Honorius III. They were also included in the *Decretales* of Gregory IX (1234).[46] It is the second of the three actions taken by Honorius III in the *Super specula* that pertains directly to our subject. The text, as it appears in the *Decretales* of Gregory IX, reads:

> Our predecessor, Alexander [III] once enacted in the Council of Tours in regard to religious persons leaving their cloisters to attend lectures on laws or physic,[47] that unless they return to their cloisters within the space of two months, they are to be

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claustris suis exire non formident, et ex eo ipso deficiant in interioribus, quando se putant aliis in exterioribus providere· ipsius concilii vestigiis inhaerentes, praecipimus, ut nisi infra duorum mensium spatium ad claustrum suum redierint, non obstante abbatis sui licentia, quam dare non potuit, sint excommunicati, et ab omnibus evitentur, et in nulla causa, si patrocinium praestare voluerint, admittantur. This canon is included as document 19 in the cartularies of the University of Paris (supra n 38)

[42] N 32

[43] Tours is sometimes referred to as a general council (although it was not), even by leading canon scholars, e g , by C. N L. Brooke, "Canons of English church councils in the early decretal collections," *Traditio,* 1957 *13*· 471

[44] The complete text is printed as document 32 in the cartularies of the University of Paris (supra n 38) It is summarized as papal rescript 6165 of Honorius III in *Regesta Pontificum Romanorum inde ab anno post Christum natum MCXCVII ad annum MCCCIV,* edited by Augustus Potthast, 1874 (reprinted, Graz Akademische Druck- und Verlagsanstalt, 1957).

[45] Supra n 16. These are, respectively, 5, 2, 1, 3, 27, 1, and 5, 12, 3

[46] As 5, 5, 5; 3, 50, 10; and 5, 33, 28, respectively.

[47] " . ad audiendum leges vel physicam "

> avoided by all as if excommunicate, and in no case, if they wish to present a defense, are they to be heard. When they have returned, however, they are to be the last of the brothers in choir, at table, in the chapters and in the rest, and they are to lose hope of every advancement, unless, perhaps, by the pity of the Apostolic See. Indeed, because some of these, on account of the varied opinions of certain men, adopt any kind of excuse, we, wishing that they henceforth incur the sentence of excommunication *ipso facto*, as a warning strictly command that both by their diocesan [bishops] and by their chapters, and also by the other bishops in whose dioceses they study to this effect, such men be publicly announced as excommunicate [and] liable to the above stated penalties. Inasmuch as we desire the study of theology to be increased, so that, just as when the space for one's tent has been broadened one also lengthens its cords, the Catholic faith might be girded about by an impregnable wall of warriors, with which she will have the power to fend off those scaling up from outside: we wish and order that this [prohibition] be extended to archdeacons, deacons, rural deans, priors, cantors and other clerics having benefices, and also to priests, unless they desist from these things within the prescribed period, and that [this order] be firmly enforced, with the right of appeal having been rescinded.[48]

It is important to consider the tone and the intent of the entire rescript of which the section just quoted above is only a part. Honorius III's purpose in issuing the *Super specula* was to encourage theological study, not to condemn any activity. It could easily be argued that this rescript furnishes solid evidence of his open hostility to the study of civil law. Not only did Honorius extend the prohibition of its study (as well as the study of medicine) to new groups of clergy but he also forbade it being taught or studied in the city of Paris. Hastings Rashdall comments that the teaching and study of civil law in Paris was forbidden "not (as is sometimes represented) in a narrow spirit of hostility to legal or to secular studies in general, but because it threatened to extinguish the study of theology in the one great theological school of Europe."[49] Indeed, the papacy was highly supportive of the faculty of civil law at the greatest center for legal studies, Bologna. On the University of the Roman Curia, Rashdall writes:

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[48] 3, 50, 10 Contra religiosas personas, de claustris exeuntes ad audiendum leges vel physicam, [ ] Alexander praedecessor noster olim statuit in concilio Turonensi, ut, nisi infra duorum mensium spatium ad claustrum redierint, sicut excommunicati ab omnibus evitentur, et in nulla causa, si patrocinium praestare voluerint, audiantur. Reversi autem in choro, mensa, capitulo et ceteris ultimi fratrum exsistant, et, nisi forte ex misericordia sedis apostolicae, totius spem promotionis amittant. Verum, quia nonnulli ex talibus propter quorundam opiniones diversas excusationis aliquid assumebant, nos, volentes, ut [ .] de cetero ipso facto sententiam excommunicationis incurrant, districte praecipiendo mandamus, quatenus tam a dioecesanis et capitulis ipsorum, quam [. .] a ceteris episcopis, in quorum dioecesibus in huiusmodi student, tales [ .] excommunicati [. ] praedictis poenis obnoxii publice nuncientur. Quia vero theologiae studium cupimus ampliari, ut dilatato sui tentorii loco et funiculos suos faciat longiores, ut sit fides catholica circumcincta muro inexpugnabili bellatorum, quibus resistere valeat adscendentibus ex adverso. ad archidiaconos, decanos, plebanos, praepositos, cantores et alios clericos personatus habentes, nec non [. ] presbyteros, nisi ab his infra spatium praescriptum destiterint, hoc extendi volumus et mandamus, et appellatione postposita firmiter observari

[49] *The Universities of Europe in the Middle Ages*, revised edition, edited by F M Powicke and A. B Emden

> It is particularly worthy of notice that the civil law received especial encouragement in a school which was the absolute creature of the holy see, priests habitually receiving dispensations to enable them to study it in spite of the prohibition of Honorius III—a sufficient refutation of the idea that the supreme pontiffs were systematically hostile to that study. Even Honorius III was no enemy to the civil law as such, though anxious to promote the study of theology and canon law by the priesthood and the religious orders. But the study of civil law soon became essential to the study of canon law; and the Popes themselves were usually lawyers rather than theologians.[50]

Nor is there any evidence that the papacy opposed the teaching and study of medicine at the universities under ecclesiastical control.[51]

With the issuance of the *Super specula* the prohibition of the study of medicine extended to clerics whose major functions were spiritual and to those who possessed ecclesiastical benefices to which they owed their primary responsibilities. Extensive as the groups now forbidden to study medicine and secular law may appear at first glance, the prohibition still did not apply to a vast number of clergy: no mention is made of those in minor orders or of the major order of subdeacons. Nor is there any prohibition of the practice of medicine even for those who are forbidden to leave their ecclesiastical duties for the purpose of studying it.

In 1298 Boniface VIII promulgated the *Liber Sextus*, designed to supplement the *Decretales* of Gregory IX.[52] The *Liber Sextus* contains a decree that reduces somewhat the restrictions imposed by the *Super specula*:

> Following in the footsteps of Pope Clement IV,[53] our predecessor of pious memory, we declare that the statute of our predecessor, Pope Honorius III, of blessed memory (which forbade the study of laws and physic to deacons, archdeacons, rural deans, priors, cantors, and other clerics having benefices) does not extend, by the pro-

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(London: Oxford University Press, 1936), vol I, p. 322. On the *Super specula*, see Walter Ullmann, "Honorius III and the Prohibition of Legal Studies," *Juridical Review*, 1948, *60*: 177 ff., and Stephan Kuttner, "Papst Honorius III und das Studium des Zivilrechts," *Festschrift fur Martin Wolff*, edited by Ernst von Caemmerer et al (Tübingen: Mohr, 1952), pp. 79 ff

[50] *Ibid.*, vol II, p 29

[51] Rather there is ample evidence of papal support For example, the papal confirmation of the statutes of the medical faculty of the University of Montpellier (1239) lauds the study of medicine and contains such statements as, e.g , "medicine shines forth among the liberal arts." See Luke Demaitre, "Theory and practice in medical education at the University of Montpellier in the thirteenth and fourteenth centuries," *J. Hist. Med & Allied Sci* , 1975, *30*. 107.

[52] Supra n. 18 The *Decretales* was divided into five books, thus the title of Boniface VIII's supplement, the *Liber Sextus*, i.e , the "sixth book.'

[53] The legislation of Clement IV referred to here does not seem to be extant. See, however, n 2 to the text of the *Super specula* in the cartularies of the University of Paris (supra n. 38) vol I, p. 93.

nouncement of this present constitution in the interest of parochial churches, to those who are known to possess churches of this kind, unless the same churches are prebends having chapels under them in which are instituted permanent clergy who are not permitted to be removed from these except for reasonable cause.[54]

Another decree in the *Liber Sextus* forbids religious to depart for the purpose of any study, then provides means whereby they may obtain permission to do so, and finally extends punishment to professors and teachers who knowingly take on as students religious who have laid aside their habit without permission:

> In order that a dangerous pretext for wandering be removed from religious, we more strictly order that henceforth no one, who has silently or verbally taken vows of a religious life, audaciously abandon the habit of his religious life in schools or elsewhere, or depart to any schools of learning whatsoever, unless permission is first given to him to depart for study by his prelate with the consent of his religious house or the majority of its members. If anyone, however, is a rash violator of these, let him incur the sentence of excommunication *ipso facto*. Also professors or teachers who knowingly presume to teach religious, who have laid aside their habit, attending lectures on laws or physic, or to retain them in their schools, shall have been implicated in a similar sentence *eo ipso*.[55]

These two statutes in the *Liber Sextus* appear to be the last pronouncements of medieval canon law addressed to the study of medicine and secular law by clerics. Thus in the officially promulgated collections of universal canon law of the Middle Ages there are four relevant statutes: The first, issued originally in 1163, forbade monks and canons regular to leave their religious houses for the purpose of studying medicine or secular law and imposed stiff penalties on those who departed for that purpose and were absent for more than two months. The second, originally issued in 1219, extended the same prohibitions and penalties to clergy having specific spiritual functions and those possessing benefices. The third, promulgated in the *Liber Sextus* of 1298, removed the prohibition from clergy having parochial churches, and the fourth, also in the *Liber Sextus,* addressed only to religious, permits their departure for study if permission is first obtained from their prelates with the consent of the majority of members of their religious house.

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[54] 3, 24, 1 Statutum felicis recordationis Honorii Papae III praedecessoris nostri, quod decanis, archidiaconis, praepositis, plebanis, cantoribus et aliis clericis, habentibus personatus, audire leges vel physicam interdicit, ad eos, qui parochiales ecclesias obtinere noscuntur, piae memoriae Clementis Papae IV praedecessoris nostri vestigiis inhaerentes, declaramus praesentis constitutionis oraculo ratione ecclesiarum huiusmodi non extendi, nisi eaedem ecclesiae fuerint plebaniae sub se capellas habentes, in quibus instituantur clerici perpetui, nequeuntes ab ipsis absque causa rationabili amoveri

[55] 3, 24, 2 Ut periculosa religiosis evagandi materia subtrahatur, districtius inhibemus, ne de cetero aliquis, quamcunque religionem tacite vel expresse professus, in scholis vel alibi temere habitum religionis suae dimittat, nec accedat ad quaevis studia literarum, nisi a suo praelato cum consilio sui conventus vel maioris partis eiusdem sibi eundi ad studium licentia primitus sit concessa Si quis autem horum temerarius violator exstiterit· excommunicationis incurrat sententiam ipso facto Doctores quoque sive magistri, qui religiosos, habitu suo dimisso, leges vel physicam audientes scienter docere, aut in scholis suis praesumpserint retinere, simili eo ipso sint sententia innodati

These prohibitions of the study of medicine that affected certain groups of clergy would not have prevented them from practising the art. It could be objected that if they were not permitted to leave their places of residence in order to study medicine, a prohibition of their practising it would hardly be necessary. There is nothing in the actual legislation to suggest that the clergy affected by the prohibition of being absent for extended study were not allowed to study medicine within the confines of their religious houses. There is a manuscript written by a religious in the late thirteenth century designed to instruct other religious in medicine so that they could treat the poor gratuitously since the poor "are abandoned by the ordinary physicians and surgeons."[56] Clergy unaffected by the prohibition of formal study of medicine, e.g., those in minor orders and subdeacons, unless they were monks or canons regular, could have obtained formal training in medicine without violating canonical injunctions. Those affected by the prohibition could seek and sometimes probably obtain permission to study.[57]

Being a practising physician was not an impediment to clerical advancement. A famous example is Peter of Spain who was dean of the Church of Lisbon, then physician to Pope Gregory X, next archbishop of Braga, then cardinal of Tusculum and, in 1276, was elected pope under the name of John XXI. But the practice of medicine did present certain unique problems for the clergy, on which a rescript was written by Clement III. Between 1187 and 1191, Clement had received an inquiry from a *canonicus* concerned with this matter. Clement's reply reads:

> You have brought to our attention that, since you are skilled in the art of physic, you have diligently treated many by the medical tradition of this art, although frequently it had happened to the contrary and those, to whom you thought you were applying a remedy, after taking the medicine, incurred the danger of death. But, because you desire to be advanced to sacred orders, you wished to consult us on this. We reply to you briefly that, if your conscience troubles you on account of those things said above, in our opinion you should not advance to major orders.

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[56] Talbot (supra n 13), p 96

[57] The whole question of privilege deserves much study and, until such is done on the question of medical studies, it is not safe to make any broad statements on the degree to which such dispensations were given Rashdall (supra n 49, vol I, p. 235, n. 4) writes that "the prohibition of the study [of medicine] to the priests monks, and beneficed clergy . . shows its growing importance, but did little to check the practice denounced, since dispensations were freely granted " He says the same about the study of secular law (vol. I, p. 260)

This papal rescript, which was included in the *Compilatio Secunda*[58] and later in the *Decretales* of Gregory IX,[59] clearly indicates that the major concern was whether the cleric as a physician had done anything that would prevent his fulfilling his spiritual functions with a clear conscience. If a cleric was responsible, even in a most peripheral way, for anyone's death, he incurred a canonical irregularity. If he was in major orders, this irregularity prevented him from fulfilling his most important ecclesiastical functions. If he was in minor orders, it was an impediment to his advancement to major orders.

The risk of incurring responsibility for the death of a patient in surgical practice was even much greater than in medicine. In the latter the treatment was viewed as primarily passive, as maltreatment was less readily suspected and much more difficult to prove. On the other hand, the practice of surgery is active and the death of a patient much more easily credited to the practitioner. There is an interesting rescript written by Innocent III in 1212, included in *Compilatio Quarta*[60] and later in the *Decretales* of Gregory IX,[61] that illustrates the problem as seen from an ecclesiastical vantage point:

> Your brotherhood said that we should be consulted. You asked *to be advised by the Apostolic See* what must be decided concerning a certain monk who, believing that he could cure a certain woman of a tumor of the throat, acting as a surgeon, opened the tumor with a knife. When the tumor had healed somewhat, he ordered the woman not to expose herself to the wind at all lest the wind, stealing into the incision in her throat, bring about her death. But the woman, defying his order, rashly exposed herself to the wind while gathering crops, and thus much blood flowed out through the incision in her throat, and the woman died. *She, nevertheless, confessed that she was responsible because she had exposed herself to the wind.* The question is whether this monk, since he is also a priest, may lawfully exercise his priestly office. We therefore reply to your brotherhood that, although the monk himself was very much at fault for usurping an alien function which very little suited him, nevertheless, if he did it from piety and not from cupidity, and was expert in the exercise of surgery and was zealous to employ every diligence which he ought to have done, he must not be condemned for that which happened through the fault of the woman against his advice. Then, with no penance being required, he may be permitted to celebrate divine service. Otherwise, the fulfilling of the sacerdotal office must be strictly forbidden him.[62]

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[58] 1, 8, 2 (supra n. 16)

[59] 1, 14, 7 Ad aures nostras te significante pervenit, quod, quum in arte physica eruditus sis, pluribus iuxta ipsius artis traditionem exhibuisti cum diligentia medicinam, licet plures in contrarium successerit, et quibus putabas adhibere medelam, medicinis perceptis mortis periculum incurrerunt. Verum quia, ad sacros ordines desideras promoveri, super eo nos consulere voluisti. Tibi breviter respondemus, quod, si super praemissis conscientia tua te remordeat, ad maiores ordines de nostro consilio non ascendas

[60] 5, 6, 3 (supra n. 16)

[61] 5, 12, 19.

[62] Sections italicized in the translation and in the text given below, although part of the original rescript, are

Although Innocent III was uneasy about the monk in question exercising the art of surgery and described him as having usurped an alien function that little suited him, nevertheless, it was the fact that this monk was also a priest that precipitated papal response. It is clear that if this monk/priest had been responsible in any way for the death of the woman in this case, he would have been prevented from fulfilling his spiritual functions as a priest (not as a monk). It is this concern that motivated action by a general Church council just three years later. In 1215, the Fourth Lateran Council, under Innocent III, promulgated what is one of the most significant pieces of ecclesiastical legislation from the Middle Ages, a canon especially noted for its immediate effect on medieval secular law:

> No cleric may pronounce a sentence of death, or execute such a sentence, or be present at its execution. If anyone in consequence of this prohibition should presume to inflict damage on churches or injury on ecclesiastical persons, let him be restrained by ecclesiastical censure. Nor may any cleric write or dictate letters destined for the execution of such a sentence. Wherefore, in the chanceries of the princes let this matter be committed to laymen and not to clerics. Neither may a cleric act as judge in the case of the Rottarii,[63] archers, or other men of this kind devoted to the shedding of blood. No subdeacon, deacon, or priest shall practice that part[64] of surgery involving burning and cutting. Neither shall anyone in judicial tests or ordeals by hot or cold water or hot iron bestow any blessing; the earlier prohibitions in regard to dueling remain in force.[65]

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not included in the *Decretales*. Tua nos duxit fraternitas consulendos. Quaesivisti *per sedem apostolicam explicari*, quod sit de quodam monacho sentiendum, qui, credens, se quandam mulierem a gutturis tumore curare, ut chirurgicus cum ferro tumorem illum aperuit, et, quum tumor aliquantulum resedisset, ipse mulieri praecepit, ne se vento exponeret ullo modo, ne forte ventus, subintrans gutturis apertionem, sibi causam mortis inferret; sed mulier, eius mandato contempto, dum messes colligeret, vento se exposuit incaute, et sic per apertionem gutturis sanguis multus effluxit, et mulier diem ultimum sic finivit, *quae tamen confessa est, quod, quia vento exposuit semetipsam, sibi dederat causam mortis*, utrum videlicet, quum praedictus monachus sit sacerdos, liceat ei sacerdotale officium exercere. Nos igitur fraternitati tuae respondemus, quod, licet ipse monachus multum deliquerit alienum officium usurpando, quod sibi minime congruebat, si tamen causa pietatis, et non cupiditatis id egerit, et peritus erat in exercitio chirurgiae, omnemque studuit, quam debuit, diligentiam adhibere, non est ex eo, quod per culpam mulieris contra consilium eius accidit, adeo reprobandus, quod non post satisfactionem condignam cum eo misericorditer agi possit, ut divina valeat celebrare; alioquin interdicenda est ei sacerdotalis ordinis exsecutio de rigore.

[63] Schroeder (supra n. 22, p. 258, n. 16) explains: "Also Ruptarii and Ruptuarii, bands of robbers and plunderers, drawn chiefly from the peasant class."

[64] Some texts read *artem*, others *partem*. It should be noted that medieval surgery included within its functional purview not only cautery and cutting but also the external, medicinal treatment of wounds and sores, the setting of fractures, the treatment of dislocations, etc. Thus I incline toward reading *partem*. Clark (supra n. 15, vol. I, p. 17, n. 3) writes: "The reading *artem* for *partem* would extend the prohibition to all surgery, but does not seem so satisfactory from the point of view of language."

[65] C. 18. I have used Schroeder's translation (supra n. 22, p. 258). The text given below is that of Alberigo (p. 244), although I have substituted *partem* for *artem*. Sententiam sanguinis nullus clericus dictet aut proferat, sed nec sanguinis vindictam exerceat aut ubi exercetur intersit. Si quis autem huiusmodi occasione statuti

This canon was included in *Compilatio Quarta*[66] and later in the *Decretales* of Gregory IX.[67]

It is reasonable to ask why, in all these prohibitions except that against surgery, all clerics are included, while only in the case of surgery just subdeacons, deacons and priests are mentioned. We should indeed hesitate to believe that the Church held all the activities mentioned in this canon at the same level. If that were the case, there would have been no reason not to have included all clerics in the prohibition of the practice of surgery. But the Church obviously did not view the practice of surgery in the same light as executions, trial by ordeal and similar activities in which no cleric was to participate. Surgery, on the other hand, was forbidden only to those clerics who were in major orders, who would have been most severely affected by incurring a canonical irregularity. It is frequently claimed that the Church forbade the practice of surgery to all clerics on the ground that *Ecclesia abhorret a sanguine,* that is, "The Church abhors the shedding of blood." This maxim is sometimes attributed to canon 18 of Lateran IV although usually to the Council of Tours, which took no action on the question of the practice of surgery by clerics. Talbot's comments on the maxim bear quoting:

> It is a literary ghost. It owes its existence to Quesnay, the uncritical historian of the Faculty of Surgeons at Paris, who in 1774, citing a passage from Pasquier's *Recherches de la France* ("et comme l'église n'abhorre rien tant que le sang") translated it into Latin and put it into italics. No earlier source for this sentence can be found. Quesnay himself quoted a register from the archives of the Surgeons of Paris, in which it was stated that "at the time of Boniface VIII (1294-1303) and Clement V (1305-14) a decree was put forth at Avignon and confirmed by the council of Philip le Bel that surgery was separated from medicine." No such decree can be found in the register of Boniface VIII, whilst among the ten thousand documents contained in the register of Clement V only one refers to medicine, and that concerns itself with studies at Montpellier.[68]

Leaving the infamous maxim aside, it remains that many scholars cite canon 18 of the Fourth Lateran Council as the source of the Church's prohibi-

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ecclesiis vel personis ecclesiasticis aliquod praesumpserit inferre dispendium, per censuram ecclesiasticam compescatur, nec quisquam clericus literas scribat aut dictet pro vindicta sanguinis destinandas, unde in curiis principum haec solicitudo non clericis sed laicis committatur Nullus quoque clericus rottariis aut balistariis aut huiusmodi viris sanguinum praeponatur, nec illam chirurgiae artem subdiaconus, diaconus vel sacerdos exerceant, quae ad ustionem vel incisionem inducit, nec quisquam purgationi aquae ferventis vel frigidae seu ferri candentis ritum cuiuslibet benedictionis aut consecrationis impendat, salvis nihilominus prohibitionibus de monomachiis sive duellis antea promulgatis

[66] 3, 19, 2 (supra n 16)

[67] 3, 50,9

[68] Supra n 13, p 55 I am indebted both to Prof E. A. Hammond and Prof Ruth Friedlander for bringing to my attention Talbot's treatment of this maxim

tion of the practice of surgery to *all clerics*. It is easy to see how such a misinterpretation is so frequently repeated. Even in a history of the general Church councils this summary of the canon in question is found: "Clerics are not to have any part in trials that involve the punishment of death. They are forbidden all military employment. They are not to act as surgeons. They are not to bless ordeals."[69] Such an error may not seem significant until one considers the fact that, since the prohibition applied only to those clerics in major orders, the majority of clerics were not affected.

Mention was made above of the charge that the Church found it necessary to keep repeating its prohibitions of the involvement of clerics in medicine and surgery. Indeed, if the references to the prohibition regarding surgery were simply listed, as they often are, this would appear to hold true. The provision in canon 18 of the Fourth Lateran Council is in fact repeated at least four times in the next 85 years: In the Statutes of Le Mans (1247) appears " . . . a subdeacon or a deacon or a priest shall not practise any part of surgery involving burning or cutting. . . ."[70] The Diocesan Synod of Nîmes (1284) includes, "Also no cleric established in sacred orders[71] shall practise any art of surgery including burning or cutting. . . ."[72] In the Diocesan Synod of Würzburg (1298) there is found a somewhat different version: "No cleric, deacon, subdeacon, or priest,[73] shall practise the surgical art or be present when it is practised. . . ."[74] The Diocesan Synod of Bayeux (1300) includes, " . . . nor shall a subdeacon, a deacon, a priest practise any part of surgery including burning or cutting. . . ."[75]

When these statements are isolated from the context in which they appear, are neatly lined up in a row with the similar prohibition from canon 18 of Lateran IV at the head of the procession, weight surely seems to be lent to the argument that it was indeed necessary for the Church incessantly to repeat this regulation which must have been so flagrantly ignored. But it is dangerously misleading to snatch these prohibitions from their surroundings for such purposes. All four of these examples given above are found in the context of lengthy regulations governing the conduct and responsibilities of clergy. These regulations (usually given under the rubric *De vita et honestate clericorum*, i.e., "On the life and character of clerics") range from general exhortations on moral probity to specific injunctions on types of activities expected of or denied to all clerics or to individual groups of clergy. The prohibition of the practice of surgery to subdeacons, deacons and priests occurs in these four cases as simply a part of one sentence within broad, local summaries of general Church legislation covering the spectrum of clerical morals and conduct. The prohibition of surgery was not singled out for special attention in these extensive guides; nor should it be by historians. If the repetition of this prohibition was prompted by its being flagrantly disregarded by those to whom it applied, then we must also assume that the repetitions of the entire range of regulations governing the conduct of clergy in which the prohibition of surgery appears were prompted by the same considerations.

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[69] Philip Hughes, *The Church in Crisis: A History of the General Councils, 325-1870,* 1961, (reprinted, Garden City Doubleday—Image Books, 1964, p 241)

[70] Mansi, vol. 23, col 756 nec ullam partem chirurgiae subdiaconus vel diaconus vel sacerdos exerceat, quae adustionem vel incisionem inducit . .

[71] I e., subdeacons, deacons and priests

[72] Mansi, vol 24, col. 542 Nullus quoque clericus in sacro ordine constitutus aliquam chirurgiae artem exerceat, quae ad ustionem, vel incisionem inducat . .

[73] I take "deacon, subdeacon or priest" to be partitive appositives to "cleric," since deacons, subdeacons and priests were clerics, but not all clerics were deacons, subdeacons or priests.

[74] Mansi, vol 24, col 1190. Nullus clericus, diaconus, subdiaconus, aut sacerdos artem chyrurgicam exerceat, aut, ubi exerceatur, intersit .

[75] *Ibid.*, vol. 25, col 67. . . nec ullam partem chirurgiae subdiaconus, diaconus, sacerdos exerceant, quae adustionem inducit vel incisionem . . .

It has been my purpose in this paper to present and discuss the regulations found in medieval canon law governing clerical involvement in medicine and surgery. It is evident that these regulations cannot be legitimately used to support the broad statements made to the effect that *clerics* were forbidden to practise medicine and surgery. But knowledge of the law does not equal knowledge of its interpretation and application. It is on these two aspects that considerable work remains to be done. First, in regard to interpretation: Around the different parts of the *Corpus Iuris Canonici* a vast literature of treatises and glosses grew up. It would be extremely rewarding to examine as much of this literature as possible in order to achieve a balanced appraisal of how the relevant regulations were interpreted by contemporary canonists. Citing the opinions of a small number of canonists here, on the other hand, would possibly be misleading and should be held in abeyance until they can be juxtaposed to the opinions of similar sources. Second, on the matter of application: There is a vast quantity of primary documents of a regional nature (e.g., regional councils, diocesan synods, ecclesiastical correspondence) that should be investigated with a view toward determining the effect of the relevant regulations in canon law on local practice. It is here that the most concrete information can be sought on regional enforcement, extension and, perhaps, modification of the legislation surveyed in this paper.[76]

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[76] Some scholars maintain that the proportion of clerical to lay physicians declined considerably as a result of the legislation discussed Others say that it had little effect. Prosopographical research on this matter could provide a fairly definite answer

# AR-RÂZÎ ON THE HIDDEN ILLNESS

FRANZ ROSENTHAL

The catalogue of Arabic manuscripts in the General Library in Rabat, Morocco, lists a "Treatise by Abû Bakr ar-Râzî on the Hidden Illness (*ad-dâ' al-khafî*)."[1] No such treatise by ar-Râzi is known from bibliographical reference. When I looked at the manuscript during my visit to Rabat in 1963, I found it to be a treatise on *ubnah*, on which ar-Râzî is known to have composed a monograph.[2] *Ubnah* is a much used term for passive male homosexuality, and *ma'bûn* is the individual affected by it; it will be seen that ar-Râzî's use of the word comes as close to describing genetic confusion of gender in general terms as can be expected from a man of the Middle Ages. Following a lead provided by Fuat Sezgin,[3] I inquired about manuscripts containing the treatise in Teheran. With the greatest courtesy and promptness, Professor M. T. Danespazhuh sent me photostats of four manuscripts. Three of them contain ar-Râzî's *Risâlah fî l-Ubnah,* and the

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[1] Cf. I. S. Allouche and A. Regragui, *Catalogue des manuscrits arabes de Rabat*, II, 332, no. 2641, shelf-no. 1588d (Paris, 1954, and Rabat, 1958, *Publ. de l'Institut des Hautes Études Marocaines*, 58, 63). The treatise appears on fols. 44b-49a. The manuscript contains many different works, though this is the only one dealing exclusively with a medical subject. Since it is written throughout by the same Maghribî hand, the copying date of Muharram 917/April 1511 found at the end of one of the entries, fol. 145b, applies to the entire Ms. The name of the scribe is Abû l-Qâsim 'Abdallâh b. Muhammad b. Muhammad b. Ibrâhîm b. 'Abd-as-Salâm al-Asadî al-Andalusî. Entries in the Ms. are listed under the following numbers in the Catalogue: 938, 950, 1173, 1375-1377, 1379-1381, 1385, 1387, 1712, 1737, 2007, 2424, 2426 f., 2445 f., 2454, 2501, 2503 (containing the date), and 2641.

I wish to thank the authorities of the libraries in Rabat for the courtesies generously extended to me during my visits there in 1963 and 1976.

[2] The title in Ibn an-Nadîm, *Fihrist*, ed. G. Flügel (Leipzig, 1871-2), 301, 1. 18, reads *Kitâb al-Ubnah wa-'ilâjihâ*. The Râzî-bibliography of al-Bîrûnî (*Risâlah fî fihrist kutub . . . ar-Râzî*, ed. P. Kraus [Paris, 1936], 8, no. 33) has *'Ilâjât al-ubnah*, "The Treatments for *Ubnah*." Al-Qiftî, ed. J. Lippert (Leipzig, 1903), 276, 11. 1 f., has the same title as *Fihrist*, with the addition of the preposition "on." Ibn Abî Usaybi'ah, *'Uyûn al-anbâ'*, ed. A. Müller (Cairo and Königsberg, 1882-4), I, 320, 11. 15 f., expands the title of the *Fihrist* to include the words "and its explanation (*wa-tabyînihâ*)." This may be correct, but it could be a corruption of "*wa-sababihâ*, and its reason," which looks almost alike in Arabic script.

In the treatise itself, ar-Râzî refers to it as *maqâlah* (here translated "essay"). What we have is hardly a *kitâb*, "book," which implies a more substantial work. Ar-Râzî himself made a distinction between *kitâb, maqâlah,* and *risâlah* when speaking of his approximately two hundred published works in his *as-Sîrah al-falsafîyah*, ed. trans. P. Kraus in *Orientalia*, n.s., 1935, *4*: 319, 333, ed. by the same, *Abî Bakr . . . Raghensis Opera Philosophica* (Cairo, 1939), 109, 11. 9 f.

[3] *Geschichte des arabischen Schrifttums* (Leiden: Brill, 1970), III, 291.

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fourth a quotation from it from Ibn al-Maṭrân's *Bustân al-aṭibbâ'* with Ibn al-Matrân's comments (see below, n. 20).[4]

As it turned out, the Teheran manuscripts (T) contain only the first half of the work. All three of them do not have the section concerned with the treatment of the disease which, as indicated by the transmitted titles (above, n. 2), was an integral part of it. That section can, therefore, not be a secondary addition in the Rabat manuscript (R). It must have been omitted by the original copyist or compiler of T. On the other hand, T contains a long insertion not to be found in R. Whether or not this insertion belonged to ar-Râzî's original text, is a much more difficult problem. It first refers briefly to a certain assumed characteristic of mules and hinnies, apparently as a confirmation of the possibility that either the male or the female sperm may be prevalent without being the exclusive determinant. The line of reasoning remains, however, rather obscure (see below, n. 34). We then find a long quotation, from "another treatise" (introduced in the third, and continued in the first, person),[5] on the sterility of mules. At least on the face of it, it has nothing to do with the subject at hand. In fact, it tends to argue against the assumptions relied on in the treatise on *ubnah*. The conclusion seems likely that this passage was not found in the original treatise and was derived by the compiler of T from another work of ar-Râzî. The subject of the sterility of mules, which was so fascinating because it seemed to provide a clue for the solution of the distressing medical problem of human sterility, was no doubt discussed by him in his many works. The larger part of the addition in T can thus be eliminated as an original component of the treatise on *ubnah*. It is less certain that a similar case could be made for the first few lines. If they belonged to the original text, we are faced with the possibility that the text of R is also an excerpt, and a manuscript of the complete text remains still to be discovered.

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[4] The Mss. as indicated by Prof Daneshpazhuh are. Parlement, no. 352 Tabatabai, Parlement, no. 6201/5 (*Catalogue*, XIX, 188), and Bibliothèque Senat, no. 360, pp. 383 f. (*Bulletin de la Bibliothèque Centrale de l'Université*, vol. 6, p. 563) I have so far been unable to follow up his bibliographical references, which may have relevant information. On the dates of the Mss., see below, n 76. The excerpt from Ibn al-Matrân is contained in Ms. Parlement, no. 3821/12 (*Catalogue*, X, 1852)

The three Teheran Mss are identical in contents and go back to a common source. Textual variants are minimal The Tabatabai and Senat Mss. have homoioteleuton omissions as compared to Ms Parlement, no 6201, which is the best of the three. Ms. Tabatabai has only one of Ms. Senat's three omissions, and is thus quite likely to have been the Ms from which Ms. Senat was copied, although the difference in the title (below, n 23) is a slight problem

[5] If the reference is to the work of someone else whose view was quoted and debated by ar-Râzî in his treatise on *ubnah*, that other authority must have been mentioned in a passage now omitted It would mean that there is a good likelihood that T and R represent a very much shortened version.

If the text available to us at this time is an excerpt, it might very well be a representative one that omits nothing essential. The authorship of ar-Râzî, about which I originally expressed doubts,[6] seems beyond dispute. Lingering suspicions can all satisfactorily be disposed of. The quotation in Ibn al-Matrân provides strong supporting evidence, even if by itself it proves no more than that the text, in some undetermined form, was believed in the twelfth century to be a work by ar-Râzî, and already al-Bîrûnî knew of works of doubtful attribution.[7] While ar-Râzî likes to refer the reader to other works of his, as a careful scholar he is usually specific in his references and indicates the exact titles.[8] In the treatise on *ubnah*, his references are vague,[9] but this may be because he did not feel it would be necessary for the reader to have recourse to other specific works as they did not contribute much to the debate. The introductory statement on motivation and quality has its analogues in other works of ar-Râzî.[10] Quoting cases from his own practice (even if it is only one as in the present work) was ar-Râzî's customary procedure. Forgers could have successfully reproduced these devices, to be sure, but that is unlikely. A rather uncommon scientific spirit, such as that of ar-Râzî, is unmistakably at work throughout; if the author appears to be brief and a bit jumpy in his presentation, this may be due to his uncertainty and hesitation as to what to say about the puzzling subject. In sum, there is no decisive evidence to cast suspicion on ar-Râzî's authorship, but everything tends to confirm it. However, we must repeat that the original work may have been fuller than the text preserved in R and T.

The title found in R, speaking of a "hidden illness," goes back to ar-Râzî's own reference to a work so-titled by an unnamed predecessor. "Hidden" is meant to imply that it is an illness which the afflicted person keeps secret because he is ashamed of it. Love is sometimes called a hidden disease which one tries to conceal.[11] The "hidden illness" allegedly caused by hashish may refer to *ubnah*,[12] which in another passage is stated expressly to be one of the evil consequences of hashish consumption.[13] Elsewhere, *ubnah* is referred to as the "incurable (*'uḍâl*) illness."[14] The scribe of R, or an earlier copyist, may have picked the title in order to avoid the straightforward but crude word. This would be in line with the note of prudery struck at the end of the treatise where ar-Râzî apologizes for having discussed such an unpleasant subject. Coming from a physician, it may seem a somewhat unexpected statement, and those who know the ribaldry and coarseness of the innumerable jokes and anecdotes in Arabic belles lettres that have the *ma'bûn* as their object may be surprised by it. However, they need not be. The attitude expressed by ar-Râzî reflects Muslim middle class sentiment much more accurately than the entertaining literature, and while ar-Râzî's statement is particularly to the point, it is not unique.[15]

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[6] Cf. F Rosenthal, *The Herb* (Leiden. Brill, 1971), 86, n 5

[7] Cf. al-Bîrûnî, 7

[8] Cf , for instance, *as-Sîrah al-falsafîyah* (above n. 2) or *Sirr sinâ'at at-tibb*, which corresponds to the beginning of *Liber Rasis de secretis in medicina qui liber Aphorismorum appellatur*, printed in the old collections of ar-Râzî's works (Venice, 1497, Lyon, 1511). (I used the Arabic manuscript in the Royal Library in Rabat, no 3477, for other mss., cf Sezgin, III, 286). The frequent references in the *Murshid* (below, n 10) are also always specific

[9] It seems unlikely that in one of the instances the general reference should not be to ar-Râzî's works, see below, n. 32

[10] Cf., for instance, the introduction of his famous work on smallpox where he says that there is no exhaustive and satisfactory book by the Ancients on the subject, that Galen does not mention a specific or sufficient treatment or convincing cause, and that the experts in Syriac and Greek he consulted knew of no detailed information on the subject in Galen's works not available in Arabic translation In the *Murshid*, ar-Râzî complains about the lack of arrangement, obscurity, etc., of the Hippocratic *Aphorisms*, cf. A. Z Iskandar, in *Revue de l'Institut des Manuscrits Arabes*, 1380/1961, 7 (1) 17 At the beginning of his work *On Coitus* (Ms Istanbul, Aya Sofya 3724, fol 156b), he states that the defects of earlier works on the subject caused him to write his own.

Ar-Râzî's claim that his is the first detailed work to deal with the subject is hard to refute. No earlier medical monographs, and none from later medieval times, are known at present. In fact, we find little serious discussion either before or after him.[16] The ninth-century 'Îsâ b. Mâssah invokes the authority of Aristotle and devotes the thirty-first of his own *Problems* to a paraphrase of *Physical Problems,* IV, 26, ascribed to the Greek philosopher.[17] Ar-Râzî's illustrious, much older Christian contemporary, Qusṭâ b. Lûqâ, did not evade the subject in his remarkable "Book on the reasons why people differ in their character traits, their ways of life, their desires, and their preferences."[18] Qusṭâ admits to having knowledge of only two brief passages from the works of the Ancients dealing with it, one from the sixth book of Aristotle's *Physical Problems,* and the other from his work on *Physiognomy.*[19] The contention that passive intercourse serves for some individuals as a stimulus for active intercourse goes back to the *Physical Problems,* but Qusṭâ also reports that the great Ḥunayn b. Isḥâq called his attention to it. He himself hesitantly proffers the suggestion that the cause may be sought in the smallness and dysfunction of the penis as stimulating itching and motion.

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[11] Cf., for example, the verse quoted from Ibn al-Jawzî in an-Nuwayrî, *Nihâyat al-arab*, II, 143, l. 10 (Cairo, 1342—): *siqâm khafî*

[12] Cf Rosenthal, *The Herb*, 86, 178

[13] *Ibid* , 74, 82. The genuineness of the ascription to ar-Râzî remains to be ascertained.

[14] Cf *ar-Risâlah al-jâmi'ah*, ed. Jamîl Salîbâ (Damascus, 1368-71/1948-51), II, 185. The description of *ubnah* as a disease is found also in lascivious anecdotes such as are contained in the collection Ms Aya Sofya 2938, fols 280b, 281a. In connection with cases of coprophilia and similar aberrations, the description of *marad ẓarîf*, which is probably to be understood as something like "voluptuous/decadent disease," is employed by at-Tawhîdî, *al-Baṣâ'ir wa-dh-dhakhâ'ir*, ed. A Amîn and as-Sayyid A. Saqr (Cairo, 1373/1953) 90; ed. I al-Kaylânî (Damascus, 1964), I, 108

[15] Cf. my forthcoming article on the role of sex in medieval Muslim society in the *Sixth Giorgio Levi Della Vida Biennial Conference*

[16] An Ismâ'îlî work of an uncertain early date explains that no true Muslim can be affected by the "unclean illness" which came into existence through the reincarnation of a licentious woman, cf al-Mufaddal b. 'Umar, *Kitâb al-Haft wa-l-aẓillah*, ed 'Â Tâmir and I Khalîfé (Beirut, 1960), 109 f. Analogous ideas about the origin of homosexuality appear in the religious literature

A manuscript said to exist in private possession and entitled *Kitâb al-Ubnah wa-l-ma'bûnîn* may not be a medical work but a collection of anecdotes, cf. P. Sbath, *al-Fihris* (Cairo, 1938), II, 197, no 2498. The same applies to other known titles using different terms.

The great historian of Damascus, Ibn 'Asâkir, is credited with a *Kitâb Hulûl al-mihnah bi-husûl al-ubnah* in Yâqût, *Irshâd*, ed D. S. Margoliouth (Leiden and London, 1907-27), V, 142: ed. A F Rifâ'î (Cairo, n.y. [1355-7]), XIII, 79; for *ubnah* rhyming with *mihnah* "affliction" in verses by Abû 'Alî az-Zawzanî, cf. ath-Tha'âlibî, *Yatîmat ad-dahr* (Damascus, 1304), IV, 71. There may be some relationship between this title and Ibn 'Asâkir's brief discussion of the legal prohibition of *ubnah* in another work preserved in Damascus, cf. M.

All this is indeed less, and less scientific, than what ar-Râzî had to say. As in all his medical monographs, he structured his discussion to deal in strict sequence with cause and symptoms and the recommendable treatments and medicaments. He considers the illness to be basically a genetic one. This would imply that it cannot be "cured." However, ar-Râzî uses the unambiguous root for "healing" (*b-r-'*) twice, and apparently refuses to give up hope for a complete cure under favorable circumstances; mostly he speaks of *'ilâj* which vacillates in meaning between treatment and the cure effected by it. A certain amount of physical treatment at an early stage promises some relief, as do certain drugs and the observance of certain dietary rules. He considers the psychological aspect of the disease and recommends avoidance of any sort of self-indulgence. In particular, he warns against the kind of entertainment, common at the time both in public places and at private parties, which would arouse sexual emotions. A most potent palliative he

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N al-Albânî, *Fihris makhtûtât Dâr al-Kutub aẓ-Zâhirîyah. al-Muntakhab min makhtûtât al-hadîth* (Damascus, 1390/1970), 80

[17] On Ibn Mâssah, cf. M. Ullmann, *Die Medizin im Islam (Handbuch der Orientalistik, Erganzungsband,* VI, 1, Leiden Brill, 1970), 122 f I used the Istanbul Ms. Aya Sofya 3724, fols 138b-139a. The edition and translation in the dissertation of M W. Anbari, *Streitfragen uber die Zeugung, Nachkommenschaft und uber den Geschlechtsverkehr, verfasst von 'Îsâ ibn Mâssa* (Erlangen, 1971), was not available to me It is mentioned by R. Kruk, "Pseudo-Aristotle· An Arabic Version of Problemata Physica X." in *Isis,* 1976, *67.* 251-6 Kruk adds general remarks on the vexing problem of the character of the Arabic translation of the *Physical Problems*

[18] Ed P Sbath, "Le livre des caractères de Qostâ ibn Loûqâ," in *Bull de l'Institut d'Égypte,* 1940-41, *23* Ar text, 133-6; trans , 162-5 For Qustâ, cf. the introduction of H. Daiber's work, cited below, n. 36.

[19] The reference to Aristotle clearly is to *Physical Problems,* IV, 26. For the *Physiognomy,* cf. R. Foerster, *Scriptores physiognomonici* (Leipzig, 1893), I, 34 The latter passage was referred to by Y Mourad, *La physiognomonie arabe* (Paris, 1939), 46, n 3, who cites Qustâ. Cf also the similar passage in *Polemo,* ed Foerster, I, 277, ch. 61.

finds (true to his intellectual outlook) in the occupation with science and metaphysics.

In his commentary on the passage referring to the bearded Kurdish lady,[20] Ibn al-Matrân largely agrees with ar-Râzî on the genetic origin of the disease. He is slightly more detailed with respect to the physical characteristics of masculinity and femininity. However, he feels compelled to account for the existence of various degrees in the loss of sexual identity. He suggests as a likely cause the different amount of hotness in the temperament of the afflicted individual. In both males and females, the symptoms he mentions include disinterest in intercourse with the other sex. His starting point leads him to paying slightly greater attention to female masculinity, which, he says, may even go so far that masculine women "kill with their own hands." It may be due to his interest in the factor of hotness that he concludes that race (*jins*) and geographical location (*makân*) play a significant role. "If a Slavic woman (*ṣaqlâbîyah*) were affected similarly as the Kurdish woman mentioned by ar-Râzî, she would under no circumstances have grown a beard."

A predominantly genetic cause of *ubnah* is strongly denied by Ibn Sînâ, whose brief discussion of the subject is patently dependent on the *Physical Problems*, with, however, considerable distortion and change.[21] He believes that it is the result of habituation in an individual possessing weak sexual potency to begin with and a strong desire created by the imagination (*shahwah wahmîyah*). He rejects as absurd a physiological explanation proposed by an unnamed scholar concerning an abnormality of the sensing nerve leading into the penis. The persons affected by *ubnah* may be physically better endowed than ordinary males. They are people of a vile psychological (*suqûṭ an-nafs*) and bad physical disposition who have accustomed themselves to non-virtuous ways (*radâ'at al-'âdah*) and feminine behavior. "Any other theory is wrong. He who wishes to treat them is the most stupid of men. Their disease is one of the imagination, not a physical one. Things that break the desire, such as worries, hunger, vigils, detention, and beatings, constitute useful treatment." Ibn Sînâ does not mention ar-Râzî's treatise. It would be surprising if he had not known it. Could it be that he meant ar-Râzî by his vigorous denunciation of those foolish physicians who think that there might be a cure?[22]

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[20] For Ibn al-Matrân (Mutrân), cf. Ullmann, *op. cit.* (n. 17 above), 165 f. The number of known manuscripts of his *Bustân al-aṭibbâ'* is growing. In addition to the Teheran manuscript (above n. 4), I have used the manuscript which is in the National Library of Medicine in Washington, D.C., where the passage in question appears on fol. 174b of the old backward foliation, and Ms. Tunis, Bibliothèque Nationale, 14287 (no. 5400 of the Ahmadîyah collection), fol. 7b.

[21] Cf. Ibn Sînâ, *Qânûn* (Rome, 1593), 562 f. Ibn Sînâ was quite closely followed by Ibn Hubal, *Mukhtârât* (Ḥyderabad, 1362-64), IV, 82-84.

The omission of the second half of ar-Râzî's treatise concerned with the treatment of *ubnah* in T might possibly be due to the influence of Ibn Sînâ and his school. The compiler of T might have considered this part as superfluous and outdated, and therefore omitted it. We have no way of knowing whether this was so. If this was his motivation, he did not serve scholarship well. It was unavoidable for ar-Râzî to come up with highly speculative, not to say fanciful or even silly, ideas. As a whole, however, his monograph on a problem or set of problems long debated and as yet unsolved would seem to be quite remarkable for its time as a work of serious scholarship groping in the dark.

## TRANSLATION

Muḥammad b. Zakarîyâ' ar-Râzî says:[23]

As we have stated in the beginning of more than one of our books, (scholars) later in time must investigate what early (scholars) have neglected and[24] postponed[25] or scattered[26] (in different places) or expressed obscurely. They will thus mention what they had neglected, bring together what they had kept separate, comment upon what they had expressed summarily, and explain what they had kept obscure.

Something the early (scholars)[27] have neglected is the discussion of *ubnah* and its cause and treatment. Till now, I have not found this discussed exhaustively by anyone. Indeed, I have not found it mentioned by nearly anyone of them except for one man who wrote a book on this subject which he entitled "The Hidden Illness," but he indicated in it[28] neither specific[29] cause or satisfactory reason nor medication[30] or useful[31] treatment.

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[22] In early fourteenth-century Europe, Ibn Sînâ's views were rejected in favor of those of Aristotle by Peter of Abano (writing in 1310), as I learn from N G. Siraisi, "The Expositio Problematum Aristotelis of Peter of Abano," *Isis*, 1970, *61* 336

[23] R "Abû Bakr says " The title in R is: "The essay (*maqâlah*) of Abû Bakr ar-Râzî on the hidden illness." In T, it reads. "The treatise (*risâlah*) on *ubnah* by Muhammad b. Zakarîyâ'," which in Ms. Senat, however, is replaced by. "On *ubnah*, also one of his compositions and works composed for one of his friends "

[24] R "or".

[25] Ar. *tawwala*, whose more common meaning, "to explain at length," does not seem to fit here If ar-Râzî meant to criticize too great length, it could be only as a general statement and would be inapplicable to the subject at hand

[26] R om

[27] Ar-Râzî may have thought here of the Ancients, but apparently his intention was to include the early centuries of Islam to his own time

[28] T om It could mean, "in connection with (the disease) "

[29] *Mustaqill* om. R. It means literally something like "being in independent control of "

Now, I am going to speak about it succinctly and to the extent I consider sufficient, if God wills. Thus, we say: We must seize and exploit here a premise that has been previously established in another book (of ours).[32] That is, femininity or masculinity occurs only in accordance with the prevalence of one of the two sperms over the other in quantity and[33] quality, until one of them becomes the one that transforms (*muḥîl*) and the other the one that is transformed (*mustaḥîl*).

I say:[34] The obvious proof for that is the fact that the female mule is better than the male mule, because the horseness is prevalent, and the assness inferior, in it. This is the case when the ass is the male. Conversely, if the horse is the male, as they do in some places (where they mate stallions with she-asses in order to produce hinnies), the male mule is better than the female mule.

He says in another treatise:[35] If the question is asked why mules are sterile, the answer should not be: because "male mules do not reproduce on account of the scarcity, thinness, and coldness of the sperm, and the females since their uteri are not open"; or because, "as Empedocles has stated, this results from the smallness, low location, narrowness, and crookedness of their uteri and because they are placed opposite to the position of the stomach and the sperm cannot get to them directly and reach the place where it is needed," and he assumed that "he had seen during the dissection of mules uteri of such a shape, and it may be that the sterility of women results from a similar cause."[36] For we say that, if these are indeed the reasons for the sterility of mules, he has not provided the basic reason. That is, one must know why the sperm of mules becomes colder and thinner, and why their uteri are placed in such a position. It should rather be said that the most likely and convincing (explanation) concerning this subject is that the sperm of this animal has undergone a transformation (*istaḥâla*) and has greatly deviated from the nature of reproductive sperm because it is generated from two sperms different in species that have mixed although they are very different. This has the effect that it is outside the accidental temper (*ʿarad mizâj*) of each one of the two. Each one of the sperms has thereby been removed far from its own nature, and something very unlike both is generated from them. Therefore, their sperm has become non-reproductive.

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[30] *Mudâwâh* is another synonym for "treatment," but I have chosen here "medication" because the idea of treatment by means of drugs may have been associated with the word

[31] R om

[32] T has a dubious plural (*kutub ukhar*), but this seems to be a mistake or, in view of the fact that the matter was also discussed by others, an intentional correction.

[33] R "or "

[34] This paragraph is omitted in R, see above, p. 46 It was, of course, well known that the female parent dominates in mules and hinnies, cf ad-Damîrî, *Hayât al-hayawân* (Bûlâq, 1292), I, 156. Thus, the argument seems to run, when the mule is a female, the characteristics of its female parent, the mare, give it the superior qualities of horses to a greater degree. When the hinney is a male, the characteristics of its female parent, the ass, are more concentrated in the female hinny, giving the male hinny a greater share in the superior qualities of its stallion parent Or, perhaps, what is meant is simply that the "better" parent, the horse, passes on its superiority to its offspring of the same sex, thus showing that there is something like a superior sperm when male and female sperms mix

[35] For the passage in small type omitted in R, see above, p 47.

[36] The quotations are from Pseudo-Plutarch, *De placitis philosophorum,* V, 14 The three statements attributed to Alcmaeon, Empedocles, and Diocles are here clumsily conflated Otherwise the text is almost identical with that published by H. Daiber, *Die arabische Uebersetzung der Placita Philosophorum,* (Diss. Saarbrücken, 1968), 303. Ar-Râzî wrote "an abridgment of the Book of Plutarch," cf. al-Bîrûnî, *Risâlah,* 16, no. 113 The *Placita* are possibly meant, notwithstanding the rather obscure text of Ibn an-Nadîm, *Fihrist*, 301, 1 5 Ibn Abî Usaybi'ah, I, 318, l. 30, has "Completion of the Book of Plutarch "

Now, I say: This proof does not at all achieve the objective sought, because the fact that what is generated is unlike both does not require it to be non-reproductive; it rather requires it to be able to reproduce another species. If it is correct, as stated (by some), that the sperm of the *simʿ* (Lycaon pictus), which is a cross between wolf and hyena, is able to reproduce, then the proof is entirely invalid. If it is not correct—and it is obvious that this is the truth—, the proof needs completion, because the statement mentioned leaves it incomplete. The most likely assumption is that, if sperms of two species combine in the uterus, the formative power of the sperm of the male finds it hard to act upon the sperm of the female because it can indeed influence it, or be influenced by it, only with difficulty. The same applies to the formative sperm of the female. Thus, both powers are weakened or ineffective in the male and female of the species that originates from the two species, because they find it difficult and troublesome to act and be acted upon. Similarly, if the stomach of a person is able to digest only soft foods, and he then combines soft and coarse food, the digestive power of his stomach is weakened, because it suffers on account of having to digest that coarse food. If the power of formation in the sperm of the female mule is weakened or ineffective, no animal can be generated from it.

If[37] the sperm of the man is the one that transforms, the newborn child will be male, and if the sperm of the woman is the one that is prevalent, the newborn child will be female. We have explained the correctness of this proposition in another book (of ours).[38] The Ancients have also much discussed it.[39]

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[37] T adds. "Then he says "

[38] See above, n. 32

[39] The theory adopted here by ar-Râzî is reported by Galen in the name of Stratonicus and rejected by him, cf. *De semine*, IV, 629 (Kühn) Another theory more acceptable to Muslim authors (cf , for instance, ʿArîb, *Khalq al-janîn*, ed & trans N Abdelkader and H Jahier [Algiers, 1375/1956], text, 24, trans , 32) is related to one discussed by Aristotle (*De generatione animalium* 765a4 ff ) and reported in the *Placita philosophorum*, V, 7, 5 (cf Daiber, op cit. [n 36 above], 86), where it is not made explicit that Aristotle rejected it; cf ar-Râzî, *Hâwî* (Hyderabad, 1374-90/1955-71), IX, 75, 79 The theory that the amount (*kathrah*) of the male or female sperm determines sex is mentioned by ar-Râzî, *Hâwî*, IX, 126, as that of "the Indian" (cf Ullmann, *op cit* [n 17 above], 106). The subject was, of course, of immense interest and provoked many attempts to explain it (For the Arabic translation of the cited passage of *De gen anim* , cf the edition by J Brugman and H J. Drossaart Lulofs [Leiden, 1971], 137 f.)

If the matter is as we have described it, it may happen[40] in some cases that the sperm of the man is very forceful and strong in transforming the sperm of the female. Accordingly, it is necessary that the child born from such a sperm will be very strongly masculine—I mean, the properties of masculinity in him will be strongly and obviously masculine, such as limbs hard, dry, and large, much hair, a strong pulse and breath, prominent (*ẓuhûr*) joints, thick bones and similar properties peculiar to persons of a hot and dry temper, such as courage, quickness of speech and anger, and the like.[41] If it happens in some cases also that it is the sperm of the female that possesses very great force and prevalence, then the newborn child will have the properties that are peculiar to females, which are the extreme opposites of what we have mentioned. Mostly it happens that one of the two sperms undergoes transformations between these (extremes). Then, the newborn child, whether male or female, is not masculine in the extreme and not feminine in the extreme.

If the matter with regard to this subject as is we have described it,[42] it is possible that in some cases it happens that a male child's masculinity or a female child's femininity is extremely weak. We therefore find masculine women, as we find feminine men. Among masculine women, it may go[43] so far that they have little menses or none at all,[44] and occasionally, they will grow beards. I have seen beards and[45] faint moustaches[46] on certain females.[47] Once I saw a Kurdish woman with a luxurious beard who had been brought to al-Muʿtaḍid as a curiosity.[48] This is not the only thing that may occur. Rather, when the two sperms are equivalent and the one has little superiority over the other, hermaphrodites (*khinâth*) will occur, and a child may even be born with both penis and vulva. We have learned[49] stories in this connection containing things that are marvelous,[50] hideous, and noteworthy[51] in connection with this chapter. We have omitted to mention them because we consider them absurd.[52] For instance, a certain surgeon is reported to have found a uterus in some male animals, and many[53] people tell of a woman who gave birth to children and then afterwards, a male organ appeared on her (body).[54] This story and others like it have come to us from many directions. For the purpose we have before us, we do not need (to ascertain) the correctness of this (particular) story. Rather, we can be satisfied with what has been mentioned before,[55] namely, that not every male is masculine in the extreme and not every female is feminine in the extreme and that there exist masculine women and feminine men.

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[40] T "and it happens."

[41] R "and other such properties peculiar to them "

[42] T "mentioned."

[43] R adds the negation *lam* (?)

[44] Cf. Aristotle, *De gen. anim.* 747a1 ff.

[45] T om. "beards and "

[46] T *s(h)wr't* (?). *Shawârib* "moustaches" occurs also in Ibn al-Matrân's comments

[47] *ʿAlâ* (R *fî*) *khalq min an-nisâ'*.

[48] See above, p. 50

[49] R appar. *ta'addat*; T *tm'dt ilaynâ*

[50] T om

[51] T om.

[52] Thus T (*li-buʿdihâ ʿindanâ*); R *li-buʿd kawnihâ ʿannâ*

For, after the ideas which we have just mentioned have been perceived, the cause of *ubnah* is easy to understand. That is, when it happens by chance that the male newborn child is feminine because of the weak degree of prevalence of the sperm of the male over the sperm of the female, even if it is prevalent in general, the consequence is that the male organ, the testicles, and the sperm ducts and vessels do not fully tend outward and do not hang down low[56] and are not big and strong, but the contrary is the case. I mean, they tend upward, are mostly also small, withdraw inward into the cavity of the belly, and are drawn toward the hypogastric and[57] pubic regions because of their weak masculinity, since the organs of reproduction in the female are placed inside the belly and conditioned to tend in that direction, while in males, they are outside the belly and are by nature disposed to tend in that direction. Such disposition has the effect that the tickling and the motion that results from the sperm stirring it up by its quantity and quality are in the region of the rectum behind[58] and not in the direction of the hypogastric[59] and pubic regions, because the sperm vessels and testicles tend by nature in that direction. Therefore, someone affected by *ubnah* is rarely found to have large[60] testicles that hang down, but the contrary is the case. Mostly he will have small and wrinkled testicles, drawn toward above and entering the groin (*urbîyatân*). The hanging down of the testicles and the thickness and width of the skin of the testicles are an unfailing[61] indication of the absence of

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[53] T om.

[54] T "she revealed a male organ."

[55] Thus T (*al-madhkûr ânifan*); R *al-mawjûd dâ'iman*, which may be translated "what always exist," but is probably a mere mistake.

[56] Thus T (*mutasabbilah mutadalliyah*), R *musbilan mutadâyimah*

[57] R om "hypogastric and." The term given here the approximate translation "hypogastric region" is *thunnah* It is defined as the region between stomach and pubes and stated to correspond in the front of the body to the lower part of the back, *qatan*, which extends down and includes the perineum Cf for instance, ar-Râzî, *Hâwî*, X, 59, 11. 13 f ; Ibn Manzûr, *Lisân al-ʿArab* (Bûlâq, 1300-8), XVI, 235

[58] T *min khalfin*; R *min khalfihî*

[59] R "bladder "

[60] Thus T (*ma'bûn 'aẓîm*); R *fawq* (or *bwn*) *'ẓm*

[61] R om

*ubnah*. It is also[62] mostly accompanied by bigness of the penis, as is *ubnah* by its smallness.

If it happens by chance that the male newborn child is feminine and those parts of the body are placed in that manner, he is affected[63] by something like the motion of tickling in the region of the rectum when the sperm is plentiful and sharp, just as masculine persons have that in the pubic region when the sperm is plentiful and[64] sharp. If someone whose natural disposition is such is aided by his passion because of delicateness[65] or some chance happenings that happen to him, so that something that[66] touches and moves that place of his gets near it, he finds,[67] on account of it, a pleasure similar to that found by someone who[68] scratches his ear or nose by putting a finger into it and moving it around and scratching it,[69] because that removes the prickling humor (*khilṭ*) and scatters it and also resolves these(?),[70] which quiets[71] the stirring and tickling. And if he aids the pleasure and is inclined to it,[72] this sensation (*ʿâriḍ*) gains in strength, and the limit of it is reached, corresponding to the strength of the tickling and stirring of the sperm in that man and corresponding to his degree of effeminacy (*khanath*) and his love of femininity and[73] his inclination to pleasure. This is the active cause of this disease.[74]

We have described this disease[75] as briefly and succinctly as we are able to do.[76] Now, we shall mention what we consider useful and satisfactory with respect to its treatment. Thus, we say:

If the *ubnah* is prolonged, the person affected by it cannot be cured, in particular, if he is obviously feminine and effeminate (*ta'nîth-takhnîth*) and loves very much to be like a woman. If it is in its beginning stages and the person affected by it is not obviously effeminate and not strongly inclined to pleasure but rather ashamed (of it) and would like to be free from it, it is possible for him to be treated.

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[62] T om.

[63] Thus T (*i'tarâhu*), R unclear

[64] R "or "

[65] Thus T (*li-taraffuh*); R somewhat unclear.

[66] T *bi-mâ*; R *mâ*

[67] Thus R (*w-j-d lahû*), T *wa-yaltadhdhu bi-dhâlika*.

[68] Thus R (*yajidu man*); T *yajibu an*

[69] In his *Kitâb al-Ladhdhah*, ar-Râzî compares the pleasure of intercourse with that derived from the scratching (Persian *kharîdan*, here Ar. *hakk* rather than *daghdaghah*) of itching scab, cf P Kraus, *Abî Bakr . . Opera philosophica*, 154.

[70] T *wa-yuhallilu minhu aydan*; R *wa-yuhallilu aydan hâdhihî*. Neither seems correct.

[71] R om

[72] Both T and R read *wa-marra ma'ahâ* (?), which could hardly mean "goes along with it " Read *wa-mâla ma'ahâ*?

[73] R "or."

[74] R om. "of this disease "

[75] T "it" (referring to "cause")

[76] T ends here, adding "And God knows better " The subscription Ms Parlement, no. 352 Tabatabai reads. "The treatise has been finished in the year 993/1585." The date of Ms. Parlement, no. 6201, is Ramadân 11, 1006/April 17, 1598 Ms Senat has: "And God knows better about the verities of secrets Finished on Muharram 12, 1007/August 15, 1598."

The best treatment consists of frequently massaging penis and testicles and drawing them downward. Maids and slaves[77] with nice faces and much practice (*mufriṭât*) in this matter should be put in charge of the patient, in order to rub and massage that place and apply themselves to it and kiss it and fondle it. This should be done as much as possible. At other times, his treatment should consist of the application to the pubic region, penis, and testicles of ben oil to which borax, euphorbia, and musk are added. At times, some asafetida may be added to the oil. The penis is massaged with it, and drops of it are put into the urethra. When this treatment is applied, he should sit in hot water, and the penis and testicles are massaged while he is in the bath tub.[78] Once each week, fluid pitch is employed. For it[79] is the strongest treatment, which draws the blood and the hot vapor into this direction. If erection approaches (?),[80] while the testicles hang down, the penis extends and grows big, and desire increases, it is a sign of the effectiveness of the treatment. He must apply the entire treatment faithfully. Nothing whatever should be omitted. When he feels well (*fî awqât al-ittisâ' lahû*), the patient must not give up any part of it, the first being, as we have described, the maids, then the application of (various) oils, and then the fluid pitch. In addition, we should undertake treatment (*tadbîr*) of the lower back. For as there is need for the blood and the spirit to be drawn to this region (the genitals), thus the region of the lower spine and the rectum must be cooled. This is to be done by his lying on his back on the ground that is sprinkled (with water) or by his placing on his lower back rags likewise wetted with ice water. He should guard against lying down on the back upon something hot. He should wear a wide belt for a long period.[81] He should receive an enema of rose oil into which vinegar has been boiled till the vinegar (with the rose oil) can be poured, and also with rose water with a little vinegar. In general, if he is able to have his lower back always cold and the pubic and hypogastric regions always hot, he should do it, for it is the most appropriate thing to do.

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[77] R *wa-rq't*, read *wa-riqâb*.

[78] In the Hellenistic tradition, ar-Râzî has much to say on the therapeutic use of baths, cf., for instance, *Murshid* (n. 10), 38, or *Liber ad Almansorem*, IV, 18.

[79] The entire procedure, it would seem, rather than just the fluid pitch

[80] R is not clear (*fa-idhâ aqbala al-in'âẓ fîbit, etc -k-w-n wa-l-baydatân tandaliyân. .*), but there can be little doubt about the general meaning

[81] Cf. below, n 84

It is clear that there is nothing more harmful for someone affected by this disease than having passive intercourse, as there is nothing more useful for him than practicing active intercourse or attempting to practice such intercourse as much as possible.

This then is the sum and substance of the treatment of *ubnah* according to the straightforward norm and method.

I am going to mention also (other) ways of treating it. One of them consists of giving the patient many times an enema of strong intoxicating liquor (*ash-sharâb al-muskir*). More than one person has been cured by one or two enemas. Other things that lighten (?)[82] and weaken this are lying on the back upon the ground[83] and wearing as a belt a large sheet of black lead,[84] also, receiving an enema of the boiled seed of the chaste-tree[85] and lying on the back upon its leaves.

This is advice (*ishârah*) for someone who confided his secret to me. I decided that he would benefit from it, and he did so greatly. When that man ate and repaired to his bed, this disease bothered him. So one day I advised him to cut off a piece of ice to serve as a suppository and carry it (in his anus). He slept in this manner satisfied and was able to dispense with what he had been forced to do most days. He was almost close to being cured, and he would have been if he had exercised strong control over himself.

The treatments mentioned by me are right for young men and affluent people (*mutraf*). In connection with others, your aim in treating them should be to make them lose weight and diminish the blood. Thus they should be ordered to fast, to give up drinking wine, to use vinegar for seasoning, to keep the lower back as cold as possible, and (to take) drugs known as effective for diminishing the sperm, especially those that are cold, such as the drug derived from the roots of waterlilies, roses, camphor, and tabasheer, as this is a drug that diminishes and jells the sperm. We have mentioned its composition in more than one place in our books. They should also stick to foods such as

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[82] R *yukhaffif* (?) "Drying" (*yujaffif*), common in therapy, does not seem to make sense here. Read, perhaps, *yud'if* another word for "weaken."

[83] R *al-ward,* read *al-ard*

[84] Cf. Galen, *De sanitate tuenda,* VI, 14, 11 (Corpus Medicorum Graecorum, V, 4, 2, p. 195 [Leipzig, 1923]). *molibdinên lepida tais psoais hypoballonta . . . tou athlêtou pros to mê oneirôttein auton,* a passage used by ar-Râzî, *Hâwî,* X, 255, l. 17, 336, l. 1.

[85] *For banjakusht,* cf., for instance, *Al-Biruni's Book on Pharmacy and Materia Medica,* ed. & trans. Hakim Mohammed Said (Karachi, 1973), 78, or, for convenient reference, M. Levey, *The Medical Formulary or Aqrâbâdhîn of al-Kindî,* (Madison, Milwaukee, and London: University of Wisconsin Press, 1966), 247. I have refrained here from going into the thorny and much discussed subject of the identification of medicinal plants

*qarîṣ*,[86] *maṣûṣ*,[87] *hulâm* (?),[88] *hiṣrimîyah*,[89] with gourds and lentils,[90] drink constantly oxymel, do much sweating in the bath, place the feet in cold water, keep away from parties where there are amusement, winedrinking, and singing, and occupy themselves with pious devotions and the true kinds of knowledge that take hold of the hearts and engage the souls strongly, such as geometry and logic, and more than that, with that divine knowledge (metaphysics). For the concern with it and the immersion in it weaken all desires.

Having reached this place, I would like to mention the prescription for the compound that diminishes the sperm, so that the person who studies this essay (*maqâlah*) has no need to search for it in my other books[91] and in the books of the Ancients. This is one of the prescriptions:

Take ten dirhams of the dried roots of waterlilies, five dirhams of ground red roses, two and a half dirhams of white sandalwood, and five dânaqs of camphor. These are ten drafts.

Another prescription which is useful when the roots of waterlilies cannot be found: Ten dirhams of ground roses, five dirhams of white tabasheer, two and a half dirhams of white sandalwood, and five dânaqs of camphor. These are ten drafts.

Another prescription for those who dislike camphor: Take five dirhams of ground roses, ten dirhams of fleawort seed that is not crushed, three dirhams each of lettuce seed, endive seed, and origanum (?)[92] seed, and two and a half dirhams of dry coriander, (each) draft three dirhams of the whole, with half an ounce of vinegar.

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[86] Defined in the Persian-English dictionary of F Steingass as "meat or fish eaten with bread "

[87] Described in Steingass as "a dish made of boiled meats, especially young pigeons, fowls, or other birds, dressed in vinegar and stuffed with condiments " A recipe for it may be found in Muhammad b. al-Hasan al-Kâtib al-Baghdâdî, *Kitâb at-Tabîkh*, ed. F. al-Bârûdî (n.p. [Beirut], 1964), 56, where one also finds *masûsîyah*, p 23; trans A. J Arberry, in *Islamic Culture*, 1939, *13*. 41 Cf. also M Rodinson, in *Revue des Études Islamiques*, 1949 [1950], 133, with reference to another medieval cookbook, *al-Wuslah ilâ l-ḥabîb fî wasf at-tayyibât wa-t-tîb*.

[88] R has *wa-l-'m'l*, which is not known to me. The correction to *hulâm* (or *halâm*) is suggested by the fact that *hulâm* follows upon *masûs* and *qarîs* in ar-Râzî, *Hâwî*, X, 262, 1.7 Hava's Arabic-English dictionary describes *hulâm* as "veal served with the skin. Kind of animal jelly "

[89] Lane, *Arabic-English Dictionary*, 584b, basing himself upon J. G. Wetzstein, *Samachschari Lexicon Arabicum Persicum*, 60, defines the dish as "soup made [or flavored] with unripe grapes or dates, or with the juice thereof." It occurs in ar-Râzî, *Hâwî*, X, 215, 1 5. A recipe for it in al-Kâtib al-Baghdâdî, 17 f., trans. Arberry, 37 f., Rodinson, *op cit*. (n 87 above), 136.

[90] R, which is not very clear, seems to have *bi-l-qar' wa-l-'adas* Note that "lentils" are also mentioned in the *Hâwî* passage cited in n 88

[91] Cf *Liber ad Almansorem*, V, 67

[92] *M-r-w* seems to come quite close to what is found in R. The reading *murabbâ*, "coated with inspissated fruit juice," would presuppose that the word specifying the particular kind of seed is omitted

Another prescription, good for those of a cold and wet nature and for those who are advanced in age: Take ten dirhams of chaste-tree seed, five dirhams of river mint[93] dried, and two and a half dirhams of dried rue leaves, (each) draft three dirhams of it, with an ounce of vinegar. Those who are sensitive to vinegar should drink it with cold water or rose water.

I have discussed this matter sufficiently. We apologize for having discussed it. We have been forced to do so because our predecessors did in no way discuss it sufficiently.

To the Giver of the intellect be praise without end as He deserves and merits. His prayers and blessings be upon His Prophet, Muḥammad, and his family.

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[93] For the three kinds (mountain, field, and river) of *fawdanaj*, cf., for instance, ar-Rāzī, *Ḥāwī*, XXI, 243-50. R omits the final ī of *an-nahrī*

# THE HYGIENE OF THE WHEEL: AN EPISODE IN VICTORIAN SANITARY SCIENCE

JAMES C. WHORTON

During the past two decades in the United States, there has occurred a remarkable warming of public feeling toward physical exercise. The medical profession has assumed a leading role in deploring the excessively sedentary and tension-filled lives of urban Americans, and in educating the public to the cardio-vascular and nervous benefits of regular physical exertion. One of the most visible expressions of public response has been the adoption of bicycling by so many adults. But in recommending their sport, cyclists have at times appeared overly sanguine, claiming rewards beyond plausible physical improvements. Thus Paul Dudley White, who was physician to former President Eisenhower and chief instigator of the current interest in cycling for health, (has) proposed that the bicycle is not just "the salvation of the underexercised American," and not only a liberator from tranquilizers, sleeping pills and tobacco. It is also an instrument "that our clergy and psychiatrists should pay more attention to," as, apparently, should our statesmen and legislators. "Regular and vigorous use of our leg muscles," Dr. White (has) argued, "is essential for the best use of our brains. Our country's welfare may actually depend on your cerebral blood supply."[1]

A thread of "hygienic utopianism," of belief in moral and social perfection through reform of the body, winds continously through American culture of the last 150 years, and the more hopeful proclamations of bicyclists might easily be attributed to this continuing frame of mind. Yet the nature of cycling is such as to encourage utopian visions more readily than other forms of hygiene. The bicycle is relatively inexpensive, and usable by both sexes and nearly all ages; it is a machine which removes its rider from irritating urban scenes to relaxing pastoral ones, and from sources of temptation to those of inspiration; and all the while it healthfully exercises the body and leaves it too fatigued for dissipation. The social and moral accoutrements of the bicycle become evident as soon as its use for physical recreation is examined, so that one might even expect to find

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[1] "Dr White on wheels," *Life*, Nov 10, 1961, *51*: p 71; "Pedaling to health," *Time*, Aug 2, 1963, *82*, p 42; "Cycling for fitness," *Recreation*, 1964, *57* 405-6, p. 405.

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an optimistic social hygiene of cycling predating the current excitement. In fact, pronouncements such as White's sound remarkably like echoes of many late nineteenth-century doctors who also expected the bicycle to further the welfare of the country in addition to that of the individual. For a brief period, sanitarians, professional and amateur, were engrossed in promoting cycling as a panacea, and then absorbed in defending it from the extravagant denunciations of more conservative physicians. As prosaic and trivial as it may at first appear, the 1890s debate over the bicycle offers an informative capsule view of the pressure imposed by cultural preoccupations on medical deliberation. The debate was affected as much, it will be seen, by Victorian fervor for self-improvement and social progress, uneasiness about the accelerating pace of urban life, misgivings about the consequences of female emancipation, and fear of racial deterioration, as by any purely medical considerations. It was, indeed, a social-medical issue which serves to reiterate the energetically humanistic "social medicine" orientation of nineteenth-century sanitary science.

* * *

The rise of cycling was at first merely coincidental with sanitary reform.[2] Several versions of "celeripedes"—two wheels connected by a beam, on which a rider sat while running over the ground—had been marketed in France and England by 1830, but all were so expensive and exhausting that only young men of means could manage them. The machine was therefore initially a fashionable affectation rather than an instrument of hygiene, and even the introduction of crank-driven "velocipedes" in the 1860s, and high-wheeled "ordinaries" the following decade, only partially popularized cycling. The fear of "taking a header" ("by no means a difficult exploit," beginners were advised; "indeed the difficulty is to avoid performing it")[3] confined the unathletic and timid to inelegant tricycles until the final stage of evolution of the bicycle, the "safety." The pattern of this machine, utilizing wheels of the same moderate size and placing rider and pedals between the wheels, was worked out gradually through the 1880s. With the addition of pneumatic tires in 1889, and the reduction of price to as low as seventy-five dollars, the bicycle was at last democratized. Now nearly anyone of middle-class status could afford a "wheel," and could learn to ride it safely; and nearly everyone, it appears, took advantage of the opportunity. Physicians and clergymen began making their rounds on bicycles, instead of horses; policemen and journalists adopted the machines in preferance to walking their beats; the army experimented with wheels as replacements for cavalry horses; athletes turned to long-distance cycle-racing as a challenging form of competition. Most of all, the public at large greeted the "safety" as an exhilarating recreation (or at least adopted it in fashion-conscious imitation). By the mid-1890s, the "bicycle era" was in full flower in England and America: "We have become a race of Mercurys, . . . and the joy which is felt over the new power amounts to a passion."[4]

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[2] For general treatments of the history of bicycling, see Robert Smith, *A Social History of the Bicycle* (New York American Heritage Press, 1972), Frederick Alderson, *Bicycling, A History* (Newton Abbot, Eng : David & Charles, 1972); F.P. Prial, "Cycling in the United States," *Harper's W.*, 1890, *34*· 669-72

[3] Quoted in Alderson (n. 2), p 55

Undoubtedly much of this passion was aroused by the promise of improved health offered by the bicycle. Sanitarians had by that time come to appreciate that efforts toward environmental cleanliness had failed to produce full health. Only in cities like "Hygeia," Benjamin Ward Richardson's sanitary utopia, were miasms entirely absent. In the real world, the urban atmosphere remained foul in spite of the progress of sanitary engineering, and periodic flushing of the lungs with the pure air of the countryside was recognized as a hygienic necessity. So were general bodily exercise and mental diversion and relaxation. Regular muscular exercise, in particular, had been urged for years, but had been generally ignored by a public that found riding horses and rowboats expensive and gymnasia often inaccessible or uninteresting. Before the bicycle, the evening constitutional, "that dreariest of occupations to the average man,"[5] was the only form of exercise commonly available and likely to be taken. But cycling, after a reasonable initial expenditure, was inexpensive and carefree. All could indulge in it, the old as well as the young, the weak as well as the strong. It drew people out of dark, ill-ventilated bowling alleys and gymnasia into the sunshine, and with its easy speed carried them from the congested atmosphere of the city to the pure, open air of the countryside. Finally, cycling was fun, unlike the ritualized drudgery of calisthenics. "The bicycle," it was soon clear, "is inducing multitudes of people to take regular exercise who have long been in need of such exercise, but who could never be induced to take it by any means hitherto devised."[6]

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[4] Joseph Bishop, "Social and economic influence of the bicycle," *Forum*, 1896, *21* 680-9, p 680

[5] J Boothroyd, letter to editor, *Lancet*, 1892 (ii), p 616 Other articles arguing the practical superiority of cycling to previously available forms of exercise include "Some thoughts on the hygiene of the wheel," *Wheelman*, 1882-3, *1*, 90-3, "Cycling and vitality," *Lancet* (ii), 743-4; and H Williams, "The bicycle in relation to health," *Harper's W* , 1896, *40*· 370

[6] Williams (n. 5), p 370

Cycling not only inspired otherwise inactive people to exercise, but provided more effective exercise than other sports. "The rider of a wheel takes exercise in the most even, steady way I know of"; "by [the cycle's] use the muscular system is exercised and developed with a uniformity that cannot be achieved by any other means"; "the exercise is well and equally distributed over almost the whole body, and . . . when all muscles are exercised, no muscle is likely to be over-exercised": these were widely shared opinions among physicians excited about the bicycle.[7] When taken in the proper quantity, this exercise not only strengthened the voluntary muscles, but healthfully stimulated the heart, lungs, and digestive system, and encouraged the excretion of accumulated morbid matter. Even the nervous system would benefit both from the general invigoration of the exercise, and from the watchfulness the sport required. "The rider must constantly use the senses of hearing, seeing, and feeling in order to avoid collisions, direct his machine, and keep his equilibrium." Gradually the nerves acquired a new tone. "Every rider has felt it. He may not appreciate it now, because he is accustomed to it; but let him stop his riding, and the result is soon apparent in sluggish memory, or annoying organs, or easily tired limbs—in short, a loss of balance."[8] With good reason, apparently, could sanitarians proclaim that the bicycle had advanced "the sanitation of this country . . . a hundred years," and expect "that the 'cyclo-anthropos' of the twentieth century will suffer less from his nerves and will be more muscular than the man of the nineteenth."[9]

It went almost without saying that an exercise so efficient at improving the health of the normal, must restore the health of the debilitated. Merely a cursory gleaning of the medical literature of the 1890s yields an imposing list of ailments pronounced curable by the wheel. Dyspepsia, anemia, obesity, curvature of the spine, asthma, varicose veins, heart disease, and diabetes were the more frequently mentioned. Even the "over-riding of the right great toe by its minor neighboring toe" yielded to regular pedaling.[10] In the treatment of bilious diseases, another physician announced, "bicycle exercise . . . surpasses even calomel."[11] When it was revealed in 1897 that the death-rate from consumption among Massachusetts females had been steadily falling for five years, the announcement included the proposal that as women had been using cycles for about that length of time, the decline of consumption could be attributed to the invigorating respiratory exercise of wheeling.[12] By the end of the decade it could be truthfully said that "it will soon be difficult to mention an ailment whose victims, provided they are not bedridden, may not, in somebody's opinion, derive benefit from the use of the bicycle."[13] Indeed, the excitement with which some doctors and hygienists touted "the new remedy, 'Bicycle,' " knew no bounds. It should be put into the "Materia Medica at the head of the 'Ferrum' preparations," urged one; it is "a preparation of steel and rubber . . . far more beneficient in its health and life giving properties than all the pills and potions ever invented," eulogized another; one could "throw physic to the dogs," all the enthusiasts concluded, for "the bicycle has done more for the good of the human race than all the medicines compounded since the days of Hippocrates . . . No, it seems hard, but we feel that the doctor's occupation is gone."[14]

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[7] T. N Gray, quoted in Luther Porter, *Cycling for Health and Pleasure* (New York, 1895), p. 14; "Some thoughts on the hygiene of the wheel" (n. 5), p 91, Seneca Egbert, "The bicycle in its relation to the physician," *Univ Med Mag.*, 1892-3, *5* 104-9, p. 104.

[8] Henry Garrigues, "Woman and the bicycle," *Forum* 1896, *20* 578-87, p. 584, T. N. Gray (n. 7), p. 14.

[9] Benjamin Ward Richardson, *Vita Medica* (London, 1897), p 247, quotation of C Lombroso, "The criminal anthropology of the bicycle," *Brit. Med J*, 1900 (i), 859.

[10] J James, "The beneficial effects of cycling as an orthopaedic agent," *Brit. Med J*, 1896 (ii), 947-8, p. 947.

But there was one affliction in particular for which the panacea seemed designed. As if by providence, the wheel had been introduced soon after the emergence of the disease of technologically advanced countries—neurasthenia, the nervous exhaustion supposedly brought on by the high tension brain work and frenzied activity of modern professional life. A cycling physician spoke for his profession as a whole when he lamented that Americans, especially, "are fast becoming, if we are not now, a race of nervous dyspeptics; in fact, many amount to little more than a rather unenviable . . . mass of organized nervousness." In recommending the bicycle as the proper remedy for this miserable condition, the author flirted with paradox by stressing the wheel's congeniality with the hurried American way of life: "a large amount of exercise can be obtained in a short space of time, and . . . it gives the idea of doing something with a rush."[15] Other writers were more apt to emphasize the escape from mental activity and worry afforded by cycling:

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[11] E B Turner, "A report on cycling in health and disease," *Brit. Med. J* , 1896 (ii), 38-9, p 38

[12] "The bicycle and phthisis," *Med News*, 1897, *71* 535

[13] "The bicycle for persons with hernia,"' *N Y Med. J* , 1899, *69* 606

[14] G S Hull, "Physicians and the bicycle," *Outing*, 1883-4, *3* 57-8, p 58, W T. Parker, "A new remedy," *ibid* , 59-60, p. 59, "For and against the bicycle," *J Hyg Herald Health*, 1896, *46* 304-5, p 305

[15] "Some thoughts on the hygiene of the wheel" (n 5), p 92 Also see Graeme Hammond, "The bicycle in the treatment of nervous diseases," *J. Nervous Mental Dis* , 1892, *10* 36-46, and F Fry, "The bicycle and the nerves," *Med Mirror*, 1896, *7* 272-4

I have the wheel of life;
Soiled with my city's dust,
From the struggle and the strife
Of the narrow street I fly
To the road's felicity,
To clear me from the frown
Of the moody toil of town.[16]

Apparently, with respect to the nervous system, the wheel could be either a tonic or a sedative, accomodating itself to the rider's needs.

In either event, cycling freed the individual from any craving for artificial stimulants or narcotics. The exercise itself was stimulus enough, and even were it not, all but the novice wheelman knew that "riding requires a steady head [and] *no drunken man can maintain his balance*."[17] So incompatible was smoking with steady cycling, that America's consumption of cigars fell, according to 1896 industry estimates, by one million a day.[18] Even morphine habitués soon "discovered that a long spin in the fresh air on a cycle induces sweet sleep better than their favourite drug."[19] Cycling optimists were confident that opiate addiction was retreating before the wheel.

Such intimations of moral elevation through physical hygiene reflect what Stevenson has described as the "spiritual sanitation"[20] of nineteenth-century sanitary reformers. For example, Benjamin Ward Richardson (perhaps the wheel's most ardent champion) considered the production of human "felicity" to be the ultimate object of sanitary science, and regarded physical exercise as an important means to that end. "We of all men," he harangued fellow sanitarians, "ought to see how to combine the physical with the moral, and to understand the interdependence of the one on the other."[21] By the last quarter of the century, the precepts of the sanitary philosophy had pervaded the literate public mind so thoroughly that laymen were as quick as physicians to perceive extra-physical benefits from bicycling. Thus while it seemed indeed unfortunate that weekend cycling cut deeply into church attendance, it was futile for preachers to conjure visions of Sunday cyclists speeding uncontrollably downhill toward a "place where there is no mud on the streets because of its high temperature."[22] Disciples of the wheel could not believe that brimstone awaited them for their faith in a machine that offered such opportunities for self-improvement. "In no other form of exercise," the typical apology began, "is there such a chance for good comradeship. From this comradeship grow a kindly expansiveness, a friendly enthusiasm, remarkable and pleasant to contemplate. There is no telling how much active moral force this expansive enthusiasm may in time generate."[23] Expanded friendliness would be complemented by enlarged mentality. The intellectual challenges of bicycle touring—the reading of maps, the introduction to new places and people, the study of the history of areas visited—"makes each rider a Columbus," while developing his faculties of understanding and toleration.[24] As the only physical exercise husband, wife, and children could conveniently take together, cycling strengthened "the whole family, [making it] one as it perhaps never was before."[25] At the same time, it educated and moved people to civic and political responsibility, because of "something pecularly democratic and independent about" it. So inexpensive as to be open to all, encouraging both cooperation and private initiative, and not requiring the use of servants, the wheel made "democracy [take] a newer and more vigorous form; no longer the closet work of the student, or the idling of the voluptuary, or the intense but intermittent strain of the professional, but the healthy, regular, natural work of an American who understands that acting is living, and healthy action healthy life."[26]

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[16] "The wheel," *Wheelman*, 1882-3, *1* 18

[17] C A. Kinch, "The bicycle and tricycle for physicians and patients," *Wheelman*, 1883, *2*: 361-3, p 362

[18] Bishop (n. 4), p 686

[19] "Cycling versus morphine," *Brit Med J.*, 1895 (ii), p 1371.

[20] L G. Stevenson, "Science down the drain On the hostility of certain sanitarians to animal experimentation, bacteriology and immunology," *Bull Hist. Med*, 1955, *29*: 1-26, p 9

[21] Richardson, "Felicity as a sanitary research," *Asclepiad*, 1884, *1*. 62-90, p. 65

Examples might be multiplied, but the visionary bent of cycling enthusiasts is sufficiently clear. In their search for virtues in the wheel, no possibilities were left unclaimed. The bicycle ultimately appeared as the vehicle for the return to the Golden Age. The glory of Greek civilization, a representative zealot preached, had been its union of mental and spiritual aspiration with physical striving. This organic wholeness had disappeared with the Greeks, and had become practically despised by Americans in recent years. But restoration was at hand: "All we [can] ask is done by the bicycle.''[27] It was not merely romantic sentiment that inclined cyclists to so frequently describe themselves as ''knights of the wheel,'' but more substantially a genuine feeling that they were comrades-in-arms rescuing a civilization in distress.

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[22] Quoted in Bishop (n 4), p 683

[23] J G Speed, "The bicycling era," *Sci Am* , 1895, *73* 124

[24] "Bicycle riding for women," *Outlook*, 1895, *51* 1104

[25] Bishop (n 4), p 685, also see "A woman on bicycle-riding for women," *Med Rec* , 1895, *47* 159-60

[26] "Democratic sport," *Outing*, 1884, *4* 222-223

Good morning, fellow-wheelmen,—here's a warm, fraternal hand,
As, with a rush of victory, we sweep across the land! . . .
For we are pure philanthopists,—
Unqualified philanthropists;
And would not have this happiness to any one denied;
We claim a great utility that daily must increase;
We claim from inactivity a sensible release;
A constant mental, physical, and moral help we feel,
That bids us turn enthusiasts, and cry, ''God bless the wheel!''[28]

* * *

This glorious prospect was, however, threatened by one great, lowering cloud. Cycling was *so* enjoyable as to be potentially its own undoing. The excitement of hurtling over the ground, the challenge of going even faster, and the assurance that all that exertion was a helpful purge for body and mind, conspired to spur riders beyond reasonable limits of speed and distance. ''Century runs'' (non-stop rides of one hundred miles or more) became a test of manhood second only to that of ''scorching'' (''we may define the term as an impulse over-ruling the cyclist's reason compelling him to overtake any and every moving object which may be in front of him'').[29] Arrests for ''furious riding'' rose dramatically once the ''scorcher'' perfected his technique, magistrates accompanying fines with warnings that a speed in excess of six miles per hour ''is not safe in the streets of a large town.''[30] The scorcher nevertheless continued ''to slay miles and pedestrians,'' excite public outrage, and force increases in life and accident insurance premiums for all cyclists.[31] But however abhorrent the scorcher, the medical profession could not ignore the injuries cyclists inflicted upon themselves. Initially, concern was directed almost entirely at the damages from accidents. There was, of course, much inevitable bruising in learning to mount and steer the machine. The exquisitely battered legs of H. G. Wells' novice wheelman, Mr. Hoopdriver, was the fee that all paid to enter

[27] H. H M , ''Greek vs modern physical culture,'' *Outing*, 1883-4, *3*, 211-6, p 216

[28] Will Carleton, ''Wheelman's song,'' *Outing*, 1884, *4* 5

[29] ''The dangers of cycling,'' *Lancet*, 1896 (ii), 133-4, p 133

[30] ''A tricycle ride,'' *Brit. Med J* , 1882 (ii), 825

[31] ''Why scorchers scorch,'' *Med Rec.*, 1899, *55* 662, ''Bicycling and insurance,'' *Brit Med. J* , 1896 (i), 1244

the world of cycling.[32] But even masters of the wheel were subject to losing control on crowded city streets, and collisions with other cycles, wagons, horses, and lampposts placed a new heading—"Death by the Wheel"—in obituary columns. And if most accidents were not fatal, they were at least frequent. New York and London averaged more than one recorded cycling accident per day in the 1890s, and it was estimated that only one-fifth of all accidents were reported.[33]

The fall and the collision, however, were hazards that were obvious to all before adopting the sport, and the liberal opinion was that the informed individual should be permitted to risk limbs and life if he chose. As the novelty of the bicycle wore thinner, the interest of physicians rapidly shifted from the subject of acute injuries to that of chronic ailments, and considerations of posture, pressure, exertion and excitement created a new medical category—"diseases of cycling." The wheel, it came to be seen, was a double-edged instrument which might cut through debility caused by slothfulness only to produce a new debility from over-exertion. Virtually everything cited as a benefit by advocates of the bicycle might be turned around and shown to be injurious by opponents. In America, physicians were still trying to catch up with the medical implications of the post-Civil War physical training boom.[34] Gymnastics, baseball, football, lawn tennis, and finally cycling had roused the public to unprecented levels of physical effort, and the long term effects of repeated stimulation of the heart, lungs, and nervous system were anything but clear. It was only natural, considering the newness of mass athletics, to err on the side of safety and find danger where a later, more experienced and sophisticated generation would see only invigoration. At any rate, the danger demanded evaluation, and from 1891 on, the subject of the hygiene and pathology of the wheel attracted a swarm of papers, letters, and editorials to medical journals. France's Académie de Médecine devoted a good portion of its time and energy in 1894 to opening up the question of the bicycle and health. In 1896 the *British Medical Journal* published a special, ten-part "Report on Cycling in Health and Disease." At least a half-dozen authors explored the subject at book-length.[35]

As suggested by the accompanying cartoon (Fig. 1), the obvious place

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[32] H G Wells, *The Wheels of Chance* (London 1901), pp 8-9

[33] "Cyclomania," *Living Age*, 1897, *215* 470-2, p 470, "Editor's table," *Sanitarian*, 1897, *39* 176, "Cycling and vitality," *Lancet*, 1892 (ii), 501-2, p 502

[34] *Report of Commissioner of Education, 1897-98* (Washington, 1899), pp 549-57

[35] "The French academy of medicine on cycling," *Lancet*, 1894 (ii), 643, E. B Turner, "A report on cycling in health and disease," *Brit. Med. J*, 1896 (i), 1158, 1211-2, 1336-7, 1399, 1510, 1564, 1896 (ii),

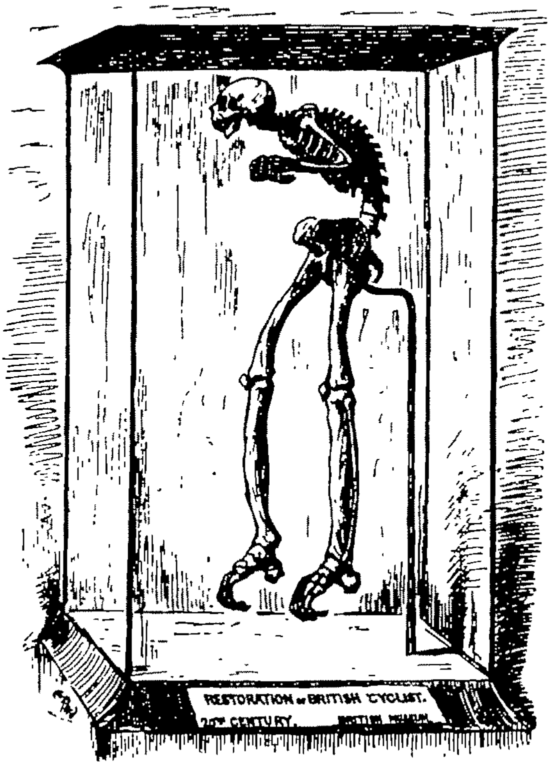
RESTORATION of BRITISH CYCLIST.
20th CENTURY. BRITISH MUSEUM.

Fig. 1. A Warning to Enthusiasts. From *Punch*, July 6, 1889, 97: 5.

for cycling damage to begin was with the skeleton. The opinion was already strong that sports or exercises which concentrated muscular effort in a localized region of the body, and/or required positioning or movements not naturally practiced, were productive of musculo-skeletal deformities. Curvature of the spine was supposedly commonplace among gymnasts, fencers, and jockeys, and since the scorcher so studiously imitated the jockey, "doubling himself up like a capital C," he should be expected to suffer the same skeletal distortion.[36] "Kyphosis bicyclistarum" was the technical name given this special condition, but it was familiarly known as "cyclist's figure," "cyclist's spine," and "cyclist's stoop." And not

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38-9, 98-9, 203, 469-70; Benjamin Ward Richardson, *The Tricycle in Relation to Health and Recreation*, (London, 1885); Oscar Jennings, *Cycling and Health* (London, 1890); Luther Porter (n 7), O Laurent, *L'hygiene du Cycliste* (Brussels, 1897)

[36] Fernand Lagrange, *Physiology of Bodily Exercise* (New York, 1898), pp. 314-26; "The bicycler should sit erect," *Med News*, 1893, *63*: 613

only did it afflict the wheelman of the present, transforming him into "Round-Shouldered Robbie, the Rough Rider of the Road;"[37] its possible hereditary transmission treatened the future health of the race. With so many young married couples taking to the wheel, it seemed surely but "a figure of speech [to refer to] the rising generation."[38]

To forestall such a turn, two changes were required. First, adult cyclists had to be educated as to correct posture, and exhorted to adopt it in place of the injurious position of the scorcher. Manufacturers might hasten the conversion, it was hoped, by the invention of "a 'health bicycle' which could only be propelled by a person who sits erect."[39]

At the same time adults were being pulled back to the perpendicular, children would have to be lifted off their wheels altogether, or at least restricted in their activity. The craze for cycling had driven many parents to train their children in the sport at the earliest possible age, sometimes getting them started before the second birthday. The possible effects of the exercise on such young bodies, on soft bones and undeveloped ligaments, appalled even the proponents of wheeling. Ensuing discussion resulted in a broad range of recommendations for the proper age at which to begin cycling, from seven to twenty-one, but all agreed that a rider below the age of twenty-one should never attempt feats of unusual speed or distance lest his growing frame be stricken by kyphosis bicyclistarum or other ills.[40]

On the question of injury to the mature skeleton, however, there was nothing like this consensus. Most physicians thought the scorching posture unnatural, but some defended it as the best position for producing motion with a minimum of effort.[41] An article in the prestigious *Lancet* announced as early as 1893 that kyphosis bicyclistarum "has already declared itself in many wheelmen,"[42] yet two years later the American physician Graeme Hammond examined fourteen *professional* cyclists, men who spent much of their lives in the scorcher's "bend," and could find no evidence of spinal deformity in any of them. "If any such condition has ever been observed," he concluded, "the number of cases of it must be very few indeed."[43] The historian is inclined to agree with Hammond, not just with respect to kyphosis, but as regards nearly all the cycling ailments discovered in the '90s. The period's physicians were generally anxious about the unnatural positions and exertions of the new sports, and whenever an abnormality was found in a cyclist, *post hoc* reasoning easily led to the placing of blame, rightly or wrongly, on his pastime. It seems true that the actual cases of alleged kyphosis bicyclistarum were "very few indeed," but a few reports in reputable journals were sufficient to suggest a much greater frequency. Reinforcement of these with a few reports each of other cycling complaints could soon generate a frame of mind which directed the doctor to consider the possibility of the bicycle as an etiological agent in all his cases. When the ease of rationalizing pathology by the peculiar positions and motions of cycling is considered, an epidemic of wheeling "diseases" becomes almost predictable.

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[37] John Davison, "Is bicycle riding conducive to impotency?" *Trans. Colo. State Med. Soc.*, 1895, 225-9, p. 228.

[38] "Kyphosis bicyclistarum," *Lancet*, 1893 (i), 1397-8, p. 1398. Also see B. W. Richardson, 'Cycling and physique," *Asclepiad*, 1890, *7*: 21-39.

[39] "Bicycling and its physical results," *Med. Rec.*, 1893, *43*: 500.

[40] "Bicycling for children," *Pediatrics*, 1899, *7*: 357-9; Richardson (n. 38), p. 22; E. B. Turner, "A report on cycling in health and disease," *Brit. Med. J.*, 1896 (i), 1337.

[41] H. Campbell, "A note on cycling," *Lancet*, 1897 (ii), 1279; A. Gubb, "Precepts for cyclists," *Lancet*, 1895 (ii), 942.

[42] "Kyphosis bicyclistarum" (n. 38), p. 1397.

Thus even Richardson feared that the heavy leg work of cycling developed one set of muscles at the expense of others, and a new race of youth with bulging calves and thighs topped by scrawny chests and arms might appear. At least there could be no surprise at the "indifferent gait" acquired by "persistent riders" whose "pedestrian powers" had been altered by this muscular distortion. Finally, the diversion of so much nervous force to the lower extremities must deprive the rest of the body of normal stimulus and incline it toward general systemic enfeeblement.[44]

Less reluctant critics of the wheel cited more specific internal injuries. By the mid-1890s, for example, many victims of appendicitis happened also to be bicyclists, and anatomical genius was hardly required to see that strenuous wheeling, particularly riding uphill, might twist the appendix over the edge of the contracted *psoas magnus,* contuse the appendix and open it to infective agents.[45] Similarly, the cyclist bent forward over his wheel had his abdominal contents pressed against the inguinal abdominal wall, and inguinal hernia might be expected as an easy product of pedaling strain. This injury was being charged to the bicycle by the early 1880s, though the specific entity "bicycle hernia" seems not to have been proposed until 1896.[46] Throughout the period, cycling advocates disputed the claim, some arguing that men already afflicted with rupture might safely ride (and without a truss!), others going so far as to advocate cycling as a cure for hernia.[47]

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[43] Graeme Hammond, "The influence of the bicycle in health and disease," *Med. Rec.*, 1895, *47*: 129-33, p 130

[44] Richardson (n. 38), p 33, *ibid.*, "What to avoid in cycling, ' *N Am Rev* , 1895, *161* 177-86, pp. 180-1 Also see J. W Roosevelt, "A doctor's view of bicycling," *Scribner's Mag.*, 1895, *17*: 708-13, p. 710, for a denial of muscular imbalance from cycling.

[45] "Bicycling and appendicitis," *Bost Med & Surg J* , 1897, *136*: 43-4 Also "The Bicycle and the appendix," *Brit Med J.*, 1897 (I), 872.

[46] George Miel, "Bicycle hernia," *Trans Colo. State Med Soc* , 1896, 347-354

Whether the wheel caused or cured hernia mattered little in practical terms; it apparently caused so many other ailments that the removal of rupture from the list might go unnoticed. From deformity of the foot, the wheel worked its malevolence upward through the anal veins and the kidneys, to the rider's throat and face. "Cyclist's sore-throat" was the penalty for inhaling through the mouth while riding, the cold air, dust and bacteria taken in by rapid breathing combining to irritate and inflame the respiratory passages.[48] The throat discomfort, furthermore, must have contributed to the distortion of a final affliction—"bicycle face." In the words of its discoverer, this condition was "the peculiar strained, set look" produced by the "incessant tension" of maintaining balance on two-wheeled machine.

> Has anybody ever seen persons on bicycles talking and laughing and looking *jolly*, like persons engaged in any other amusement? Never, I swear . . . All their attention is given up to the road and the machine. With set faces, eyes fixed before them, and an expression either anxious, irritable, or at best stony, they pedal away, . . . Did ever pastime wear a mien so sombre?

The sequelae of "bicycle face" were general nervousness, and "headache at the back of the head, where the balancing centre is situated."[49]

"Cyclo-phobia" surely reached its peak in the excitement over bicycle face and in the announcement by a New York physician of a new *specialty*, that of giving "advice regarding the medical and surgical diseases associated with the use of the bicycle."[50] Not surprisingly, such positions were widely regarded as extremism. Every wheelman was now careful, the *Medical Record* jested, "to keep his brow smooth and his lip curled, so that no one can suggest the presence of the so-called bicycle face." *A propos* of the same topic, another medical editor predicted, "The medical man will soon have to worry about this question; hence he may deem it wise to read up on the subject. Then we may hear of the 'bicycle-face face.' "[51]

It was not from such fringe areas as bicycle face that the great majority of medical opponents of the wheel made their assault, but from more solid central ground. Three points of attack were particularly favored—those of the cardio-vascular, nervous, and genito-urinary systems. A rapid increase in the heartbeat being one of the obvious effects of cycling, concern for aged riders, or for those with undiagnosed heart disease was to be expected. But although reports of sudden death from heart failure while cycling were not unusual, acute attacks never provoked the fear that chronic injury did. Pulse rates were reported to soar as high as two hundred to two hundred and fifty during vigorous cycling, and while riders regularly survived such experiences, it was hard to imagine their hearts could carry so heavy a burden for years without suffering damage. In the context of the 1890s, the condition of "bicycle heart" was a certainty, and men were actually rejected from military service in the United States and France on the basis of this diagnosis.[52] Anginoid symptoms, palpitations, tachycardia, dilatation and hypertrophy were the frequently identified manifestations of bicycle heart, each easily understood as a consequence of over-exertion on the wheel.[53]

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[47] *Lancet*, 1879 (ii), 968, *ibid.*, 1882 (ii), pp 296, 337, 375

[48] "Cyclist's sore throat," *Lancet*, 1898 (ii), 95, *Med Rec* , 1896, *49* 358, Jennie Chandler, "Sore throat from bicycling," *J. Hyg Herald Health*, 1898, *48*. 243-4

[49] A Shadwell, "The hidden dangers of cycling," *Living Age*, 1897, *212* 827-34, p 833 Also, "A new explanation of the bicycle face," *Med Rec* , 1895, *48* 308

[50] "A medical bicycle specialist," *Med Rec* , 1895, *48* 702.

[51] "A physician on the wheel," *Med Rec* 1896, *50* 557-8, p 58, "Bicycle is king," *Med Sentinel*, 1895, *3* 402-3, p 403

No one denied that the heart required exercise to remain healthy, and proper doses of cycling were acknowledged by all to be an ideal cardiac stimulant. The difficulty, the reason for so much bicycle heart, was the refusal of cyclists to observe moderation. Their passion for motion made them deaf to medical warnings, indeed puffed them with a cavalier disregard for danger. The magazine *Wheeling,* ignoring the bicycle as a *cause* of heart disease, advocated its use for the diagnosis of *weak* heart.

> The cyclist who has doubts as to the strength of his heart may easily put the matter to the test by riding up a steep hill as far as possible, and if there is any heart weakness a sharp pain will strike him in the back, between the shoulders. If the pain does not appear, the cyclist may take it that his heart is practically sound.[54]

Clearly physicians doubtful of the heart's ability to withstand hard exercise had reason to hate the wheel.

So did those who were uneasy about the nervous system's capacity to absorb shock and strain. It was all well and good for cyclists to extoll the benefits of escape from care and tension, but did their machines truly provide such release? On the contrary, opponents of the wheel insisted, did not the bicycle impose a nervous irritation more unyielding and demanding than would ever be encountered in the counting room or study? The machine aroused, first of all, an elemental fear of falling or colliding, and though this fear might be suppressed under easy circumstances, in tight situations it could spring up and instantaneously immobilize the rider. Like stage fright, "bicycle fright" was a "perversion of determination" in which

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[52] "Cycling and heart disease," *Med Rec* , 1895, *47* 186, "Bicycle heart," *Brit. Med J.* 1898 (i), 908, A C. Getchell, "Bicycling in its relation to heart disease," *Med News*, 1899, *75*. 33-7, p. 36.

[53] For detailed presentations of the pathology of bicycle heart, see B W. Richardson, "Cycling and heart disease," *Med Soc Trans.*, 1895, *18*. 85-103, and G. Herschell. "Cycling as a cause of heart disease," *Lancet*, 1895 (i), 540-2

[54] "The cycle as an instrument of diagnosis," *Bost Med & Surg J* , 1899, *141* 458.

> the intense and overwhelming desire to do well in the face of danger temporarily paralyzes the necessary will force and makes such a result impossible. The untrained bicyclist who wishes to avoid the vehicle approaching from an opposite direction finds himself helplessly steering toward the object instead of away from it. . . . When once attacked with this nervous malady it is almost useless to attempt to prevent its injurious tendencies. The victim can no more help himself than can the gnat resist the attraction of the flame.[55]

The resulting crash was the acute injury caused by bicycle fright, and even experienced riders were subject to it. In response, teachers of cycling instructed their students to always look away from objects to be avoided, if they would escape the "fright" and consequent collision, but the very necessity of the precaution admitted the constant lurking presence of a fright that would chew at the nervous system until it produced *chronic* damage. Bicycle face, it will be recalled, was presumed to be a manifestation of the strain of balancing the wheel, and balancing was but half the problem. Accurate steering was also required, on rough roads, down steep hills, and in thoroughfares jammed with pedestrians, equestrians, carriages and omnibuses. Negotiation of the last terrain, especially, demanded unceasing strain on the ear ("to catch every voice and sound"), the eye ("to meet every danger as well as to avoid being a cause of danger to others"), and the sense of touch. Such a tax upon the senses, even of young riders, "makes them prematurely old and unfitted for the more delicate tasks that have afterwards to be performed."[56]

Compounding the injuries from active exertion of the nervous system were the insults it received directly from the wheel. Physicians had been disturbed ever since the days of the velocipede (also called the "boneshaker") about the effects of continued jarring of the spinal cord on rough-riding machines. When patients began to complain of "seediness" following bicycle rides, the nervous nature of the symptoms—headache, sleeplessness, lassitude the following day, depression—suggested repeated concussion of the nervous system as the cause. The inventions of spring frames, suspension saddles, and pneumatic tires were all expected to overcome the "vibratory habit," yet the syndrome persisted through the 1890s. And if some doctors interpreted the condition to be an expression of fatigue, others continued to regard it as a nervous ailment and to recommend altered posture (more weight over the saddle springs) and lower tire pressure as preventatives.[57]

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[55] "The bicycle fright," *Med. Rec.*, 1896, *50*: 269-70.

[56] "Cycling and vitality," *Lancet*, 1892 (ii), 501-2, p. 501, "Cycling from the standpoint of health," *Med. Rec.*, 1897, *52*: 89-91, p. 90; Richardson (n. 44), p. 181.

Even those skeptical of the vibratory habit acknowledged the existence of what might have been called "bicycle hand." The tendency of the unskilled rider to throw his whole weight forward on his arms and grip the handlebars "as a last hope between life and death" subjected the nerves of the hands to extraordinary pressure. Further exacerbated by the vibration of the front wheel, the nerves eventually succumbed, and numbness of the fingers began to be experienced after even moderate riding.

> Following the numbness, unless something is done to prevent further progress of the disease, comes a weakness of the principal muscles of the fingers, making the act of holding anything with a tight grip almost impossible and very painful. A trembling like palsy follows this stage, and the sufferer ends by losing the use of the member entirely.[58]

Again the erect riding position, which transferred weight from the handlebars to the saddle, was advised as prevention and cure, but to some observers this seemed to transfer the seat of the malady as well. The saddle might impose pressure as effectively as the handlebars, and then the physician might find himself "face to face with a new disorder, which for want of a better name [he] dubbed cyclist's neurosis."[59] Neurosis or neuralgia were preferred descriptions of the perineal injuries of cycling, for it was conceived that the standard bicycle saddle, adapting itself through use to the curve of the perineum, must press steadily against the nerves of the pubic region. With so many structures involved, the symptoms of cyclist's neurosis could range widely. The fortunate wheelman might escape with only a numbness in the toes, but too often the damage was more localized and inconvenient. Tenderness of the perineum and testicles was the standard complaint, though pain about the anus, constipation and retention of urine were common as well. A physician who suffered the neurosis reported difficulty "in getting rid of the last few drops" of urine, so that an "uncomfortable trickle" down the leg marked the condition.[60] He also experienced "shrivelling and aching" of the penis due to nerve pressure, though it was much more common for anesthesia of the male organ to result.

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[57] E B. Turner, "A report on cycling in health and disease," *Brit Med J*, 1896 (i), 1211-2, James Prendergast, "The bicycle for women," *Am. J Obstet*, 1896, *34* 245-263, p 252, S A K Strahan, "Bicycle riding and perineal pressure, their effect on the young," *Lancet*, 1884 (ii), 490-1, p 490

[58] *N Y Times*, Aug 4, 1895, p. 14 Also, J. C. Minor, "May engender prejudice in bicycling," *J A.M.A*, 1897, *29*. 195, "Paralysis of the forearm from bicycling," *Lancet*, 1897 (i), 1355

[59] W H Brown, "A form of neuralgia occurring in cyclists," *Brit Med J*, 1898 (i), 553. Also see Arthur Roper, "Perineal pressure in cycling," *Lancet*, 1896 (i), 1341-3, and Edward Semple and James Taylor, "On certain symptoms of spinal cord affection in bicycle riders," *Lancet*, 1897 (i), 1084.

Saddle pressure was implicated in a number of other ailments, though on these physicians were divided or unsure of the mechanism of injury, sometimes staying with nerve interference, other times blaming direct pressing and jarring of the organ involved—"pommeling of the perineum," as one described it.[61] The incidence of strictured urethra apparently rose noticeably as early as the high-wheeled "ordinary" days, and as the popularity of the bicycle mushroomed, so did the number of cases of urethritis, prostatis, and cystitis.[62] At least one loser in the battle of "Bicycle vs. Bladder" was reduced to such unceasing agony that he shot himself, confiding to his wife before he died that, "That bicycle brought me to this terrible end, yes it did."[63]

While the urethra, prostate, and bladder seemed to take the brunt of the saddle's punishment, other organs were also believed to be affected to some degree. Hemorrhoids, ischio-rectal abscesses, and malignant testicular tumors, for example, were reported as wheel induced.[64] There were also instances of priapism amoung cyclists, and although these were properly interpreted as the effect of impeded venous flow (rather than a sign of sexual arousal), erections in the saddle pointed to the unavoidable question of cycling as an aphrodisiac.[65] The predictable answer was that the bicycle saddle, by rubbing and bouncing the perineum, must "call attention to the organs of generation, and so lead to a great increase in masturbation in the timid, to early sexual indulgence in the more venturous, and ultimately to early impotence in both."[66] But surprisingly, for this period when there persisted so much uneasiness about sexual indulgence, both solitary and social, a majority of the medical profession seem to have regarded the wheel as an anaphrodisiac. "As it is well-known," a Denver physician explained,

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[60] Roper (n. 59), p. 1341

[61] John Robinson, "Bicycle urethritis," *Med News*, 1898, *73* 428-9, p 428

[62] For representative discussions, see "Cycling and traumatic urethritis," *Med Rec*, 1898, *53* 950, and William Townsend, "Bicycle-riding upon improperly fitting saddles," *N Y Med J*, 1896, *63* 243

[63] G H Heard, "Bicycle vs bladder," *Med World*, 1898, *16*· 495

[64] "The pathology of bicycling," *Bost Med & Surg J*, 1894, *130*. 197-8; "Cycling and ischio-rectal abscess," *Brit. Med J.*, 1896 (i), 1179, "Bicycle and malignant testicular tumor," *Med News*, 1901 *78* 684

[65] "The effects of bicycle riding on the perineum," *Med Rec*, 1897, *51* 484, "The pathology of bicycling" (n. 64), p. 197.

> that certain debased individuals resort to the stimulation of any of the parts mentioned to provoke and maintain the venereal cycle, resulting in due time to complete lack of response to any stimulus however intense, analogy of reasoning prompts us to believe that the bicycle stimuli produce in time an exactly similar condition of affairs.[67]

By whichever route—stimulation to venereal excess or depletion of nervous capacity—the wheelman's destination was the same: impotence. Since the same condition might also result from damage to the prostate or bladder, there is double meaning to the statement that sexual exhaustion completed the list of infirmities of the saddle. It was not merely the final addition to the list, but also the state to which other perineal injuries might lead. Whether or not they resulted in impotency, however, the number and range of these injuries was sufficient to back up the advice of Albert Gihon, retired Medical Director of the U.S. Navy. Because of the wheel, Gihon asserted, "I can give no more significant hint to the young man choosing his field than to make the perineum and its adjacencies the subject of his especial study."[68]

Unfortunately for any medical students who accepted Gihon's counsel, the budding specialty was already being nipped by the bicycle manufacturers. Responding to the medical demand for a saddle which would not pressure the perineum, manufacturers were selling wheels with a wide variety of "Safety," "Hygienic," and "Anatomical" saddles by the mid-1890s. The design of these seems to have been carried out by mechanics, without medical consultation, and it is necessary to question the accuracy of descriptions like "Anatomical" upon learning that at least one cycling mechanic desired a new style saddle because his old one kept "bruising my sternum bone."[69] Nevertheless, the multiplicity of saddle designs was such that some were bound to meet with medical approval. The style that emerged with the highest praise was the one which combined a widened back portion, to better accommodate the ischial tuberosites (which should properly bear the weight) with a slit through the peak or front section. When correctly slit, the front of the saddle assumed a concave form under the rider, exerted no pressure against the body, and freed him from "perineal servitude."[70]

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[66] Strahan (n. 57), p 490. Also see William Bodenhammer, 'Equitation and bicycling," *N Y. Med J.*, 1895, *62*: 554-9

[67] Davison (n. 37), pp 226-7. Also see George Batten, letter to the editor, *Lancet*, 1884 (ii), 708-9

[68] Quoted in "Editor's table," *Sanitarian*, 1896, *37*: 353-8, p 355.

[69] "Injurious bicycle saddle," *Med. Rec* , 1899, *56*: 395

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Yet the new saddles did not immediately free the medical profession of their perineal anxiety. To be sure, concern for the wheel*man*'s genito-urinary system all but disappeared by 1900, and even by 1898 physicians could chuckle about the "time after the advent of the modern bicycle [when] the male perineum, as a medical and surgical subject of discussion, bade fair to rival its analogue of gynecologic and obstetric fame."[71] But at the same time, that analogue, the female perineum, the object of such extraordinary medical solicitude throughout the Victorian era, was being recognized as the most vulnerable prey of all for the insatiable wheel. A new creature had been spawned by the cycling craze, the wheel*woman*, and her presumed weaker constitution and more impressionable nervous system insured she would excite a level of concern never attainable by the male cyclist.

It was not an easy ascent to such notoriety, for women were actively discouraged from attempting the wheel. The epithet "unladylike" was, of course, the first obstacle to be overcome, but more formidable, probably, was the supposition of physical incapability. An early pitch man for "Bicycle" as the panacea "regretted that the prescription is for men only," though he generously allowed that "possibly" the day would come when females might ride.[72] He surely was aware that a few adventurous women had already mounted tricycles, but that they might advance to the much more demanding and hazardous two-wheeler was unthinkable to most. "Girls cannot ride the bicycle," another wheelman pontificated. "That is undeniable."[73]

The only thing more difficult to deny, apparently, was the will of the "new woman" of the day to emulate masculine accomplishments. She had to parry moralistic charges that "the bicycle may be the first step . . .

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[70] "Cycling for elderly men," *Brit Med J* , 1895 (ii), 561 Also see "An improvement in cycle seats," *Lancet*, 1893 (ii), 1193

[71] Robinson (n 61), p. 428.

[72] Parker (n. 14), p 59

[73] H H M (n 27), p 216

towards a sportive life,'' that cycling gave her ''horsey airs,'' or that she suffered from ''actual dowdiness.''[74] She had, further, to endure the same anxious and pain-filled learning period as stronger male riders, but she persevered. By 1893, no up-to-date woman with self-respect would be caught pedaling more than two wheels.

After the shock subsided, early reports were largely in the wheelwoman's favor. However repelled many were by the sight of a female on a bicycle, all agreed that the female in the home had become at least as pitiable a creature. Confined indoors, burdened with petticoats, denied any exercise more arduous than fainting, the American woman as semi-invalid had become a stereotype by the 1880s. And though previous decades had been filled with health reform efforts in their behalf, women had been frustratingly coy, refusing to display real feeling for any hygienic activity. Women, like men, would respond warmly only to exercise which appeared as pleasure instead of duty. ''No amount of dress-reform preaching or of calisthenic exercises,'' a Boston physician recognized, ''will . . . awaken the woman'' to her need of increased exercise and light, loose clothing. ''This is what the bicycle is doing, and is destined to do in the future.''[75] In precisely the same way it had been hailed as the savior of man from the dissipations of alcohol, tobacco and opium, the bicycle seemed to be the rescuer of woman from her special ills—oppressive clothing, inactivity, and nervous-reproductive weakness. The wheel simply could not be managed by a rider clad in a tightly laced corset, long, heavy skirt, and high heeled shoes. Appeals for rational dress, unheeded over the previous half century, suddenly found an audience as women discovered the incompatibility of proper pedaling and breathing with a whalebone-bound torso.[76] While many heroically tried to endure in the old fashion for a while, as many more discarded their corsets and billowing skirts in favor of equestrian tights and leggings, or even bloomers. Female health was the undoubted beneficiary of the transaction.

There obviously was equal benefit in the invigoration of wheeling in the open air, but cycling might be seen to confer special rewards beyond those of general bodily exercise. The clearest presentation of these was given by the New York obstetrician Robert Dickinson. From the observation that most of the work of cycling is done by the legs, he advanced the argument that the adjacent area of the pelvis—its vessels, muscles, and organs—would share in the stimulus and increased blood flow during the exercise, and gradually develop a greater tone. With improved tone in the pelvic floor should come relief from the all too prevalent female complaints of chronic catarrhs, varicose vessels, hypersensitive local nerves, and other chronic pelvic disorders. Lack of pelvic tone, he submitted, was the cause of women mistakenly thinking that "God intended [them] to suffer during menstration and labor."[77] As the best single exercise for strengthening the pelvis, bicycling, Dickinson concluded, was woman's best hope for restoration to reproductive normalcy. He even included the case histories of eight patients suffering from a variety of female disorders, all of whom had improved after following his prescription of the wheel.

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[74] ''Woman and the bicycle,'' *Bost Med & Surg J*, 1894, *131* 247, ''A plea for the new woman and the bicycle,'' *Am J Obstet*, *1896*, *33*. 575-6, p. 575, ''Cycling for women,'' *J Hyg Herald Health*, 1893, *43* 165

[75] Charles Townsend, ''Bicycling for women,'' *Bost Med & Surg. J*, 1895, *132* 593-5, p 594 Also see F S Nash, ''A plea for the new woman and the bicycle,'' *Am J Obstet*, 1896, *33* 556-60

[76] ''The influence of cycling on woman's dress,'' *Pediatrics*, 1897, *4* 78-9.

The numerous physicians who joined Dickinson in citing the pelvic profits of cycling, furthermore, rarely did so without also claiming it to be beneficial in the other great class of female complaints—nervous ailments. Unhygienic living, the *ennui* of home life, and the pressures of maintaining social standing had reduced women to "mere bundles of nerve fibres, ready to explode on the slightest provocation."[78] What they needed was some means of releasing accumulated energy and tension, transportation away from "the place of her worry and work," and "an exhilaration that takes the mind off those things that madden and drive them insane."[79] What they needed was the bicycle, as demonstrated by reporters such as "Psyche": "A friend of ours, who was afflicted with nerves, fainted on any reasonable or unreasonable opportunity, wept quarts in the month, and wished she was dead" was coaxed to try the wheel. After less than a year of cycling, "she cries yet sometimes (all girls do); but her nerves give her no more trouble; she has grown fifteen pounds heavier; is bright and jolly, and as enthusiastic a wheel missionary as anyone ever was or will be."[80]

That degree of enthusiasm was no mean achievement, for "Psyche" and her friend were but two of many women, and men, who regarded the question of woman and the wheel in an aggressively optimistic light. Even in the years before women graduated from the tricycle, it could be perceived that their "right to a healthy body is on the way to full recognition," and that the tricycle "is a direct power in making the health and happiness of the world."[81] Nor did the transition from three to two wheels diminish this confidence. An English physician concluded by 1896 that bicycling for women "was one of the greatest boons which had been conferred upon the sex in the present age. Morally, mentally, and physically . . . it [should] improve not only the individual, but the race generally."[82]

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[77] Dickinson, "Bicycling for women from the standpoint of the gynecologist," *Am J Obstet*, 1895 *31* 24-37, p. 25.

[78] Prendergast (n 57), p 245 The nervousness of late nineteenth century American women has been analyzed by John and Robin Haller, *The Physician and Sexuality in Victorian America* (Urbana, Ill , University of Illinois Press, 1974), pp. 24-42

[79] "Editor's table" (n 68), p 358. W. H Fenton, "A medical view of cycling for ladies," *Nineteenth Cent* , 1896, *39* 796-801, p 800

[80] Quoted in Porter (n 7), pp 17-8

That same year, Charlotte Smith, president of the Women's Rescue League, launched an "anti-bicycle crusade" to save women from the physical and moral insults of the wheel. In her attack, and those of others to be discussed below, there is displayed again the Janus-like aspect of the bicycle that allowed it to appear benevolent from one side and malicious from the other. Charlotte Smith's loathing for the bicycle was basically visceral, the anger of a traditionalist offended by new ways. "All this pure-air-of-heaven racket," she confided, "makes me weary of life. I have never yet seen a woman who looked well on the bicycle, and as for riding being good for the complexion, I take no stock in that excuse whatever."[83] And even though she considered immorality a worse effect of cycling than ill health, she still spiced her condemnation with phrases like "weakening [of] the back," "disorders of the system," and "physical wreck."

The path of the Smith crusade was to be a rocky one, but not because of lack of support for her hygienic charges. A goodly portion of the medical community was just as disturbed by the new female ambitiousness, and quickly uncovered a female cycling pathology. All the male conditions, *de pied en cap,* from bicycle-foot to -face, were reintroduced, and with greater urgency, since women were by nature more delicate and by upbringing unacquainted with the hardening rough and tumble of boys' sports. Consequently, "all what may be said of man has to be said in italics of woman."[84] But woman was also affected by a more significant difference from men: the possession of a special apparatus for reproduction which might be damaged not only to her own detriment, but to that of posterity as well. The injuries incurred by careless male cyclists might be regarded as largely their private affair, "but it is another matter when the welfare of that sex is concerned upon which depends the welfare of the race."[85]

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[81] Minna Smith, "The tricycle for American women," *Outing*, 1884-5, *5*: 423-6, p. 423.

[82] "The sanitary aspect of cycling for ladies," *Brit. Med. J.*, 1896 (i), 681

[83] "For and against the bicycle" (n. 14), p 304

[84] Harry Cushing, "The moderns awheel," *Harper's W*, 1896, *40* 353.

Warnings about the undermining of racial welfare by the wheel were concentrated on two possibilities. It was immediately observed that the movements involved in pedaling a bicycle were similar in appearance to the motions of running a sewing machine, an activity already condemned by the medical profession as injurious to health, and particularly to the health of the pelvic organs. Closer analysis quickly removed the objection by noting that sewing "exercise" was taken indoors, with the corseted body constantly bent forward to maintain vision of the needle, and with only the lower legs actually being moved. When the monotony and long hours of sewing were added, it seemed absurd to compare it to cycling. Yet defenders of the wheel felt compelled to repeatedly point out this absurdity. A Wisconsin physician was still complaining in 1897 of being frequently asked why he criticized the sewing machine but not the bicycle.[86]

Of much greater concern than the wheelwoman's motions was her position, specifically her straddling of the dreadful saddle. As the point of contact between the machine and the woman's reproductive organs, it was the saddle which received the closest medical scrutiny. The modifications of saddles for the purpose of relieving perineal pressure have been related, but without emphasis that these alterations were designed for men. In fact, the manufacturers of the saddles of the early 1890s completely overlooked the broader pelves of the growing number of women riders and confidently offered to both sexes seats cut to the narrow contours of the male. As late as 1895, a Boston obstetrician surveyed the situation and found "that no serious attempt has as yet been made to produce a saddle that shall be adapted to a woman's anatomy."[87] Some saddles were "absolutely unfitted" for female use, and virtually all the rest required many adjustments to be adapted to women riders. The chief inadequacy, he and other critics noted, was that the rear of the seat was not wide enough to support the average woman's ischial tuberosities, so that she sank and rested instead on her perineum.

To physicians already worried about bicycle urethritis and bicycle prostatitis, the possibilities were alarming. Uterine displacements, distorted pelvic bones, hardened perinea which resisted parturition, and contracted birth canals might all be expected. The pelvic outlet could also be diminished by hypertrophy of the muscles of the region (produced by pedaling). Even the occasional woman fortunate enough to find a wide saddle might have her ischium pushed upwards and inwards, and her birth canal narrowed.[88] The fear of such bogeys was sufficient to inspire the invention of the "Komphy," an elaborate perineal pad designed for ladies to wear beneath their cycle clothing.[89]

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[85] "Editor's table" (n 68), p. 355

[86] J Mills, "The use and abuse of the bicycle," *J A M A*, 1897, *29* 275-6, p. 276 Also, Anna Galbraith, *Hygiene and Physical Culture for Women* (New York, 1895), pp 87-8, and Dickinson (n 77), pp 27-30

[87] James Chadwick, "Bicycle saddles for women," *Bost Med & Surg J*, 1895, *132* 595-6, p 595 Robert Dickinson, "Bicycling for women," *Outlook*, 1896, *53* 550-3, and Prendergast (n. 57), pp 249-50

The Hallers have analyzed American physicians' opposition to cycling as basically moralistic, stemming from fear for the modesty and delicacy of women abducted by the wheel from the protection and responsibilities of the home.[90] It seems undeniable that uneasiness about the social implications of feminism fed doctors' suspicions of a physical pathology of cycling. The very costume of the wheelwoman, though adopted for reasons of safety and convenience, seemed to brand her a suffragette. At the least, it was immodest. A Dr. I. Love, of St. Louis, thanked the wheel for prying women out of their unhygienic corsets, but regretted the ladies had stepped into bloomers, "which lessened the respect of mankind for womanhood."[91] In the midst of his worries over the low vitality of American women, the nation's declining birthrate, and the flooding of the country with immigrants of non-Anglo-Saxon stock, the physician must have been hard put not to regard the bicycle as an instrument of indulgence on which women shirked their social and racial responsibilities. The Seattle doctor who explained why female cyclists had *not* apparently suffered the predicted pelvic damages surely struck a responsive chord with many of his medical readers.

> While bicycle riding may deform the pelvis, it will have little or no effect as regards the ease of childbirth. And why? For the simple reason that "bike" riders do not have babies. Where or who is the physician that has attended a "bike" rider in confinement? Will the female rider throw aside her wheel long enough to have a baby, let alone to rear a respectable sized family? The bicycle is a nuisance and a curse instead of a blessing. . . . The advent of this horrid means of locomotion is an instance in which the inventive genius of man has overtoppled itself.[92]

This distaste for women following mannish pursuits was publicly shared by more than one physician. Shadwell, discoverer of bicycle face, deplored the fair sex's fondness for "unsexing itself"; another observed that not only had the wheel "displaced the horse, [but] in women has, in a measure, replaced the uterus;" a third complained that women had become "wedded" to the bicycle, and as a result

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[88] T R Evans, "Harmful effects of the bicycle upon the girl's pelvis," *Am J Obstet* , 1896, *33*. 554-6, *Med Rec* , 1895, *47* 573; J McVitie, "Bicycling for women: its effects on parturition," *Lancet*, 1897 (ii), 51

[89] "A pneumatic pressure-preventing pad for cyclists," *Lancet*, 1899 (i), 1164.

[90] Haller (n. 78), pp 174-87

[91] "The bicycle from a medical stand-point," *Med Rec* , 1895, *48* 464

[92] A Simonton, "Bicycling," *J. A M A* , 1898, *31* 1253-4

> they have little desire for matrimony which they come to regard as a . . . kind of bondage . . . Consequently they prefer to remain spinsters, and a quasi third sex is formed . . . Womanhood in its physiological and motherly sense is sacrificed to narrow hips, atrophied breasts with retracted nipples, and a development of brachial, leg and abdominal muscles that would do credit to a lightweight.[93]

It was surely physicians' dismay about the bicycle's apparent moral effects on women that compelled Francis Nash to conclude his defense of the *physical* hygiene of cycling for women with the pledge that "when the New Woman comes we may be assured that she will be the same sweet, pure, lovable creature on whose shrine we have through endless ages respectfully laid our laurels."[94]

Nash's comments portray at once the doctors' dilemma and its solution. For her own, and the race's sake woman had to be emancipated from weakness, neurasthenia, and pelvic disorders, and the wheel was an admirable means of lifting her to health—except that it threatened to also remove her from the home and its familial obligations and moral insulation. Fear of the latter possibility could easily be translated into condemnation of the bicycle on medical grounds, and the wonder is not that so much pelvic damage was credited to the wheel as that so many physicians resisted the temptation. More often than not, the charges of pelvic injury were dismissed as imaginary, and sometimes with stinging commentary. "The contents of a woman's pelvis should not be jarred [by cycling]," one doctor scoffed; "better a total extirpation than a jolt."[95] The majority of the profession, whatever their feelings about the social and political liberation of women, welcomed the hygienic liberation offered by the bicycle. What they hoped was that the new female vitality would be channelled into traditional functions, and woman be made a more efficient homemaker rather than a rival of man. Even "Psyche," the self-appointed missionary of the wheel quoted previously, cherished her new found strength at least partly because it improved her sweeping and bed-making.[96]

There was a final aspect to the question of woman and the wheel, the most distasteful and nearly the most discussed, which further exhibits the physician's uncertainty about the moral-physical results of bicycling. The Hallers present masturbation as the "single issue" which dominated the minds of physicians worried about the bicycle's erosion of morality,[97] and to be sure, references to the possibility of the habit appeared with great frequency in the mid-90s, stemming from long-standing fears about masturbation in both sexes and from the apparent ease of the act for females on a cycle. It looked like self-gratification might be readily and inconspicuously achieved by tilting the saddle pommel up and pudendum down. One woman, "who claimed a rather varied experience in sexual pleasures," actually admitted "that she could not ask a more satisfactory development than could be obtained from the saddle of her bicycle."[98] But for once on a question of masturbation, common sense prevailed. Doctors did not have to probe the matter very deeply to realize that while a chance existed that some girls might discover self-excitation if a bad saddle and poor posture led to "friction and heating of the parts,"[99] in most cases the rider would have to be consciously seeking gratification. And then, why complicate the act with a bicycle? "If a girl wanted to masturbate," it was pointed out during discussion at the New York Obstetrical Society, "she would carry out her wish anyway, as well sitting on a chair as on a bicycle."[100] Indeed, many concluded, the wheel was more likely to prevent the habit than generate it. Riding required concentration and diverted the mind from prurience; it induced fatigue, and it was positively uncomfortable when converted to clitoral stimulation. Gynecologist Robert Dickinson went so far as to assert that the bicycle saddle compressed the pudic nerve in such a way as to quench desire—all but one of the wheelwomen patients who answered his inquiries testified "against sexual excitation, though one of them admits freely that she was formerly in the habit of relieving herself manually."[101] The editor of a Canadian journal probably won some nods of approval for his claim that "the consensus of opinion is increasing overwhelmingly day by day that bicycle riding produces in the female a distinct orgasm." But the weight of medical sentiment appears to have rested with the New York editor who responded that

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[93] *Med Rec*, 1898, *53*: 949.

[94] Nash (n. 75), p 560

[95] "The dangers of the bicycle," *Bost Med & Surg J*, 1894, *131* 352-4, p 354

[96] Porter (n 7), pp 21-2 Haller (n 78), pp 86-7, also takes note of the medical profession's determination that the New Woman be only an improved home economist

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[97] Haller (n. 78), p. 184.

[98] Dickinson (n. 77), p. 33.

[99] Prendergast (n. 57), p 250.

[100] "Bicycling for women from the standpoint of the gynecologist," *Am J Obstet* , 1895, *31*· 85-90, p 86.

[101] Dickinson (n. 77), p. 34, Also, "The bicycle and sexual excitation," *N Y Med. J.*, 1898, *68*: 179

> compared to Canada . . . Sodom and Gomorrah were as pure as Salvation Army shelters. It appears that cycling . . . is in Canada . . . merely a means of gratifying unholy and bestial desire. . . . Either the wheelwomen of Toronto are the vilest of their sex, or they are the victims of a contemptible slander. . . . The question of the healthfulness of cycling . . . is one that still admits of discussion; but the man who can assert or even suggest that the thousands, perhaps millions, of women throughout the world, who ride the wheel, are giving themselves over to self-abuse, puts himself beyond the reach of argument.[102]

When all was said and done, it was this kind of level-headedness which characterized the medical response to all the facets of cycling and health. There was extremism in both directions, born of the optimism of veteran sanitarians and the anxieties of a fledgling "sports medicine." Yet nearly all gravitated toward the central position which had been the essence of hygienic philosophy since Hippocratic times: temperance. Eventually the most common opinion about bicycling was that it was wholly beneficial to the person who was careful not to exceed his individual capacity, and harmful only to the one who indulged to excess. Replying to implications that the bicycle was "a machine more to be avoided than a roulette wheel," the editor of the *Boston Medical and Surgical Journal* put the matter in proper perspective.

> It is to be doubted whether such a beneficial exercise will perish because a few imprudent persons with cardiac lesions overdo themselves, or a French youth excoriates his perineum. There have been too many spindly children built up to healthy vigor, and too many chlorotic, languid girls made rosy and buxom by riding, for physicians to be easily alarmed and dissuaded from believing in "wheeling."[103]

The true difficulty, as it developed, was not in winning the support of physicians, but in maintaining the participation of the public. The great attraction of the wheel, that it made exercising enjoyable and thus stirred to hygienic activity people who would otherwise have shunned exertion, was also its great weakness. For too many cyclists, improved health was a fringe benefit of the pastime. What actually drew and held them to the wheel was the speed and mobility, and feeling of adventure and fashionableness it provided. If another diversion should appear to offer the same qualities in greater abundance, many cyclists were ready to switch with little thought of the hygienic merits of the new pursuit. The bicycle, in fact, paved the way for its own obsolescence. It created a taste for rapid private transit at a time when the public appetite preferred mechanical to man power. The "motocycle" was the inevitable supplanter of the wheel once it was made economical. By 1903, *The Lancet* could detect a significant decrease in the number of cyclists, as well as in the quality of the sport for those who had remained in it. "The noise and dust and pace of the motor-car," it was lamented, "have taken the poetry completely out of a ride on the high-road."[104] Articles on the hygiene and pathology of the wheel, once so numerous, had already become a rarity in the pages of medical, and public journals. The bicycle was being demoted to the status of a child's toy, while the automobile was coming in for its own share of hygienic praise ("With the motocycle in use, the problem of street cleaning will be greatly simplified, and the health of our cities promoted"), and criticism ("A speed of fifteen or twenty miles an hour in a motor car causes [women] acute mental suffering, nervous excitement, and circulatory disturbances").[105] That the machine expected to free humankind from foul air and idleness should only foster another machine which would pollute the atmosphere and encourage sedentariness on a grand scale was the ironic *dénouement* of an originally auspicious chapter in Victorian sanitary science.

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[102] "Immorality in Canada," *Med. Rec*, 1896, *50*: 681-2

[103] "The dangers of the bicycle" (n 95), p 354.

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[104] "Cycling and motor-cars," *Lancet*, 1903 (1), 1460

[105] S. Barton, "The evolution of the wheel—velocipede to motocycle," *Sewanee Rev* , 1897, 5 48-62, p 62; quoted in Fank Donovan, *Wheels for a Nation* (New York Crowell, 1965), pp. 8-9

# THE INTRODUCTION OF THE EDINBURGH QUIZZING SYSTEM INTO AMERICAN MEDICAL EDUCATION*

W. ROBERT PENMAN

My topic is a very elusive subject and one that has been very difficult to research and define for over one hundred years. Neither the established general historical texts on American medical education nor research sources at the College of Physicians, the National Library of Medicine or the Countway Library have any catalogued reference to the Edinburgh Quizzing System. To date, I have found no medical student notes or diaries that discuss the operating details of the "Quizzing System." My information, in general, has been derived from biographies of physicians,[1] Gould's *History of Jefferson Medical College,*[2] general texts and inaugural addresses at the College of Physicians of Philadelphia,[3] the Librarian of the Royal College of Surgeons in Edinburgh[4] and cryptic references in F. P. Henry's *Standard History of the Medical Profession in Philadelphia*[5] which Mrs. Lisabeth Holloway[6] has recently indexed. I came across reference to the existence of the System while working on the career of Charles DeLucena Meigs[7] who quarrelled and lost the childbed fever argument with Oliver Wendell Holmes in the 1850s and whose professional obituaries then carried the theme that he had helped to introduce the Edinburgh Quizzing System into American medical education. Meigs was one of those whom the late Richard Shryock[8]

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[1] Samuel D. Gross, *Autobiography of Samuel D Gross with His Reminiscences of His Times and Contemporaries,* 2 vols. (Philadelphia: W. B Saunders, 1893)

[2] George M. Gould, *The Jefferson Medical College of Pennsylvania* (New York. Lewis Publishing Co., 1904), vol. 1, p. 82.

[3] Joseph Smith, *History of Jefferson Medical College (Pittsburg, Pa. J. T Shryock, 1857).*

[4] Personal communication with Dorothy U. Wardle, Librarian, Royal College of Surgeons, Edinburgh, Scotland, August 1, 1975.

[5] F P. Henry, *Standard History of the Medical Profession of Philadelphia* (Chicago. Goodspeed Bros , 1897), p. 153.

[6] Lisabeth Holloway, former curator, Historical Collections, Library, College of Physicians of Philadelphia

[7] W. Robert Penman, "Charles Delucena Meigs, M D · An Assessment of His Role in Philadelphia Obstetrics," *Trans & Stud. College of Physicians of Philadelphia,* 4th ser , 43, January 1976.

[8] Personal communication with Richard Harrison Shryock (1893-1972). Born in Philadelphia on March 29, 1893; died Fort Lauderdale, Fla., on January 30, 1972 Former director of the Institute of the History of

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called the famous loser in a famous quarrel. Perhaps if he had not met such disgrace his family would not have mentioned the Edinburgh Quizzing System and we would have lost all awareness of its chameleon-like existence. "Quiz Sections" were medical school jargon terms until twenty years ago in American medical schools. Were they the last vestiges of C. D. Meigs and the Jefferson faculty introduction of the Edinburgh Quizzing System?

The Edinburgh Quizzing System is important from an historical point of view because it represents the introduction of a new educational technique that replaced the faltering medical education curriculum of the 1830s. As George Corner noted, "formal medical education was failing in 1830 and the failure was due to the breakdown of the Preceptorship System."[9] The fledgling medical students were acutely aware that they could not learn to practice medicine by sitting on hard lecture room benches for eight hours a day, four days a week, for the short seventeen-week courses given at the medical school of the University of Pennsylvania.[10] This "warming-the-hard-benches" teaching problem was compounded even further when the City of Philadelphia lost valuable clinical teaching material after the Philadelphia Almshouse left the city in 1830 and moved across the river to Blockley Township, deliberately, to rid itself of the pestiferous medical students for a four-year period.[11] With the loss of the Almshouse, the Pennsylvania Hospital teaching wards became even more popular and crowded than they had been from 1800 on. Back in 1802 the large number of students crowded around the patient's bed made it impossible to hear the case presentation or the ensuing medical discussion by Benjamin Rush or Philip Syng Physick.[12] The situation became worse as years went by and medical school enrollment increased.

To fill these important medical educational deficiencies many private, specialized schools of medicine were established in the city of Philadelphia. A few of these schools became justifiably famous—the Philadelphia School of Anatomy[13] and Nathaniel Chapman's "Philadelphia Medical Institute,"[14] as

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Medicine, Johns Hopkins University, Baltimore, Md. Former librarian, American Philosophical Society, Philadelphia

[9] George W. Corner, *Two Centuries of Medicine A History of the School of Medicine, University of Pennsylvania* (Philadelphia· J. B. Lippincott, 1963), p. 244.

[10] F. P. Henry, *Founders Week Memorial Volume* (Philadelphia: City of Philadelphia, 1909), p. 244.

[11] *Ibid.*, p 448.

[12] Dorothy I. Lansing (Editor), *Heads of the Practice of the Physicians of the Pennsylvania Hospital· During the Winter of the Year 1802-03 by William Darlington, Student of Medicine* (Devon, Pa.. Anro Press, 1973).

[13] Henry, *op. cit* (n. 3 above), pp 157-58. The Philadelphia School of Anatomy was founded by Dr. James McClintock in 1839.

[14] *Ibid* , p 148 Founded by Chapman in 1817

well as W. P. C. Barton's Therapeutic Institute[15]—not only because of their excellent teaching but because these private medical schools provided the personal contacts as well as the training grounds for some of Philadelphia's great future professors of medicine who began their careers as assistants in such schools.[16] Many successful teacher-physicians in the city of Philadelphia also taught private pupils in their offices, after office hours, and at night.[17]

George McClellan, the founder of the "New School," as Jefferson Medical College was called in its early years, was one of these very famous private teacher-physicians.[18] As noted by one of McClellan's most famous students, surgeon Samuel D. Gross, McClellan was a most brilliant teacher and surgeon, and Gross remained a private student of McClellan's throughout his years of study at Jefferson.[19]

George McClellan perceived the need in the 1820s to use patients in clinical teaching. He made available to the Jefferson Medical College a large number of charity patients from his own office practice. McClellan then proceeded to establish a teaching dispensatory at Jefferson, the first one in the city of Philadelphia.[20] At this dispensatory, his surgical procedures were performed before the students and medical patients were examined and treated. This was a very successful and innovative form of teaching for Philadelphia and as such was promptly copied by the medical school of the University of Pennsylvania and added to its medical curriculum.[21]

As the Jefferson student body increased in size because the school was providing a better grounding in the real art of medicine, the Jefferson professors created the Saturday evening medical "conversaziones" which they held in the Hall of the Museum in the College.[22] These meetings were of a social nature, with coffee, tea and cake being served. The social lasted from eight to eleven o'clock and the professors attempted to know the students more intimately and freely offered advice and assistance when needed. Apparently informal quizzing groups of students, questioning each other, coexisted in both medical schools at the time and this large, loose, amorphous body of medical instruction waxed and waned in importance in the 1830s.[23]

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[15] William Frederick Norwood, *Medical Education in the United States before the Civil War* (Philadelphia University of Pennsylvania Press, 1944), p. 103. The Therapeutic Institute was founded by Barton in his private dwelling, May 1, 1831, for the purpose of teaching botany, materia medica, general and nautical therapeutics and toxicology.

[16] Henry, *op. cit.* (n. 10 above), p 244

[17] Norwood, *op cit* (n 15 above), p 35

[18] Howard Kelly and Walter Burrage, *American Medical Biographies* (Baltimore Norman Remington Co , 1920). George McClellan (1796-1847), born in Woodstock, Conn , died in Philadelphia in 1847 Graduated with an A B. from Yale in 1816. Preceptor of Thomas Hubbard, Pomfret, Conn , and John Syng Dorsey in Philadelphia. Graduated from the medical department of the University of Pennsylvania in 1819

[19] Gross, *op cit* (n 1 above)

[20] Henry, *op cit* (n 10 above), p 269

[21] Corner, *op cit* (n. 9 above), pp 90-91

[22] Gould, *op. cit* (n 2 above), p. 82

As social needs arose and monetary gifts were offered in the late 1830s, the Philadelphia physicians began to travel abroad and inspect other systems of education, practice and hospital care. On their return they would report to their colleaguse, especially at the College of Physicians of Philadelphia. Meanwhile, at Jefferson, this was a period marked by constant bickering and dissension with frequent turnovers in the faculty and resultant unsatisfactory results in teaching of the students.[24] As a consequence the Board of Trustees dismissed the entire faculty in 1841 and appointed an entirely new group consisting of Charles D. Meigs,[25] Thomas Dent Mütter,[26] Joseph Pancoast,[27] Franklin Bache,[28] Robley Dunglison,[29] J. K. Mitchell,[30] and R. M. Huston.[31]

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[23] Henry, *op cit.* (n 10 above), p 167 See also Robley Dunglison, *The Medical Student or Aids to the Study of Medicine* (Philadelphia Carey, Lea and Blanchard, 1837), p. 181. Dunglison advised students to form quiz clubs.

[24] *Ibid.*, p. 268.

[25] Charles D. Meigs (1792-1869). Born February 19, 1792, on the Island of St. George, Bermuda. Graduated Medical Department of the University of Pennsylvania in 1817. Professor of Obstetrics, Jefferson Medical College, 1841-1861. Penman, *op cit.* (n. 7 above)

[26] Thomas Dent Mutter (1822-1859). Born in Richmond, Virginia. Graduated from the Medical Department of the University of Pennsylvania in 1831. In 1841 he was appointed Professor of Surgery at Jefferson Medical College He donated the contents of the museum that bears his name to the College of Physicians of Philadelphia.

[27] Joseph Pancoast (1809-1882). Born in Burlington, New Jersey. Graduated from the University of Pennsylvania School of Medicine in 1828. In 1838 appointed Professor of Surgery at Jefferson Medical College succeeding George McClellan In 1847 he was appointed Professor of Anatomy at Jefferson Medical School, resigning in 1874.

[28] Franklin Bache (1792-1864). Born in Philadelphia. Graduated from the Medical Department of the University of Pennsylvania in 1814 In 1841 he was appointed Professor of Chemistry at Jefferson Medical College. He is also remembered for his *Dispensatory of the United States of America*, a book which has gone through over twenty editions.

[29] Robley Dunglison (1798-1869). Born at Keswick, England. Graduated with his surgical degree at the Royal College of Surgeons in London in 1819 and his medical degree at Erlangen in 1823. He was invited by Thomas Jefferson to come to the University of Virginia as Professor of Anatomy, Physiology, Materia Medica and Pharmacy. In 1836 he assumed the Chair of the Institutes of Medicine and Medical Jurisprudence at Jefferson. He retired in 1868

[30] John Kearsley Mitchell (1793-1858). Born in Shepherdstown, Virginia, son of a Scottish physician and father of S. Weir Mitchell. He graduated from the University of Pennsylvania School of Medicine in 1819. In 1841 he was appointed to the Chair of Theory and Practice of Medicine of the Jefferson Medical College.

[31] Robert Mendenhall Huston (1795-1864). Born in Abingdon, Virginia. Graduated from the Medical Department of the University of Pennsylvania in 1825. He was Professor of Obstetrics and Diseases of Women and Children at Jefferson Medical College from 1838 to 1841 and Professor of Materia Medical and General Therapeutics from 1841 to 1857.

With this new faculty professional peace and harmony prevailed at the Jefferson Medical College for the next fifteen years and it was during this period that great medical educational changes were made utilizing our elusive component, "the Edinburgh Quizzing System."

The change was first suggested by Thomas Dent Mutter.[32] An 1831 graduate of the Medical School of the University of Pennsylvania who had studied surgery and anatomy in the clinics of Paris and on the wards of Edinburgh, he studied under James Syme, Professor of Surgery at the University of Edinburgh.[33] Syme credits himself with inventing the Edinburgh Quizzing System by way of changing the teaching of surgery in Edinburgh, first at his private hospital in Edinburgh—"Minto House"—in 1829 and then later in 1833 as professor at the University.[34] It was from Syme that Mutter learned of the Edinburgh Quizzing System and introduced it to his new faculty members at Jefferson in 1841.[35]

The system was simplicity itself. The large class of students was broken down into units of six people. Six students plus what came to be popularly known as "quizzers" would question and examine a charity patient together and then discuss diagnosis, treatment and management.[36] The practical knowledge thereby gained had the advantage of being firmly remembered for life by the fledgling physicians. Interestingly enough, I have never found tickets sold to the students for the Quiz Sections. Therefore, I can draw no conclusions regarding the financial arrangements; and there must have been some fee, for the Quizzes had to be paid for.

The installation of the Edinburgh Quizzing System was an attempt by the new Jefferson faculty to ensure adequate and thorough teaching of what was then known in medicine. With each of the Jefferson Medical School classes, then consisting of 300-400 students, it was almost impossible for the six or seven full professors to know intimately all of the students on a personal basis and attend to their own private practices besides. It was even more impossible for the faculty to examine the students closely enough to be aware of the depth of their actual medical knowledge. Therefore, Dr. Mütter, based on his Edinburgh experience, suggested that his surgical classes be divided into groups of six to eight students. To each six to eight man group was assigned a teacher, not necessarily himself. This "quizzer" or teacher met with his group on a weekly basis. On the other four days of the week the students were occupied with daylong lectures, sitting on those long, hard benches, teaching rounds at the dispensatory as well as observations of surgery in the amphitheatre, and teaching sessions at Blockley (the Philadelphia Almshouse) and Wills Eye Hospital.[37]

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[32] Henry, *op cit.* (n 5 above), p 209

[33] Robert Paterson, *Memorials of the Life of James Syme* (Edinburgh: Edmonston and Douglas, 1874), pp 49-52. See also "Clinical teaching," *Lancet*, October 1, 1864, pp 390-91; and T N Foulis, *Famous Edinburgh Students* (Edinburgh, 1914), pp. 126-28

[34] Sir Alexander Grant, *The Story of the University of Edinburgh* (London. Longmans, Green, 1884), volume 2

[35] Henry, *op. cit.* (n. 5 above)

[36] *The Lancet, op cit.* (n 33 above)

The Jefferson quiz classes—their use of the Edinburgh Quizzing System—should not in any way be confused with the student Quiz Clubs so prevalent and popular in earlier Philadelphia university and medical school education.[38] Those voluntary student operations were conducted without any faculty supervision or school authority; but they, again, consisted of a small group of students with each student assigned to take subject notes from one faculty member and at night to "quiz" or educate the other members of the club in the lecture of the day in a specific subject. One surmises that the word quiz was synonymous with educate.

The fame and the success of the Jefferson Medical College in the period up to the Civil War, when its enrollment rose to over 600 students a year, has been attributed to this new, young, cooperative and innovative faculty of 1841 and also to their actual teaching methods and to the institution of the famous Edinburgh Quizzing System as introduced by Thomas Dent Mütter. The Edinburgh Quizzing System was a teaching tool of the medical school that outlived the new young faculty of 1841 and eventually spread to medical schools throughout the United States[39]—lasting up to the present, when it was finally replaced in many instances, at least, by the introduction of the new Duke-Harvard "Core Curriculum" of the 1960s. Interestingly enough, the Jefferson Medical faculty never embraced the new Duke-Harvard Core Curriculum. With minor modifications, the old system is still in existence today, with quizzes and reviews, especially at the sophomore level, to make sure that students can pass Part I of the National Board Examinations. Two other medical schools in Philadelphia in the past two years, Temple University and the Hahnemann Medical College, have tried and rejected the Duke-Harvard Core Curriculum and returned to the medical school curriculum of the 1940-1960 era.[40]

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[37] William Gates Henderson (1821-1852), "Case Books of Lectures and Notes as a Jefferson Medical Student," Archives, College of Physicians of Philadelphia, Cage 10-A-211

[38] Henry, *op. cit* (n. 5 above).

[39] Charles Wilkens Short, *Duties of Medical Students during Attendance on Lectures* An Introductory Address, Delivered at the Opening of the Session of 1845-6, in the Medical Institute of Louisville, Nov. 3d, 1845 (Louisville, Ky. Morton & Griswold, 1845) In Louisville the professor "quizzed" or interrogated the students in the classroom, usually at the end of the formal lecture Also personal communication with Martin Kaufman, Professor of History, Westfield State College, Westfield, Mass. Kaufman noted in his *History of Vermont College of Medicine* (in print) that Professor William Darling (Anatomy) conducted a regular Saturday morning quiz session Kaufman notes that these quiz sessions were very intense and that the quizzing progressed at a very rapid pace.

In summary, then, the Edinburgh Quizzing System was a formalized mode of actual medical practice that was scheduled into the medical students' weekly calendar of events, the original idea for which came from Dr. James Syme of Edinburgh in 1828. Twenty years later, by 1848, some five to six years after its introduction into the United States, it was a firm feature of the successful Jefferson Medical School curriculum.

I wish to stress that I look upon this presentation as being a preliminary presentation. I have just scratched the surface in my research and in order to continue my investigation I will need student notebooks, case books, and diaries of the medical educational experiences of this period. I believe that further investigation and research into the changes in medical education from 1840 on are very important and must be pursued so that we will be able to understand and observe the evolutionary changes in medical education. I hope that this presentation may possibly stimulate other investigators to continue to work in this field of early medical education.

### *Acknowledgement*

I am deeply indebted to my wife and colleague, Dr. Dorothy I. Lansing, for her support, encouragement, editorial advice and proofreading.

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[40] Personal communication with Hugo Dunlop Smith, M D , Associate Dean for Curriculum, Temple University School of Medicine, Philadelphia. *Medical Alumni Bulletin*, Summer-Fall 1975

# MICHAEL BERNHARD VON VALENTINI NON-SPECIALIST IN MEDICINE AND SCIENCE: PHYSICIAN OF THE ENLIGHTENMENT*

ILZA VEITH

We live in an age of distrust of medicine and physicians. In the United States the epidemic of malpractice suits contributes to the extreme caution in recognizing therapeutic achievements, which are adopted only after having been subjected to long series of control experiments and "double-blind" studies. Therefore medical textbooks are now compilations of individual bits of wisdom or expertise by numerous contributors, rather than the work of one man, such as was Sir William Osler.

In contrast to the present-day situation of ultra-specialization are the personality and work of one of the medical scientists of the 18th century, Michael Bernhard von Valentini (1657-1729), who felt so confident of his diagnostic and therapeutic skill that he could entitle his work *Praxis medicinae infallibilis,* or "The Infallible Practice of Medicine" (1711). This remarkable physician, Professor of Medicine, and Rector Magnificus of the University of Giessen, did not confine himself to the task of writing infallibly about all of medicine, but also was a founder and initiator of the idea of natural history museums and—with his superb tome, *Museum museorum* (1704)—became the spiritual father of the many curio cabinets and museums of natural history that can now be found all over the world. Beyond that, Valentini was also one of the first to write extensively on forensic medicine. This paper is intended to be an appreciation of this brilliant representative of the time that straddles the Baroque and the Enlightenment.

Who was this multi-faceted, extraordinarily productive man who so greatly enriched the medical and scientific literature and yet did not attain the fame that was reached by some of his contemporaries? The literary productivity of Boerhaave, for example, did not by any means match that of Valentini. It is true that Boerhaave's fame was largely derived from his teaching activities at Leyden, but Valentini also had an extensive teaching career during the years in which he served as professor at the University. Moreover the multiplicity of his interests and diversity of his achievements bespeak a deep cultural

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* Based on a lecture delivered at the 25th International Congress of the History of Medicine, Quebec, Canada, August 21, 1975.

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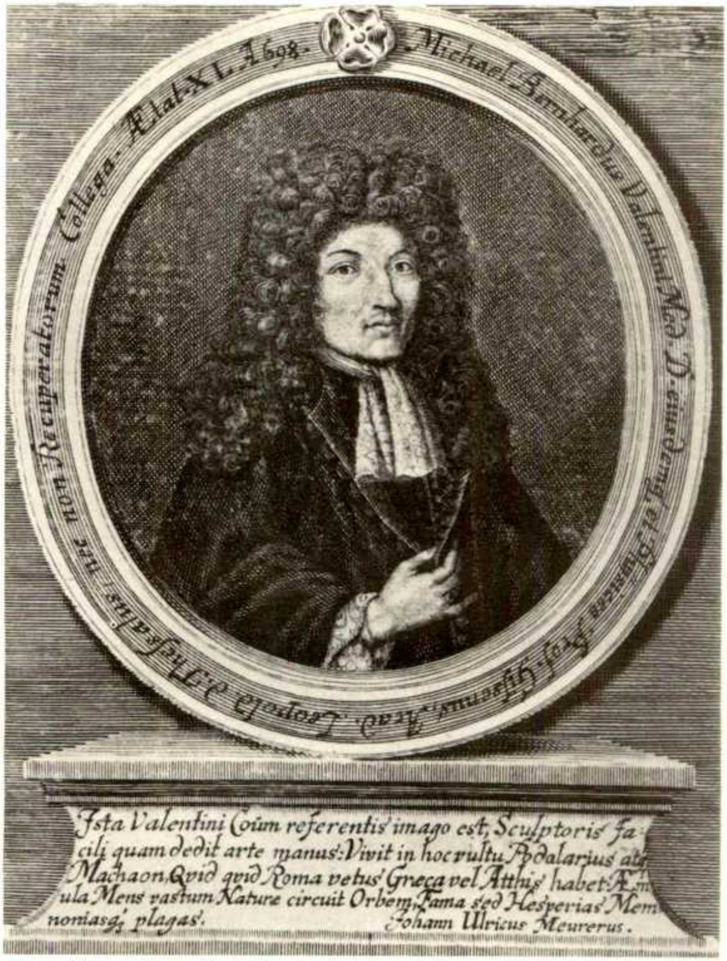
Michael Bernhardus Valentini Med. D. eiusdemq et Physices Prof. Gissensis Acad. Leopold. d. Thesalus, nec non Recuperatorum Collega. Ætat. XL. A. 1698.
Ista Valentini Coum referentis imago est, Sculptoris facili quam dedit arte manus: Vivit in hoc vultu Podalarius atq Machaon, Quid quid Roma vetus Græca vel Atthis habet: Æmula Mens vastum Naturæ circuit Orbem, Fama sed Hesperias Memnoniasq plagas.
Johann Ulricus Meurerus.

Michael Bernhard von Valentini, Med. D.
Physician and Professor at the University of Giessen, Member of the Academia Leopoldina (under the name of) Thesalus at the age of 40 years in 1698.

background and a broad educational preparation. Through fortunate circumstances, we are able to reconstruct his personal and scholarly life, even though his memory has not yet been honored with a full-length biography.

It has often been said that social mobility, though a distinguishing feature in American life, was nearly non-existent in Europe. If this statement were confidently made, the life of Michael Bernhard von Valentini would serve best to contradict it. Not only was he a highly esteemed university professor who was elected to serve as rector magnificus of his university, but he was also greatly respected and well received when he traveled to other university medical centers, and met colleagues of varying ages and distinctions. Here it may be of value to give a very brief sketch of his life history.

Valentini was born in Giessen in 1657. His father, Johann Justus, held a master of philosophy degree from the University of Giessen and the office of actuary and archivist; the father had in fact latinized his old family name of Velten into the more learned-sounding name Valentinus. Only when Michael Bernhard also followed an academic career did he change the name Valentinus into Valentini (literally "of Valentinus"). As he was convinced in early youth that he would want to become a physician, and as he believed that a successful medical practitioner would have to be a highly educated man, he prepared himself by obtaining a thorough humanistic education which included philosophy, rhetoric, logic and metaphysics, ethics, and political science; only then did he turn to the study of medicine proper. In spite of his industry and the thoroughness of his preparation in the liberal arts and the social sciences, he was able to graduate from medical school in 1680, at the age of twenty-three. The next three years he filled with a busy medical practice and with giving private instruction to medical students. At the age of twenty-six he received an honor that is usually reserved for much older scientists. He was elected to membership in the Imperial Academy of Natural Sciences of Vienna. With this distinction he could be certain of his welcome among the scientists of all universities throughout Europe, and he set out for the first of many study trips. He spent one year in post-graduate study at the Universities of Heidelberg and Frankfurt. In 1686 he traveled to Leyden, which, even then, fifteen years before Boerhaave's appointment to the faculty of that university, offered lectures in chemistry, botany and physics. What is more, Valentini tells of having attended Professor Lukas Schacht's lectures in clinical medicine which were held in a small teaching hospital. This statement by Valentini is especially intriguing as it would indicate that there must have been some form of bedside teaching practiced at Leyden prior to Boerhaave's arrival, although he has generally been credited with the introduction of the earliest teaching hospital. In fact, when Boerhaave was first appointed as a teacher at the University of Leyden in 1701, he found a small teaching hospital in existence, together with chemical, botanical and anatomical institutions, all of which have generally been attributed to his inventiveness.

Having made full use of the medical and scientific instruction that Leyden had to offer, Valentini inspected Amsterdam and the Haag, and then crossed the Channel to study the medical institutions of London and to call on the

British physicians and scientists in residence. There he met Robert Boyle and Thomas Sydenham, both of whom received him very cordially, and "gave him the opportunity to increase his knowledge most usefully." Valentini must have been a ready student, for only a month after his arrival in England, he left for Paris to make a quick round of visits with the leading French physicians who also received him very hospitably. This extensive study trip was to serve not only to enrich his knowledge but also to convince the authorities of the University of Giessen to grant him his doctorate from the medical faculty. In this he succeeded in the autumn of 1686, the same year in which he had begun his whirlwind tour to Leyden, London and Paris. Only a few months later, when he was barely thirty years old, he was appointed to the professorial chair of medicine in Giessen. The Leopoldina Imperial Academy of Natural Science, which had extended membership to him so early, now elected him to the Associate Presidency, and the Italian *Academia Recuperatorium* also honored him with membership. Valentini's subsequent academic career had the irresistable force and power of an avalanche; other foreign honors were bestowed upon him. His numerous publications dedicated to the local ruler, the Duke of Hesse, attracted the attention of many scientific societies and led to his being invested with hereditary nobility. Thereafter he was entitled to add the prefix "von" to his name.

The foregoing summary of Valentini's vast experiences in all aspects of medicine inevitably led to the publication of his *Praxis medicinae infallibilis,* which is illustrated with numerous selected case histories from the vast experience of this highly placed physician. The "Infallible Practice of Medicine" is divided into four parts, which reflect a then uncommon insight that each age of man brings with it its own diseases and propensities of specific diseases: I. "On the Disease of Youth" *(puerorum)*, II. "On the Diseases of Young Adulthood *(secundae aetatis)*, III. "On the Diseases of Middle Age" (which is tertiae aetatis), IV. "On the Disease of Old Age." In the first section dealing with the diseases of childhood, we find extensive chapters on epilepsy, others dealing with nausea and vomiting, each of them subdivided into the phenomena of symptoms, the organs affected, the cause of the disease, the prognosis and, finally, the cure.

Among the diseases of the adults there are found discussions of hemorrhages, of melancholia, all ending with the description of the cure.

Among the diseases of the aged he included, though by no means restricted to that heading, a description of diabetes, fluor albus, somnolence. Interspersed with tables on the above-mentioned diseases there are case histories—among the earliest in the older medical literature—most of which end with the victorious statement *curavi*, "I cured" [the patient or the disease]. If in the early 17th century Valentini actually was able to cure a 16-year-old boy of diabetes, then the title of his "Infallible Practice of Medicine" was indeed justified.

Space does not permit a description of the nature and contents of all Valentini's impressive publications such as the *Amphiteatrum zootomicum* (1720) and the *Museum museorum* (1714), both of which contain most of what was known in the early 18th century about natural history in Europe and the newly explored parts of the world in Africa, Asia and the Americas.

I will therefore conclude with a few words about Valentini's publications on forensic medicine, of which I was able to obtain only the *Novellae medico-legales*, that followed only by 10 years his chief work *The Pandectae medico-legales* of 1701. It is evident from the date of publication that Valentini was indeed one of the founders of medical jurisprudence and one of the first medical authors to concern himself at least with forensic medicine. It is interesting that in the *Novellae medico-legales*, Valentini deals in detail with the validity of marriages concluded with eunuchs, hermaphrodites, and other abnormalities of mankind. He discusses also the physical complications that may arise in marriage and may make its consumation impossible, and the legal consequences of incest, mania, abortion, alcoholism, and many of the questions that now concern forensic medicine.

In the beginning of my brief biographical sketch of Michael Bernhard von Valentini, I alluded to his family's origin as Hessian peasants by the name of Velten. Evidently, neither success, honors nor nobility set him apart from the people from among whom his family had sprung, and his otherwise completely Latin treatise on the infallible practice of medicine is concluded with the unexpected German sentence: *Bauren sind auch Leute*.

In concluding, I wish to admit that although I have tried to give an entirely objective account of Michael Bernhard von Valentini, I must "declare an interest," as British politicians are wont to say: Valentini was the founder of my husband's maternal family and for his achievements in scientific literature deserves to be rescued from the obscurity into which history of medicine has permitted him to lapse.

## *Selected Bibliography of the Writings of Michael Bernhard von Valentini*

*Dissertatio inauguralis de convulsionibus* (Giessen, 1680).

This dissertation is also contained in *Praxis medicinae infallibilis,* pp. 421 ff.

*Epistola de monstrorum hassiacorum ortu atque causis* (Marburg, 1684).

*Historia moxae cum adjunctis meditationibus de podagra* (Leyden, 1686).

*Diss. epist. de natura naturae,* ad Rob. a Boyle (Giessen, 1689).

*Diss. de china chinae* (Giessen, 1689).

*Polychresta exotica in curandis affectibus contum contumacissimis probatissima . . . ut et nova herniarum cura . . .* (Frankfurt a. M., 1700). This work also contains several of the above-mentioned dissertations.

*Pandectae medico-legales, sive responsa medico-forensia ex Archivis Academiarum celebriorum scriptisque probatissimorum medicorum deprompta, ac exterorum gratia latinitate donata . . .* (Frankfurt a. M., 1701).

*Museum museorum, oder vollständige Schau-Bühne aller Materialien und Specereyen nebst deren natürlichen Beschreibung* (Frankfurt a. M., 1714).

*Praxis medicinae infallibilis* (Frankfurt a. M., 1711).

## *Main Sources*

Friedrich Wilhelm Strieder, *Grundlage zu einer Hessischen Gelehrten- und Schriftsteller-Geschichte. Seit der Reformation bis auf gegenwärtige Zeit.* 18 vols. (Göttingen, 1781-1819), Bd. 16, pp. 261-281.

*Vorlesungsverzeichnis* der Universität Giessen, 1686-1689.

# AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE REPORT OF THE FIFTIETH ANNUAL MEETING

## Madison, Wisconsin May 11-14, 1977

The Fiftieth Annual Meeting of the American Association for the History of Medicine was held in Madison, Wisconsin, May 11-14, 1977. Sessions were held at the Edgewater Hotel and at the Wisconsin Center, University of Wisconsin. Preceding the meeting, two continuing education seminars (6 hours of Category 1 credit), sponsored by the University of Wisconsin Departments of the History of Medicine and Continuing Medical Education in cooperation with the American Association for the History of Medicine, were offered. "Traditions of Medical Ethics: Rights and Duties," Chester R. Burns; and "What the History of Professionalization of American Medicine Says to the Professions Today," Robert P. Hudson. In addition a workshop, "Preservation of Library Materials (including Photographs)," was sponsored by the Association of Librarians in the History of the Health Sciences.

The following report, which includes the program, abstracts of the minutes of the Council and Business Meetings, as well as the financial report for fiscal year 1976-77, has been prepared by the Secretary-Treasurer, Dr. Charles G. Roland, and is intended for the information of the members of the Association. The official minutes and financial report are preserved in the office of the Secretary-Treasurer.

### PROGRAM

Wednesday, May 11

5:00 p.m. Registration and Reception
7:00 p.m. Council Meeting

Thursday, May 12

9:00 a.m. General Session
Guenter B. Risse, *presiding*
"The Wisconsin Programs in the History of Science and the Health Professions," Glenn A. Sonnedecker
9:30 a.m. General Session
Caroline C. Hannaway, *presiding*
"Phillippe Pinel and the Beginning of Clinical Teaching in France," Dora B. Weiner
"J. B. A. Chevallier, Pharmacist-Chemist: A Major Figure in Nineteenth-Century French Public Health," Alex Berman
"Civil War in the French Medical Profession: Doctors and Medical Schools, 1878-1914," George Weisz

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"The Ressurectionists" (film), Thomas R. Forbes and Susan Wheeler

12:00 noon Luncheon Roundtables

"How to Collect Medical Books," W. Bruce Fye, *Coordinator*

"How to Prepare and Publish a Paper in the History of Medicine," Lester S. King, Lloyd G. Stevenson, *Coordinators;* Saul Jarcho, Janet B. Koudelka and Leonard G. Wilson, *Contributors*

"How to Teach the History of Medicine to Undergraduates," Theodore M. Brown and Judith Walzer Leavit, *Coordinators*

"Unexplored Resources for the Social History of Medicine," Martin S. Pernick and Barbara Gutmann Rosenkrantz, *Coordinators*

"What Medical History is About," William B. Bean, *Coordinator*

2:00 p.m. General Session

Gordon W. Jones, *presiding*

"Alleviation of Pain in Childbirth: A Study in Popular Medicine of the Eighteenth and Early Nineteenth Centuries," Inci A. Bowman

"Childbirth and Health Care Throughout Time: Maternal Mortality in England, 1750-1920," Terry Ruth Toll

"Toward a Reconstruction of Contagionism and American Medical Thought: Puerperal Fever as a Case Study, 1840-1860," Gail Pat Parsons

"Crib Death: Its Medical and Social History," Todd L Savitt

4:45 p.m. The Fielding H. Garrison Lecture

John Duffy, *presiding*

"Taking Things Apart and Putting Them Together Again," Jane M. Oppenheimer

8:30 p.m. Movie Time: An Evening of Medico-Historical Films.

Exhibit: "200 Years of Wisconsin Medicine" and "Rare Books in the Middleton Medical Library"

Friday, May 13

9:00 a.m. Session A

Arthur G. King, *presiding*

"The U.S. Army Medical Department: Formative Years, 1783-1818," Allen C. Wooden

"Founding an Army Medical School, 1893-1913," Robert J. T. Joy

10:15 a.m. E. A. Hammond, *presiding*

"Ancient Celtic Incubation," William James Hersh

"Medieval Schematic Literature and the Separation of Medicine and Surgery," Darrel W. Amundsen

"Richard Bright and Mercury as the Cause and Cure of Nephritis," Steven J. Peitzman
9:00 a.m. Session B
Pauline M. H. Mazumdar, *presiding*
"The Influence of the Nineteenth-Century American Botanico-Medical Movement on British Medicine," John K. Crellin
"World War I and British Psychiatry: The Treatment of Shell Shock," Jeffrey C. Lerner
10:15 a.m. Ronald L. Numbers, *presiding*
"The Emergence of a Medical Community in Massachusetts 1700-1792: The Demographic Factors," Eric H. Christianson
"Dr. James Lloyd (1728-1810) of Boston: Tory Force in a Whig Society," Philip Cash
"To Find a Stand: New England Physicians on the Western and Southern Frontier, 1790-1840," William Barlow
2:00 p.m. Session A
John H. Ellis, *presiding*
"Medical Theory and Practice in Philadelphia, 1783-1813," Lilith R. Kunkel
"How Medicine Became Scientific," Edward C. Atwater
"Polygamy, Congenital Defects, and Utah Statehood: Interactions of Religious and Medical Thought in the Mormon Community," E. Victoria Grover-Swank
"Physician-Chiropractors: Medical Presence in the Evolution of Chiropractic," Russell W. Gibbons
2:00 p.m. Session B
Charles G. Roland, *presiding*
"The Hippocratic Tradition in Japan: A Legacy of the *Rangakusha*," Norman T. Ozaki
"Famines and Epidemics in Kenya, 1890-1918," Marc H. Dawson
"The Self-Image of American Physicians, 1880-1930," Daniel M. Fox and James Terry
4:15 p.m. Business Meeting
7:30 p.m. Annual Banquet
"The Mercuriad, or The Spanish Practice of Physic" (an anti-mercurial American play, 1807), The Wisconsin History of Medicine Players

Saturday, May 14

9:00 a.m. Symposium: "History, Science, and Politics—Influenza in America, 1918-1976." Co-sponsored by the Department of History of Medicine, University of Wisconsin, June Osborn, *presiding*
"The Pandemic of 1918," Alfred W. Crosby, Jr.
"The Scientific Basis for the 1976 Immunization Campaign," H. Bruce Dull

"Immunization and Public Policy; A Short History of P.L. 94-380," Arthur J. Viseltear

*Program Committee*: William Coleman, Martin Kaufman, Arthur G. King, LaVerne Kuhnke, Ronald L. Numbers, George E. Gifford, Jr., *Chairman*.

*Local Arrangements Committee*: Helen Crawford, Judith Walzer Leavitt, Lawrence D. Lynch, Ronald L. Numbers, John Parascandola, Glenn A. Sonnedecker, Guenter B. Risse, *Chairman*.

## MINUTES OF THE ANNUAL MEETING OF THE COUNCIL

May 11, 1977

The annual meeting of the Council of the American Association for the History of Medicine, Inc., was called to order at the Edgewater Hotel, Madison. Wisconsin, at 7:30 p.m. on May 11, 1977. President John Duffy served as Chairman, and Charles G. Roland as Secretary. The following Council members were present: Edward C. Atwater, Thomas G. Benedek, John C. Burnham, William Coleman, Audrey B. Davis, Lester S. King, Genevieve Miller, Guenter B. Risse, Charles Rosenberg, Ilza Veith, Dora B. Weiner, Richard J. Wolfe. Absent were George E. Gifford, Jr., and A Benedict Schneider. Guests: Donald G. Bates and Chester R. Burns.

The minutes of the last meeting, as circulated to the Council in May 1976, were approved. An abbreviated version was published in the Spring 1977 issue of the *Bulletin of the History of Medicine.*

The Secretary reported that the new *Membership Directory* was distributed to members in July 1976, and that the third annual *Research in Progress,* compiled by James H. Cassedy, was distributed early in 1977. Twenty-two members have applied for membership in the International Society for the History of Medicine.

Dr. Roland presented the Financial Report for the Fiscal Year 1975/76, ending 31 October 1976. It was approved for presentation to the Business Meeting, and the President appointed Peter D. Olch and Janet B. Koudelka as Auditors. Dr. Roland commented that the membership decreased, in the past fiscal year, from 955 to 918.

### *Committee Reports*

Dr. Duffy announced that James H. Cassedy has been selected as Garrison Lecturer for 1978. The Chairman of the selection committee was Russell C. Maulitz.

The William H. Welch Medal Committee (David F. Musto, *Chairman*) unanimously recommended and the Council approved by mail vote that the medal be given to Lester S. King, of Chicago, for his scholarly contributions to the history of medicine.

The William Osler Medal Committee (Wilhelm Moll, *Chairman*) recommended and the Council approved the award of the 1977 Osler Medal to James Allen Young, the class of 1977 at the Duke University School of Medicine, for his essay on "Anthropometric Study of Human Growth in Nineteenth Century American Medicine". Lawrence G. Miller, Harvard University, was awarded Honorable Mention for his paper, "Pain, Parturition, and the Profession: The Twilight Sleep in America." A dozen essays were submitted for the contest from Cornell Medical College, University of Alabama at Birmingham, Howard University, Yale University, Columbia University, Bowman-Gray School of Medicine, Harvard University, University of Texas at Galveston, State University of New York-Upstate Medical Center, Johns Hopkins University, and Duke University.

The Loeb Library Committee (Saul Jarcho, *Chairman*) reported that Paul Potter, University of Western Ontario, London, is proceeding with the translation of volume V of the Hippocratic treatises for the Loeb Classical Library. Discussions are going ahead regarding translation and publication of volumes VI and VII of Hippocrates and a volume of Dioscorides. After some discussion of the optimal way to administer future grants that may be made for this project, the Secretary-Treasurer was instructed to discuss with the Committee the possibility that future grant applications might be made by the translator's institution and be accompanied by the official approval of the Association, once that has been obtained.

The Association's Delegate to the International Society for the History of Medicine, Dora B. Weiner, reported on the 25th International Congress for the History of Medicine (see *Bull. Hist. Med.*, 1976, *50*:622-624), on the efforts to convince the ISHM to accept English as the second official language of the Society, on a pending revision of the by-laws of the ISHM, and on the possibility of holding an International Congress in the U.S.A. in 1984 or 1988. The Council voted to ask the Delegate to pursue the last possibility with the ISHM, but without making any formal commitment at this date.

Lengthy discussion followed the re-presentation of the report of the Ad Hoc Committee on Meetings (Donald G. Bates, *Chairman*), already considered at the Galveston meeting. It became apparent that reasonable consensus existed that careful consideration should be given by the Association to the planning of future meetings. Council voted to accept and implement (2 opposed) the following motion:

> That a three-member committee be appointed by the President for a term of 3 years to advise the Council on planning the arrangement of Association meetings to be held in 1981, 1982, and 1983. The committee is to address the following points: (1) whether the meeting(s) could and should be held in conjunction with some other learned society or societies; (2) whether an appropriate geographic balance can be planned; (3) whether alterations or modifications should be made in the internal arrangements and format of the meetings; (4) the committee is to draft a manual or set of informal guidelines for the use of local arrangements committees.
>
> The recommendations of this committee are to be presented to the Council in timely fashion so that decisions may be made in adequate time for the relevant meeting(s).

The Council reaffirmed its acceptance, at the Galveston meeting, of the report of the Ad Hoc Committee on Education (Chester R. Burns, *Chairman*). The establishment of a new standing committee requires a change in the by-laws; the proposed change will be submitted to the membership before the 1978 annual meeting.

### *Other Business*

Several problems with regard to publications were discussed, including instances of duplicate publication, delays in publication, and the Association's right to insist that papers delivered at AAHM meetings be considered first by the *Bulletin*. The Council voted to accept a motion stating that when the Program Committee accepts a paper it will indicate to the presenter that the paper is accepted with the understanding that the *Bulletin of the History of Medicine* will have first right of acceptance for publication.

The Council approved the President's proposed Nominating Committee for the 1977-1979 term: Saul Jarcho (*Chairman*), Guenter Risse, and James Cassedy

In response to an inquiry from James H. Cassedy, the Council reaffirmed its conviction of the importance of continuing to produce the biennial *Research in Progress*. Further, the Council voted a statement of commendation to Dr. Cassedy for his untiring efforts in preparing the three compilations already issued.

The Council meeting adjourned at 10:30 p.m.

Charles G. Roland
*Secretary-Treasurer*

## MINUTES OF THE ANNUAL BUSINESS MEETING

May 13, 1977

The Annual Business Meeting of the American Association for the History of Medicine, Inc., convened in the Edgewater Hotel, Madison, Wisconsin, at 4:30 p.m. on May 13, 1977. John Duffy, President of the Association, served as Chairman, and Charles G. Roland as Secretary.

The minutes of the previous meeting were approved. Dr. Roland read the names of members who had died since last May. They are as follows:

Milton J. Goodfriend, New York City
Jason A. Hannah, Toronto, Ontario
J. J. Longacre, Cincinnati, Ohio
Walther Riese, Glen Allen, Virginia

The members rose for a moment of silence.

The Secretary-Treasurer presented a summary of the Financial Report for the Fiscal Year 1975/1976. The auditors reported that the books were in order, and the reports of the Treasurer and the auditors were approved.

The Secretary-Treasurer also reported on membership. Two new Constitutent Societies have joined the Association: The Cecil Striker Society for the History of Medicine and the American Osler Society. Overall membership has declined from 955 to 918.

### *Old Business*

The President commented briefly on the activities of the various committees Dr. Saul Jarcho, on behalf of the Loeb Library Committee, reported that work is proceeding with Dr. Paul Potter's translation of the fifth volume of Hippocrates and that negotiations are going forward to permit the translation and publication of additional volumes. Dr. Dora Weiner summarized her efforts, as the Delegate to the International Society for the History of Medicine, to introduce bilingualism into that group's publications and meetings.

*New Business*

Russell Maulitz, Chairman of the Garrison Lecture Committee, announced the 1978 Garrison Lecturer, Dr. James H. Cassedy. Dr. Daniel Weiss, on behalf of the Welch Medal Committee, reported that the 1977 medal would be awarded to Dr. Lester King for his numerous scholarly contributions to the history of medicine. Chairman of the Osler Medical Essay Committee, Dr. Wilhelm Moll, informed the membership that 12 essays were considered and that that of Dr. James Allen Young, Duke University School of Medicine, had been awarded the medal.

Dr. James Cassedy announced that the National Library of Medicine's history of medicine data bank will go on-line sometime in the next three months.

The President reported a request for information about whether veterinary students could be eligible for competition in the Osler Medal contest. It was pointed out that that medal had been created especially to encourage *medical* students to study the history of medicine. Dr. Donald G. Bates recommended that the Association investigate the possibility of creating another, more broadly based, contest for student essays, to exist in tandem with the Osler Medal Essay contest. This suggestion was approved and the question was referred to the Education Committee.

The President announced that the next annual meeting of the Association will be held in Kansas City, Kansas, May 4-6, 1978,* under the chairmanship of Dr. Robert Hudson. Future meetings will be held in Pittsburgh, Pa. (1979), and Boston (1980). He further reported that the Council had appointed a special committee on meetings, with a 3-year term, to make general recommendations to Council and study particularly appropriate sites for the 1981, 1982, and 1983 meetings.

The following officers were elected unanimously:

*Secretary-Treasurer (2-year term)*
Charles G. Roland

*Members of Council (3-year terms)*
Leslie A. Falk, Nashville, TN
Wilhelm Moll, Charlottesville, VA
Ronald L. Numbers, Madison, WI
Ynez V. O'Neill, Los Angeles, CA

The meeting was closed at 5:15 p.m. by the President, who announced that the Chairman of the Program Committee for the 1978 meeting is Ronald L. Numbers. Enthusiastic thanks were expressed to the Local Arrangements Committee led by Dr. Guenter B. Risse for the delightful meeting in Madison. Applause.

Charles G. Roland
*Secretary-Treasurer*

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* The date was later changed to May 10-13, 1978.

## Financial Report on the Fiscal Year 1975/76
## Ending 31 October 1976

### Classified Statement of Receipts and Disbursements
### 1 November 1975—31 October 1976

*General Fund*

| | | |
|---|---|---|
| RECEIPTS | | |
| Dues | | |
| Regular Members | $13,658 26 | |
| Contributing Members | 550.00 | |
| Non-Resident Members | 468.00 | |
| Constituent Societies | 518.00 | |
| Contrib. Constituent Societies | 50.00 | |
| Family Members | 950.00 | |
| Student Members | 375.00 | |
| Int. Society History of Med. | 766.50 | |
| | | $17,335.76 |
| Mailing list sale | | 165 79 |
| Refunds from Johns Hopkins Univ. Press | | 1,267 50 |
| Sale of adding machine | | 50.00 |
| 1975/76 Total receipts | | 18,819.05 |
| Balance 1 November 1975 | | 8,037.28 |
| | | $26,856.33 |
| EXPENDITURES | | |
| Subscriptions to Bull. Hist. Med. | | 12,623.00 |
| Administrative expenses | | |
| Printing, postage, stationery, etc. | | 1,318.65 |
| Clerical assistance and FICA taxes | | 449.84 |
| Membership Directory | | 1,146.05 |
| Dues refunds | | 129.39 |
| Honors and Awards | | |
| Osler Medal | | 528.72 |
| Welch Medal | | 18.07 |
| Garrison Lecture | | 500.00 |
| Reprints "Missing Seal" article | | 98.75 |
| Transfers to other funds | | |
| Int. Soc. Hist. Med. | | 751.50 |
| Endowment Fund | | 2,000.00 |
| 1975/76 Total Disbursements | | 19,563 97 |
| Balance 31 October 1976 | | 7,292 36 |
| | | $26,856.33 |

### *President's Discretionary Fund*

| | | |
|---|---|---|
| RECEIPTS | | none |
| | Balance 1 November 1975 | $159.57 |
| DISBURSEMENTS | | none |
| | Balance 31 October 1976 | $159.57 |

### *Annual Meeting Fund*

| | | |
|---|---|---|
| RECEIPTS | | none |
| | Balance 1 November 1975 | $1,635.39 |
| DISBURSEMENTS | | |
| Galveston Meeting | | $1,802.67 |
| | Balance 31 October 1976 | (167.28) |
| | | $1,635.39 |

### *Endowment Funds*

| | | |
|---|---|---|
| RECEIPTS | | |
| From General Fund | | 2,000.00 |
| Contributions | | 250.00 |
| Interest on Savings Accounts | | 1,059.95 |
| | Balance 1 November 1975 | 5,009.99 |
| | | $8,319.94 |
| DISBURSEMENTS | | none |
| | Balance 31 October 1976 | $8,319.94 |

### *International Society for the History of Medicine*

| | | |
|---|---|---|
| RECEIPTS | | |
| Dues collected for U.S. Members | | 751.50 |
| | Balance 1 November 1975 | 164.11 |
| | | $915.61 |
| DISBURSEMENTS | | |
| Dues transferred to International Society | | 793.40 |
| | Balance 31 October 1976 | 122.21 |
| | | $915.61 |

### *Publication Fund*

| | | |
|---|---|---|
| RECEIPTS | | none |
| | Balance 1 November 1975 | $4,000.00 |
| DISBURSEMENTS | | none |
| | Balance 31 October 1976 | $4,000.00 |

| | |
|---|---|
| Total receipts for all funds | 22,880.50 |
| Total disbursements from all funds | 22,160.04 |
| | 720.46 |

## STATEMENT OF BALANCE

31 October 1975

| | |
|---|---|
| ASSETS | |
| Cash in Checking Account | |
| Continental Bank | 2,270.37 |
| Cash in Operating Savings Account | |
| Continental Bank | 2,993 18 |
| Cash in 5% Savings Account | |
| Continental Bank | 2,186.15 |
| Savings Certificate | |
| Continental Bank | 5,247.73 |
| Savings Certificate | |
| Shaker Savings Assoc | 6,308.91 |
| | $19,006.34 |
| LIABILITIES | 00,000.00 |
| NET WORTH | |
| General Fund | 8,037.28 |
| President's Fund | 159.57 |
| Annual Meeting Fund | 1,635.39 |
| Endowment Funds | 5,009.99 |
| Publication Fund | 4,000.00 |
| International Society | 164.11 |
| | $19,006.34 |

| | |
|---|---|
| ASSETS | |
| Cash in Checking Account | |
| First National Bank | 1,923 38 |
| Outstanding deposit | 7.50 |
| Cash in Savings Accounts | |
| Olmsted Federal S. & L. | 3,040.06 |
| | 2,412.96 |
| Savings Certificate | |
| Continental Bank | 5,597.24 |
| Savings Certificate | |
| Shaker Savings Assoc. | 6,745.66 |
| | $19.726.80 |
| | 00,000.00 |
| LIABILITIES | |
| NET WORTH | |
| General Fund | 7,292.36 |
| President's Fund | 159.57 |
| Annual Meeting Fund | (167 28) |
| Endowment Funds | 8,319 94 |
| Publication Fund | 4,000.00 |
| International Society | 122.21 |
| | $19,726.80 |

# *NOTES AND COMMENTS*

## GEOGRAPHICAL ORIGIN OF THE BLACK DEATH

### COMMENT

MICHAEL W. DOLS

In a recent article by Professor John Norris entitled "East or West? The Geographical Origin of the Black Death" (*Bull. Hist. Med.*, vol. 51, no. 1, pp. 1-24), the author suggests that the Black Death did not originate in China, India, or central Asia. The pandemic, he alleges, spread northward through the wild rodent population from Kurdistan and Iraq to southeastern Russia. Because this problem has been the subject of my own research and Professor Norris relies a good deal on my work, I feel compelled to comment.[1]

Passing over a number of minor errors in the article, I would like to make three major points. First, Professor Norris states (p. 3) that: "None of the original accounts is, strictly speaking, contemporary, since all were written at least a few years after the event [the Black Death], and were coloured by the knowledge of the plague's ravages in Europe and the Middle East." This is wrong. We have the account of the Black Death by Ibn al-Wardī, who died in Aleppo of the disease in 749/1349. I have published an English translation of this important notice of the pandemic.[2]

This Arabic account of the Black Death leads me to my second point: Ibn al-Wardī makes the clear assertion, which was repeated and elaborated upon by later Middle Eastern writers, that the disease came from inner Asia, where it had attacked the Uzbeks and the Khiṭai. The precise location of this region is unclear, but it is certainly not Iraq and Kurdistan as Professor Norris suggests. There is absolutely no historical basis for his suggestion because there is no evidence of plague outbreaks—to say nothing of enzootic foci—in the Middle East for at least a hundred years before the Black Death. Kurdistan, particularly, was not established as a major plague

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[1] Dols, *The Black Death in the Middle East* Princeton, 1977 see especially pp 13-67.

[2] Dols, "Ibn al-Wardī's *Risālah al-naba' 'an al-waba'*, A Translation of a Major Source for the History of the Black Death in the Middle East," *Near Eastern Numismatics, Iconography, Epigraphy and History: Studies in Honor of George C. Miles*, ed. by D. K. Kouymjian (Beirut, 1974), pp. 443–55.

BHM 52 (1978) 112-134
0002-5140/78/0521-0112 $01 00
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focus until the sixteenth century.[3] The occurrences of plague that did take place in Iraq and Azarbayjan during the Black Death were obviously later than the outbreak in the Crimea. There is no reason for saying therefore that the pandemic moved northward to southern Russia. I do not wish to repeat here my interpretation of the transmission of the Black Death in the Middle East that is based on the Arabic and Persian sources, but rather to emphasize that the origin was definitely not Middle Eastern.

Finally, Professor Norris has rightly posed the question of the ultimate origin of the Black Death, and I do not claim to have the answer. But the answer must lie, if anywhere at all, in the central Asian and far eastern sources; such an investigation should be our foremost *desideratum*. Professor Norris has not investigated the oriental sources but has reviewed and criticized earlier scholarship on the subject. Moreover, he has presented a theory for the origin of the pandemic based entirely on modern study of plague pathology. With regard to the arguments for his theory, I think that the author should have raised the important question at the outset of whether the micro-organism may have changed significantly in the past six hundred years, which I believe it has. Nevertheless, concerning his first argument, there is no cogent reason to doubt the long-range and gradual transmission of the Black Death overland from central Asia, in the same way that it was carried through the Middle East and North Africa. Secondly, the present-day map of plague foci in the U.S.S.R. does not suggest anything about the transmission of the micro-organism in the fourteenth century except that plague has remained endemic there. The third medical argument from the modern bio-chemical classification of *Pasteurella pestis* by Devignat is not persuasive because the Black Death itself was probably glycerol-positive (var. *antiqua*) and the other two strains, *orientalis* and *mediaevalis,* have developed since the fourteenth century and each strain cannot be assigned exclusively to a distinct geographical region—all three are present in the Middle East. This argument can as easily be turned around and used in favor of an Asiatic origin.

Modern medical knowledge is indeed important for our understanding of the history of disease, but it must be used with the greatest care within an historical framework. On historical grounds alone it seems to me that Professor Norris' theory is simply untenable.

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[3] This matter is dealt with in my forthcoming article, "The second plague pandemic and its recurrences in the Middle East: 1347-1894," in *The Journal of the Economic and Social History of the Orient* (Paris)

# RESPONSE

## JOHN NORRIS

In reply to Professor Dols's criticisms of my article: "East or West? The Geographic Origin of the Black Death" (*Bull. Hist. Med.,* vol. 51, no. 1, pp. 1-24) may I make the following points:

1. "On historical grounds alone it seems to me that Professor Norris's theory is simply untenable" (Dols).

As I pointed out in the article, the historical record that we have to support the attribution to the Far East as the origin of the Black Death is vague and uninformative. The most significant lacks are:

(a) Lack of clear evidence for the presence of plague in the Far East in the form of a clinical description of the disease, such as Procopius provided for the Justinian Plague and Guy de Chauliac for the Black Death in France, and such as the Chinese commentators provided, long before the fourteenth century, for such diseases as smallpox, typhus and measles. The first clinical description of the plague in China that we know of thus far dates from the seventeenth century A.D.

(b) Lack of a clear indication of what part of the Mongol dominions was being referred to in the attribution to the "Land of the Khitai" or the "land of darkness." This could have referred to almost anywhere in the lands of Chagatai or the Golden Horde, but is unlikely to have referred to China since the customary designation for China was not used.

(c) Lack of a reliable interpretation of Chwolsen's discoveries at Issyk-kul in 1885. Chwolsen jumped to the conclusion that the deaths in 1338-9 must have been due to plague, though only ten of these were even attributed, vaguely, to "pestilence." He wrote before much was known of the scientific epidemiology of plague. Stewart thoroughly garbled Chwolsen's account, and Dr. Pollitzer, with characteristic insouciance, elaborated on the claims of both without having examined the original account. Various historians since, including Professor Dols, have relied on Pollitzer, believing that this statement establishes not merely the presence of plague in its sporadic human form, but of plague in its epidemic form, in Turkestan in the mid-fourteenth century. It is a poor piece of evidence, as I have suggested in my article. At best it establishes only the presence of the sort of sporadic outbreak which occurs in contemporary times in sparsely-settled, isolated communities in close contact with the natural ecology—such as exist in the Southwest United States and the mountain valleys of Kurdistan and Ecuador—and which, for obvious reasons of population dynamics, does not lead either to mass epidemics or to transmission to centers where mass epidemics can occur.

(d) Lack of any reports of plague outbreaks en route from China to the Caspian Basin, the lower Volga and the Crimea in the half century preceding the report of the outbreak at Caffa.

Professor Dols has not addressed himself to any of these historical points.

2. In the absence of the information detailed above the historian can: (a) fall back on the reassertion of conflicting and inaccurate historical information; or (b) endeavour to develop an alternative explanation based upon epidemiological probabilities, making sure that these do not conflict with clear, reliable, historical evidence, where this is available. This brings me to what I take to be Professor Dols's main objection: viz. "I think that the author should have raised the important question at the outset of whether the micro-organism may have changed significantly in the past six hundred years, which I believe it has." I did not do so in the article because it would have distorted the theme, but will do so now.

A. *The micro-organism has changed significantly*: It may have indeed, but in what ways? If by "change" Professor Dols means that the disease's "strain" as identified by its immunology has changed, he is repeating a truism. But even changes in immunology, beyond a narrow range of biochemical alterations, are influenced exogenously:—by the changes in relationships between various hosts and various strains of the micro-organism in various exological circumstances. It is not clear from Professor Dols's comment what significant differences he is anxious to identify. He may be suggesting that the etiology (certainly not the "pathology," or even the epidemiology) of plague was different in the fourteenth century in the circumstances of the epidemics in Europe and the Middle East from what it was in the twentieth century in the circumstances of epidemics in temperate regions. But we do not know whether he is suggesting an *endemic*, rather than an *enzootic,* beginning to the epidemic in the fourteenth context, and/or an *epidemic* (originating in interhuman transmission by human extoparasites), rather than an *epizootic,* base for particular local epidemics in the fourteenth century. An hypothesis that interhuman, or some other form of transmission that is not dependent upon wild-rodent reservoir foci and mass-vector transmission by commensal rodents was the basis of *epidemic* outbreaks of plague in the fourteenth century requires that there should have been a remarkable change in the means of transmission between then and now, if we are considering simply changes over time. Today there is a differential immunology among the vertebrate and insect hosts of the disease, determined, it is generally believed, by natural selection. This is dependent, in turn, upon the duration and constancy of exposure to the disease, and upon the ecological circumstances in which the exposure occurs. Thus those hosts which are most resistant to the disease are those which, apparently, have been longest and most consistently exposed to it, and in which the association between hosts (flea-with-rat, in this instance) has been closest, longest, and most consistent. Those which are least resistant to it are those whose exposure has been shortest and least consistent, among whom man is one of the least immune by reason of his short and sporadic exposure. In order to postulate a *general* set of circumstances in which the disease, six hundred years ago, was *endemic* (which it is not now in temperate climates), and *not enzootic* (which it is now in temperate climates), we would have to suppose a process of natural selection under the influence of particular ecological circumstances which have operated over the last six hundred years to *decrease* human resistance to the disease and *increase* rodent resistance to it, relative to the situation in the fourteenth century.

This obviously presents great difficulties, and very early in the now substantial history of endeavours to attribute plague in Europe in early modern times to interhuman transmission, a specialized argument was adopted which had the effect of emancipating the attribution from purely evolutionary considerations. This was the proposition that the micro-organism changes hosts in response to different environmental, especially climatic, conditions. The emphasis was upon change as the result of differing environments, rather than simply a change in the micro-organism over time. Presumably environmental conditions governing plague etiology in the twentieth century could have existed in the fourteenth century, but did not, according to the supporters of this proposition, for various specialized reasons. The argument gained support from the fact that in the early twentieth century there were large plague epidemics in the tropics but not (with two exceptions) in the temperate regions, whereas plague had been epidemic in early modern Europe. This led some historians and some plague investigators to conclude that the etiology of plague in early modern times in Europe must have been different from that unfolding in the investigations of plague in South Asia in the early twentieth century. Hankin (1898) stated erroneously that there were no rats in England at the time of the Black Death, as witness the lack of notices of rat epizootics, and hence the transmission could not have involved rodents.[1] Sir Charles Martin (1911, 1913) compounded this by expressing the belief that a variation of the plague bacillus in the direction of greater infectivity with diminished toxicity, leading to increased septicemia in man, would permit interhuman transmission by human fleas (presumably *Pulex irritans*, the "eclectic flea"). "For various reasons, (he concluded) it seems not improbable that this may have happened in the plagues of the Middle Ages."[2] Neither this variation nor its origins have been identified scientifically, nor did Martin specify his reasons for believing that the variation occurred, but his suggestion was taken up, elaborated upon and employed by successive historians of plague. Neither at that time nor later were attempts made by supporters of this idea to identify precisely the environmental circumstances which were supposed to have caused this variation in hosts, other than vague references to the adverse effects of severe European climate in early modern times on the insect and vertebrate hosts of plague; the wide range of environmental variables determining the ecology of the micro-organism and its hosts were not acknowledged; and the possibilities presented by the rather wide range of adaptibility of the micro-organism *and* its various hosts were not explored. Nor was the fact that two major twentieth-century epidemics of bubonic plague—in Russia and the Western United States—were both based on rodent epizootics acknowledged as at least challenging the climatic variation theme.

By the 1920s the growing doubt that the Indian Plague Commission's identification of plague epidemiology applied in early modern European circumstances was being based on the following propositions:

(a) Doubt that the principal plague fleas of South Asia could survive in a temperate climate (though they survived the much more rigorous Manchurian and Russian climates in the twentieth century).

(b) The lack of records of rodent epizootics during early modern European epidemics, in contrast to the vague traditions in Asian countries that the death of rats portended plague epidemics. This likewise proved unreliable as evidence, since commensal rodents rarely die in the open in temperate climates. Subsequently the discovery that a relatively small outbreak of plague in Algeria in 1921 (the Aumale plague) apparently depended for its spread on human ectoparasites, coupled with the failure of investigators to discover dead rats during the epidemic led a number of commentators, including William Letham, Ricardo Jorge and Georges Blanc, to conclude that all human ectoparasites can play at least an episodic part in the transmission of the micro-organism. Further discoveries of this phenomenon, notably by C. R. Eskey in Ecuador and by M. Baltazard *et al* in Kurdistan reinforced these conclusions. But it was generally conceded that the major epidemics required the mass base of a rodent epizootic to begin them.

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[1] E H Hankin (1898), *Annales d'Institut Pasteur*, 12 746

[2] C. J. Martin (1913), *British Medical Journal*, 1.63-4, cf. *ibid* (1911), 763.

The Aumale discovery inspired Ricardo Jorge to conclude, *ex post factum,* that not only had the outbreak in Oporto in 1899 been transmitted between humans by human ectoparasites, but that this had been the form in which plague had been transmitted in early modern times in Europe. The most thorough application of the interhuman transmission theme to plague in early modern Europe, however, was that made by Ernst Rodenwaldt (*Pest in Venedig* [Heidelberg, 1953]), in which he endeavoured to establish that the choice of insect vectors was determined by climate; but apart from suggesting that cool weather favoured human ectoparasites as against rodent plague fleas such as *X. cheopis* and *N. fasciatus,* he made little attempt to study the ecology of the disease in temperate climates, and ignored the substantial possibilities of local reservoirs existing in the rodent populations of the marshlands of the northern Adriatic shore.

In summarizing this part of the argument for interhuman transmission by human ectoparasites, it must be stated that unquestionably there are transmissions to humans by a variety of rodent fleas in a variety of ecological circumstances; and in limited ecological circumstances between humans by *Pulex irritans* and perhaps other human ectoparasites.

B. *Limitations on interhuman transmission of plague by human ectoparasites.* There are, however, serious limitations on the transmission of plague by human ectoparasites as a means of *commencing an epidemic.*
(a) Human bacteremia from plague: It was once argued that human bacteremia from plague could not be developed. This is no longer accepted. Human plague bacteremia occurs; but it is usually found at the height of very intense epidemics, and not at the commencement of epidemics.
(b) Blocking of *P. irritans*: It was also once erroneously argued that *P. irritans* could not be blocked. It can become blocked, but only slowly. *P. irritans,* the "eclectic" flea, by reason of its eclecticism, usually has only tenuous bonds with many of its hosts. As a result, endocrine-regulation influencing the blocking enzyme, which may be regulated in turn by endocrine activity in the vertebrate host, in *P. irritans* is weak and slow. *P. irritans* becomes blocked only slowly after close association with a vertebrate host for a long time and at the peak of an epidemic. This also is of little significance in *initiating* an *epidemic.*

C. *Epizootic-based epidemics*: The most important objection to initiation of an epidemic through interhuman transmission is that of population dynamics, as is generally acknowledged by most of those who support the idea of interhuman transmission by human ectoparasites in other circumstances.

(a) As yet there is no good evidence that an *epidemic,* as distinct from a sporadic outbreak, can be developed without the mass base of an epizootic.

(b) The few epidemic outbreaks *of* plague in temperate regions in recent time—in Russia and the U.S. especially—have had an etiology based on epizootics. Outbreaks in temperate regions in recent times identified as originating in interhuman transmission by human ectoparasites (Oporto [1899]; Essex [1910] and Aumale [1921] for example) were small and sporadic, not spreading far or with epidemic speed such as is produced by the base of a developed epizootic.

(c) Speed, direction and pattern of spread of human plague in the initial epidemic of the Black Death (1347-52), as well as those of subsequent local epidemics in early modern Europe, are not reconcilable with an epidemic originating from interhuman transmission, but are reconcilable with an epizootic-based epidemic.

I must apologize for having taken so much space to discuss this particular point, but I do not think that it can be dealt with in narrower compass.

3. "Ibn al-Wardī makes the clear assertion . . . that the disease came from inner Asia. . . . The precise location of this region is unclear, but it is certainly not Iraq and Kurdistan as Professor Norris suggests" (Dols).

A fair reading of my article does not give the impression that I suggest anything of the kind. I agree that the plague came from "inner Asia," the land of the Khitai"—in fact from the northern basin of the Caspian and the lower reaches of the Volga and Don Rivers, where it was first identified. That is the *proximate* origin. I stated that the incursion into the Caspian basin, if it came from Iraq and Kurdistan, could have happened only over many centuries because there is record of plague in Iraq only in the early Middle Ages and not later, until the fourteenth century. Apart from the historical reasons already cited, there are excellent ecological and epidemiological reasons why it is improbable that the plague came from the Far East:

(i) It would have had to cross an area (the Aral and East Caspian basins) which, in the fourteenth century, was only just beginning to recover from nine centuries of dessication. The area is unlikely to have been able to support the densities of host necessary for effective transmission of plague across the region.

(ii) The transmission of plague, as experience in other areas of the world has shown, is almost invariably from the ecological boundaries (shores of oceans, seas, lakes, and rivers) where the densities of population are high, outward to the hinterlands where they are low, as I stated in my article. It was to illustrate this point that I included a copy of the modern plague map of Soviet Asia. The same point can be illustrated by plague maps of other areas, including the Southwestern United States.

(iii) The actual Caspian shores are not subject to dessication to the same degree as are the lands to the east, since the level of the Caspian depends upon the abundance of rainfall in European Russia, not Central Asia. Hence today, as in the fourteenth century, the Caspian basin can support a much higher density of zootic population than do the lands to the east. These lands are at present in the relatively early stages of a dessication cycle, in retreat from the high water table in that area in the late eighteenth and early nineteenth centuries. The marshlands of the Aral basin, containing a high sodium content, are able to support an abundance of microtines which form the principal zootic plague foci. But in the fourteenth century the area would not have supported such populations.

4. "Professor Norris has rightly posed the question of the ultimate origin of the Black Death . . ." (Dols).

I have done nothing of the sort, having been at pains to discredit this approach of "ultimate origins." Provided the ecological circumstances are right, the host population dynamics are favourable, and the micro-organism present, plague foci may develop in a wide variety of areas. The presence of the micro-organism is a necessary condition of an outbreak, but it is not a sufficient one. The "ultimate" origin of any major outbreak is that focus in which the disease becomes epizootic or epidemic—that is, in which all these conditions are satisfied.

5. " . . . the answer (to the question of the ultimate origin of the Black Death) must lie, if anywhere at all, in the central Asian and far eastern sources; such an investigation should be our foremost *desideratum*" (Dols).

"If anywhere at all"? The sources available, thus far, are unhelpful. In the absence of conclusive documentary sources, I am naturally reluctant to accept an explanation which is ecologically improbable.

6. My statement that "none of the original accounts is strictly speaking contemporary."

I agree that this was careless, the more so in that I later cite Ibn al-Wardī, via Professor Dols. For this error, I am properly contrite.

Again I apologize for the length of my reply. Obviously this material could not have been included in the original article.

# *MEDICO-HISTORICAL NEWS AND ACTIVITIES*

## CORRESPONDENCE AND REPORTS

### CHAUNCEY D. LEAKE, 1896-1978

With the sudden death in San Francisco on 11 January 1978 of Chauncey D. Leake, the history of medicine and the history of science have lost one of their staunchest boosters. Many of us will miss the encouragement and generosity that he was never too busy to bestow.

Much of the spirit of Chauncey Leake's tireless efforts on behalf of the history of medicine is readily apparent in the introduction and notes to his 1928 translation of William Harvey's *De motu cordis*. The same enthusiasm of the young (31) University of Wisconsin professor was still much in evidence fifty years later. In 1928 he wanted to note the occasion of the three hundredth anniversary of Harvey's book, but more importantly he wished to present a version "in the language and spirit of our times." "The translation is admittedly free," he wrote in the preface, "in the deliberate attempt to present Harvey's thought in the current physiological manner. Thus while Harvey nowhere actually uses a word which may be literally rendered 'pump,' it is our habit to refer to cardiac action in some such term." In an effort to make this great classic useful and appealing to students, Chauncey added footnotes that he hoped would be helpful yet unobtrusive. His was the great desire to teach, a role he fulfilled until the very last day of his life. He took no royalties from any of his books so that prices could be kept within the student's budget. The early paperback editions of the Harvey translation thus cost only ninety cents.

Chauncey Leake was born on 5 September 1896 in Elizabeth, New Jersey. He graduated from Princeton University in the war-time class of 1917, a class that was known for its many fine writers, including George Stewart and Scott Fitzgerald. Because he was already in the Army's Chemical Warfare Service Princeton did not insist that Chauncey write his senior thesis. He was soon sent to work in laboratories of the University of Wisconsin, where after his military service he stayed on for nearly ten years. Having completed work on the pharmacological action of morphine and on anesthesia, he was awarded the Ph.D. degree in 1923 on the strength of his publications. In Madison Chauncey also became a member of William Snow Miller's active seminar in the history of medicine. Chauncey himself began teaching a summer course in the history of science in 1921. Virtually every year since then he taught a course on the history and philosophy of the health professions.

Professionally, Chauncey was a pharmacologist. He was widely noted for his discovery of divinyl ether and of carbarsone to treat amoebic dysentery. The amphetamines were discovered by Gordon Alles while working in Chauncey's laboratory. In 1928 Chauncey was invited to establish a pharmacology department in the University of California School of Medicine, where he remained until 1942 when he

Chauncey D. Leake

assumed the directorship of the University of Texas Medical Branch in Galveston. From 1955 until 1962 he was Professor of Pharmacology at the Ohio State University, and returned to continue teaching in San Francisco for the last sixteen years of his life.

Chauncey Leake, who took up public speaking at Princeton in order to overcome a bad stutter and who wrote neither an A.B. nor a Ph.D. thesis, spent much of his life productively writing and speaking. His enthusiasm and vitality were undiminished at the age of 81. Among his more than 400 papers and score of books many were historical. In the year prior to the publication of the translation of Harvey, he republished Thomas Percival's book on medical ethics. In the long, informative introduction Chauncey noted the important difference between medical ethics and medical etiquette. It amused him that only when it was reprinted fifty years later did the book really gain a wide readership.

In the 1920s Chauncey wrote about the history of anesthesia and about the secret medical society, Kappa Lambda. In 1930 he wrote interestingly on Roman architectural hygiene and the work of Vitruvius, and in the 1950s on yellow fever in Texas and on the Egyptian medical papyri. He was an active member of the councils of many societies and was president of our own Association, of the History of Science Society, and of the American Association for the Advancement of Science, to name only a few. Those interested in greater detail of Chauncey's many and varied activities are referred to a fourteen-page autobiographical article, "How I Am," in 1976 in volume 16 of *The Annual Review of Pharmacology and Toxicology*.

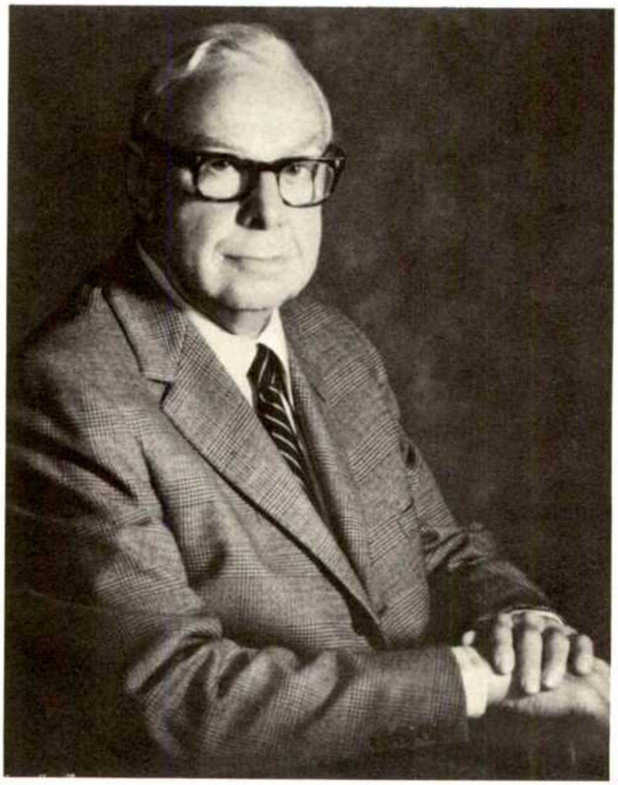
In October 1976 Chauncey and Elizabeth Leake observed their 80th birthdays and 55th wedding anniversary. Elizabeth's death came suddenly in May 1977, but Chauncey was determined to carry on, and he did so in his usual hearty style. Death came suddenly to Chauncey too, from a ruptured aortic aneurysm, but not before he was able to read from his poetry and prose to "An Evening with Chauncey Leake," at the Bohemian Club in San Francisco. Wearing his Princeton blazer and with his usual vitality and clarity, Chauncey described in his verses his enthusiasm for people, his deep care for nature and his love for the history of medicine. One of his poems was about an aging blue jay who died suddenly in pursuit of adventure and companionship, "giving what he still had to give." A more appropriate memorial one can hardly imagine.

GERT H. BRIEGER

## JOSEPH IOOR WARING, M.D.
## 1897-1977

Born on September 4, 1897, in Charleston where his ancestors settled in the late 17th century, Dr. Waring had a special love and loyalty for his native Carolina Low Country where he spent most of his life. After schooling at Porter Military Academy and Episcopal High of Virginia, he was graduated with honors in 1917 from the College of Charleston and in 1921 from the Medical College of the State of South Carolina, now the Medical University of South Carolina. After completing his residency at Roper Hospital, he pursued graduate study of communicable diseases at Willard Parker Hospital in New York and of pediatrics at Bellevue Hospital. From 1924 to 1927 he was director of a child health demonstration project supported by the Commonwealth Fund at Murfreesboro, Tennessee. In 1927 he returned to Charleston to practice medicine.

Dr. Waring was a distinguished practicing pediatrician who was interested in



Joseph Ioor Waring

medical historical studies. When he retired from medical practice in 1961 it may be said that he began a second career, that of medical historian.

During his busy and successful private practice, Dr. Waring actively participated in medical organizations. At various times he was secretary then president of the Medico-Chirurgical Club, the Medical Society of South Carolina, and the Society for the Relief of the Families of Deceased and Disabled Indigent Members of the Medical Profession of the State of South Carolina, a venerable charitable organization founded in 1848. For 17 years he was the capable editor of the *Journal of the South Carolina Medical Association*. And for many years he served as secretary, the only elected officer, of the Robert Wilson Medical History Club.

A Diplomate of the American Board of Pediatrics and member of the American Academy of Pediatrics, Dr. Waring was Clinical Professor of Pediatrics at his alma mater and was Director of Pediatrics at Roper Hospital. At the time of his passing he was chairman of the Charleston County Board of Health, a position he had held since 1968. Before that he was a member of the Executive Committee of the State Board of Health. His active participation in medical organizations and the community attests to his sincere concern for people.

Although he made his mark as an outstanding physician, Dr. Waring will be longer remembered as a medical historian. He published 44 items of a medical nature and 62 items relating to history, including medical history. His monumental work is the three volume *History of Medicine in South Carolina*, a well researched project which he pursued with love and dedication and completed in 1971. He also wrote the *History of the South Carolina Medical Association*. At one time he was a member of the Study Section of the History of the Life Sciences of the National Institutes of Health as well as a member of the Council of the American Association for the History of Medicine. On the local scene he was former president of the South Carolina Historical Society and editor of its Journal.

Dr. Waring was the recipient of honors too numerous to cite in this short memorial. In 1968 the College of Charleston accorded him an honorary degree of Doctor of Letters. The Medical University of South Carolina not only designated Dr. Waring as a Distinguished Alumnus but also memorialized him through the creation of the Waring Historical Library and the formation of the Waring Library Society.

My earliest recollection of Joe was when I was a student at the College of Charleston and he was lecturer in human physiology. Later at medical school and then in practice I came in more frequent contact with him. However, it was in small groups or at one another's home that I got to know the real Joe Waring, the kindly and unassuming man concerned for people. He was a truly gentle man, of gracious mien. I always admired his sound scholarship. His historical presentations showed that he was a quiet pace setter of excellence in historical research and an enthusiastic and meticulous researcher in South Caroliniana.

Following his death in his sleep on December 21, 1977, there was a ground swell of expressions of affection, and of recollections of how his life had touched others not only in the Carolina Low Country but throughout the State. Joe was not merely revered, he was loved. All of us are richer for having known him. He will be greatly missed and long remembered.

LEON BANOV, JR.

## JASON A. HANNAH AND THE HANNAH CHAIRS FOR THE HISTORY OF MEDICINE

Dr. Jason Albert Hannah, pathologist and medical economist, was born in Sittsville, Ontario, November 11, 1889. At the age of three he moved with his family to a Saskatchewan farm. After service overseas in World War I, he completed his secondary school education in Saskatchewan and then attended Queen's University, obtaining the degrees, B.A. in 1926, and M.D.C.M. in 1928. He was awarded a Richardson Fellowship for post-graduate study in pathology in Kingston and a George Christian Hoffman Fellowship for further study in Edinburgh. Returning to Canada in 1930, he was appointed neuropathologist to the Provincial Department of Health and a Research Fellow in the Banting Institute. Becoming

First Meeting of Hannah Professors, October 17-18, 1977, held in the Academy of Medicine, Toronto.

L to R: Dr. Toby Gelfand (Ottawa), Dr. G. R. Paterson (Hannah Institute), Dr. Ruth Hodgkinson (Queen's), Dr. Pauline Mazumdar (Toronto), Dr. Paul Potter (Western), and Dr. Charles G. Roland (McMaster).

interested in the sad financial effects of serious illnesses on civil servants, he secured a $5,000 loan and the cooperation of the Ontario Medical Association in order to establish Associated Medical Services, Inc., the first prepaid medical care plan in Ontario. He served as managing director (1937-1976) and president (1965-1976) of A.M.S.

By 1972, when the provincial government assumed responsibility for all basic health care programmes, Associated Medical Services had reserves in excess of $12,000,000. Dr. Hannah originated the concept of using those reserves on behalf of the history of medicine, in a way that is quite unique. In his plan, A.M.S. would fund a chair in the history of medical and related sciences (subsequently named in his honour) at each of the five Ontario medical faculties and would establish the Hannah Institute to represent it in its relationships with these universities and with other academic and professional bodies. Each chair would have its funding guaranteed for five years and, upon satisfactory review, funding would be continued. The occupants of the five chairs would work with the Institute to promote the gospel of history of medicine generally, but their major responsibilities would be to the appointing universities.

Identical agreements were signed between A.M.S. and the five universities individually in the autumn of 1973. Appointments made by the usual university procedures began in the next year. The first appointment was that of Dr. Paul Potter (University of Western Ontario, 1974). Subsequent appointments made were Dr. Ruth C. Hodgkinson (Queen's University, 1975), Dr. Pauline Mazumdar (University of Toronto, 1976), Dr. Charles G. Roland (McMaster University, 1977), and finally Dr. Toby Gelfand (University of Ottawa, effective July 1, 1977).

Another series of appointments should also be noted. Dr. G. R. Paterson, Professor of Pharmaceutical Chemistry, University of Toronto, was appointed Dr.

Hannah's part-time assistant in 1974. Dr. Paterson had taken a most extensive "minor" in history of pharmacy and history of sciences at Wisconsin, had taught history of medicine to pharmacy students for sixteen years, and had been one of the founders of Toronto's Institute for the History and Philosophy of Science and Technology. The A.M.S. Board named Dr. Paterson executive director of the Hannah Institute in 1975 and charged him with the responsibilities of charting the futures of the chairs and the Institute and with establishing the procedures by which A.M.S. would operate its programme.

After Dr. Hannah's retirement as president of A.M.S. in 1976, Dr. John B. Neilson, a member of the Board and an experienced medical administrator, was elected to succeed him. As part of the plan of development, a Grants Committee and a Publications Committee were established, together with programmes of support for research and graduate study, and of publication assistance (see *Bull. Hist Med.*, 1976, *50*: 450-451). The executive director serves these Committees as secretary: Committees evaluate; Board decides. Meanwhile, Dr. Neilson has led a strong new Board in obtaining supplementary letters patent (new objectives under the old charter), and charitable foundation status. An Advisory Council was created for the Hannah Institute in 1976, its membership consisting of each of the five Deans of Medicine (or their nominees), three appointees named by the Board of A.M.S. (one of them a Board member) and the Institute's executive director. The Council is chaired by Dr. Neilson. It has proven successful as a forum for University-A.M.S. relations.

Jason A. Hannah served his profession and his community on the Council and as President of the College of Physicians and Surgeons of Ontario. In turn he was himself honoured: senior member of the Canadian Medical Association (1972); F.R.C.P.(C) (1972); life member of the Ontario Medical Association and of the Academy of Medicine, Toronto. His alma mater, Queen's, awarded him an honorary LL.D. in 1974.

Dr. Hannah made his mark as pathologist and medical economist and in the history of medicine. He died after a long illness on May 2, 1977, but he lived long enough to see the creation of the Hannah Institute and to know that all five of the Hannah Chairs had been filled and would be operative by the 1977-78 academic year. The first meeting of all Hannah professors took place October 17-18, 1977.

The future looks bright for Dr. Hannah's unique programme to promote interest in medical history. A.M.S., the Hannah Institute and the Hannah Chairs have received help and encouragement from members of the American Association for the History of Medicine and look forward to the Association's 1981 annual meeting which is scheduled to take place in Toronto, Ontario.

## *FOLIA BAERIANA*

Attention is called to the fact that a new periodical, *Folia Baeriana*, containing studies relating to Karl Ernst von Baer, is currently appearing in Tallinn, Estonian SSR, under the editorship of Dr. Toomas Soot, Institute of Zoology and Botany, Vanemuise Str. 21, 202400 Tartu, Estonian SSR. The two first volumes (Vol 1, 1975; Vol. II, 1976) have reached this country. The first volume contains articles on, among various other subjects, Georg Friedrich Parrot's influence on von Baer; a re-examination of von Baer's teleological views; a list of library books signed out for and presumably read by von Baer; a comprehensive von Baer genealogy; and new information on his medical training. It concludes by reprinting his medical dissertation, originally written in Latin, in Estonian translation. The Estonian version of the dissertation is also available in reprint form. The second volume includes, among others, articles on von Baer as scientific organizer in Russia, on his relations with Burdach, on the collection of embryological literature in his library, on the Baer estate in Piep, and it also reproduces part of a diary by Andreas von Baer, Karl's great-great-grandfather (1627-1708). The articles appear in Estonian, Russian, or German. Those in Estonian are usually accompanied by summaries in German or Russian, those in Russian by German summaries. The volumes are small in format; they are well printed, bound in hard covers, and the illustrations, not numerous but fascinating, are reproduced on coated paper.

The centenary of von Baer's death was celebrated in Tartu from 29 September to 2 October 1976. The author of this note would be glad to distribute xerox copies of the program on request. Among the memorabilia distributed at the meeting, which included bookplates, postcards, etc. bearing the image of von Baer, was a delightful hardbound photographic album. This is a pictorial biography of von Baer; the illustrations are accompanied by text in Estonian, Russian, and German. Its editor was T. Ilomets, a frequent contributor to the *Folia*. The publisher was "Eesti Raamat," Tallinn, 1976.

A number of von Baer papers have been rising to the surface in recent years. Mme. T. Lukina has published, under the egis of the USSR Academy of Sciences, three volumes of letters, entitled as follows (translation): one volume of *Correspondence of von Baer on Geographical Problems* (1970), and two volumes entitled *Karl Ernst von Baer's Letters to the Members of the Petersburg Academy of Sciences* (1975, 1976). The letters are presented both in German and Russian; the apparatus is wholly Russian. Other letters in German appear from time to time in journals not always read or abstracted here: Some examples: *Baltische Hefte, Giessener Abhandlungen zur Agrar- und Wirtschaftforschung des Europäischen Ostens, Zeitschrift für Ostforschung, Lychnos,* etc.

The first, and still authoritative, biography of von Baer was written by Ludwig Stieda and published in 1878; a second edition appeared in 1886. (A more recent von Baer biography, by Raikov, was reviewed here in 1971, *45*: 85-88.) Von Baer's scientific papers had been turned over to Stieda by von Baer's family and it has long been assumed that they were lost when Stieda's house in Giessen was destroyed by bombing during the last month of World War I. They have recently come to light, however, at the University of Giessen, and the collection is being administered by Hans Querner, who is at Heidelberg.

When I wrote, as a pitchhitter, at the last moment, the biography of von Baer for the *Dictionary of Scientific Biography,* one of the editors criticized it for discussing too little embryology and too much of his accomplishments in anthropology, geology, geography and other non-biological sciences. Much of the material emanating from Estonia fortunately concentrates on those of his pursuits least familiar to embryologists. It is to be hoped that many aspects of his life and work will be newly illuminated now that von Baer studies are becoming fashionable. What is most devoutly to be hoped is that the multiplicity of new studies will avoid descending to pedantry, since von Baer himself was probably one of the least pedantic great men to have lived and written.

JANE M. OPPENHEIMER

# ANNOUNCEMENTS

## NATIONAL NEWS

### *Fellowships for Venetian Research*

The Gladys Krieble Delmes Foundation announces its 1979-80 program of pre-doctoral and post-doctoral fellowships for Research in Venice. Areas of study in the history of Venice and the former Venetian empire which will be considered are: the arts, literature, natural science, political science, economics, the law; also studies related to the contemporary Venetian environment. Applicants must be U.S. citizens, have some experience in advanced research, and, if graduate students, have completed all doctoral requirements except for the dissertation. Applications, which should be received by January 15, 1979, will be entertained for grants from $500 up to a maximum of $10,000 for a full academic year; fellows will be announced by April 1, 1979. For further information write to The Gladys Krieble Delmes Foundation, 40 Wall St., New York, N.Y. 10005.

### *Joint Atlantic Seminar*

The 14th annual meeting of the Joint Atlantic Seminar for the History of Biology took place at the National Museum of History and Technology, Smithsonian Institution, Washington, D.C., March 18-19, 1978. Members of the arrangements committee were Audrey Davis and Ray Kondratas, Museum of History and Technology, and Toby Appel, National Portrait Gallery.

### *Journal of the History of Medicine and Allied Sciences*

A useful tool for medical history research has been brought out by the *Journal*: an index of its first 30 volumes, 1946-1975. Prepared by Manfred Waserman and Carol T. Clausen of the History of Medicine Division, National Library of Medicine, the index is in an integrated, alphabetical arrangement which includes articles, book reviews, and signed notes which appeared in the "Notes and Events" section. Announcements from this section are selectively indexed. Clear differentiation is made between authors and subjects by the use of capitals and lower case in entries. The compilers' goal of utility for the user is well met. The 131-page index, priced at $10.00 (paperbound) and $17.50 (cloth), is available from: Journal of the History of Medicine, 333 Cedar St., New Haven, Conn. 06510

### *Medical History Post-Graduate Seminars*

Seminars and Symposia, Inc., a non-profit organization of physicians and dentists who are dedicated to providing continuing medical education programs, is sponsoring travel workshops combining history of medicine with a clinical topic. Nine 16-day workshops, featuring travel to Yugoslavia, Greece and Turkey, are being offered during the Spring and Summer of 1978. The topics will vary with each of the sixteen schedules, all of which will include three hours of class each day, to be conducted by two host lecturers. For additional information contact Seminars and Symposia, Inc., 505 Park Ave., N.Y. 10022.

*University of Cincinnati*

Cecil Striker was in the process of organizing a local society for the history of medicine at the time of his death in 1976. His friends and colleagues carried out his plan, naming the society in his honor. One of the objectives of the society is the expansion of the medical history collection in the new History of Health Sciences Library and Museum, University of Cincinnati Medical Center. Toward this end the Cecil Striker Fund for the History of Medicine has been established. Its specific goal is to maintain and enlarge the existing collection in the direction of (1) historical materials on medicine in the Ohio River Valley in the 18th and 19th centuries, (2) primary and secondary material on the history of diabetes, and (3) improvement of the existing history of medicine collection. A nidus of the fund exists. The Society is eager to expand upon it and will be grateful for contributions, which of course are tax deductible. Please make checks payable to "The Cecil Striker Fund for the History of Medicine" and send them to the Health Sciences Library, University of Cincinnati Medical Center, Cincinnati, Ohio, 45267.

*Woodrow Wilson National Fellowship Foundation*

Woodrow Wilson Research Grants in Women's Studies, which are made possible by the financial support of the Helena Rubinstein Foundation, are open to outstanding graduate students, both men and women. Awards are made on the basis of contribution to women's studies and the student's demonstrated ability to do outstanding work. This year's 18 winners were chosen from 175 applicants whose projects cover the spectrum of women's roles in society—in the fields of history, sociology, economics, art, literature, history of medicine and science, and others. Winners were selected by a committee of scholars noted for their work in women's studies. Research grants are intended for travel to libraries, xeroxing and microfilming, computer time, and interviewing of subject. Information about the program can be had from Judith L. Pinch, Director, Women's Studies Program, 32 Nassau St., Princeton, N.J. 08540.

*Yale University School of Medicine*

The Beaumont Club, founded in 1920, has recently established two awards to memorialize the distinguished Yale medical historians, Johns F. Fulton, physiologist and first Sterling Professor of the History of Medicine (1951-61), and George Rosen, Professor of the History of Medicine and Epidemiology and Public Health (1969-77). The generosity of Mr. Berne Dibner of the Burndy Library, Norwalk, Conn., will permit the Club to select each year in September a "John F. Fulton Fellow in the History of Medicine." The Fulton Fellow will be expected to use Yale's library and archival resources, meet with faculty and students, and present the annual "John F. Fulton Memorial Lecture." Named for Dr. Rosen and funded by Mr. Neale Watson, a New York publisher of medical historical texts, the "George Rosen Prize in the History of Medicine" will be presented every year in January to a prominent figure in the history of medicine.

## PROGRAMS OF MEETINGS

*College of Physicians of Philadelphia*

*March 15, 1978*: The Third Samuel X Radbill Lecture: "Even a Sucking Infant: 200 Years of Children's Nostrums in America," James Harvey Young.

*April 19, 1978*: The Kate Hurd Mead Class of 1888 Lecture, in conjunction with the Medical College of Pennsylvania: "Notes of a Nobel Laureate: Australia Antigen and Hepatitis," Baruch S. Blumberg.

*Handerson Medical History Society, Cleveland, Ohio*

*January 30, 1978*: A film entitled "Darwin's Bulldog," followed by commentary on Huxley by Wesley C. Williams.

*March 7, 1978*: "Historical Notes on the Evolution of Treatment for Cholera: An Iconoclastic View," Charles C. Carpenter.

*April 17, 1978*: "On the History of Blood Transfusion," Paul Dyment.

*Johns Hopkins Medical History Club, Baltimore, Md.*

*January 11, 1978*: "Archibald Garrod: A Man Whose Time Has Come," Alexander Bearn.

*March 18, 1978*: The William Harvey 400th Birthday Celebration, co-sponsored by the Departments of Medicine and Surgery of the Johns Hopkins Medical Institutions: Introduction, A. McGehee Harvey; "Harvey's Discovery in a Medical and Surgical Context," Jerome J. Bylebyl; Film, "William Harvey and the Circulation of the Blood"; "The Emergence of Cardiac Surgery: Blalock, Taussig and Beyond," Vincent L. Gott; "Cardiopulmonary Resuscitation—New Concepts," Myron L. Weisfeldt; "Management of Carotid Artery Disease," Thomas J. Preziosi and George S. Allen.

*Josiah C. Trent Society for the History of Medicine, Durham, N.C.*

*January 5, 1978*: "Hippocratic Humours," Paul Potter

*February 23, 1978*: "An Oslerian Evening," John Crellin and G. S. T. Cavanagh.

*March 16, 1978*: "Neurosurgical Reminiscences," Barnes Woodhall.

*Mayo Foundation History of Medicine Society, Rochester, Minn.*

*January 11, 1978*: "Mayo's Role in an Era of Medical History," James C. Cain.

*April 12, 1978*: "Ancestral Dietary Customs in Art," Patricia Hodgson; "Pathology in Pre-Columbian Art," Juan Ramon de la Fuente

*Medical History Society of Western New York, Buffalo*

*October 30, 1977*: "Adventures of a Medical Biographer," William Barlow; Commentator, David O. Powell.

*Society of Medical History of Chicago*

*February 16, 1978*: "Emergence of Medical Specialties in the United States," Frank W. Newell; "The Rise of Dentistry as a Special Field," Frank J. Orland.

*March 9, 1978*: Annual Morris Fishbein Lecture: "Maverick of Medicine—The Strange Case of Carl Ludwig Schleich (1859-1922)," Karel B. Absolon.

*University of Virginia Medical History Society*

*October 11, 1977*: "Sigmund Freud and the Emergence of Dynamic Psychiatry," D. Wilfred Abse.

*November 10, 1977*: "Agency of Repression? The Mental Hospital in Early Modern Europe," H. C. Erik Midelfort.

*December 8, 1977*: Motion Picture "Withering" (A Historic Re-creation of the Life and Times of Dr. William Withering and His Discovery of Digitalis): Julian R. Beckwith.

*February 14, 1978*: "Where Medicine and Poetry Meet: Reflections on the Mythic Physiology of the Renaissance," William W. Kerrigan.

*March 21, 1978*: "The Medical Frontier: New England Physicians in Virginia and the South, 1790-1840," William Barlow and David Powell.

*April 18, 1978*: "The Early Days of Thoracic Surgery," George R. Minor.

*Washington Society for the History of Medicine*

*January 19, 1978*: "The Sickness of Frederic Chopin: A Study of Disease and Society," Fitzhugh Mullen; "Americans Discover Retirement and Old Age; Dr Osler's 'Fixed Period' Address, 1905," William Graebner.

*March 23, 1978*: Boerhaave, Leeuwenhoek, and the Red Corpuscle: Discovery and Fiction," Edward Ruestow; " 'De Groot Moenster Boek' of 1736," Welmoet Bok-van Kammen.

## NEWS FROM ABROAD

*Canada*

At the meeting of the Canadian Psychiatric Association in Quebec City, October 1, 1976, the first C. B. Farrar Award in the History of Psychiatry, "for an outstanding dissertation within the History of Psychiatry published during 1974 or 1975," was presented to Dr. Jerome M. Schneck of New York City by the Clarke Institute of Psychiatry of Toronto. The research award, named in honor of the late Dr. Clarence B. Farrar, Professor Emeritus of Psychiatry at the University of Toronto and Editor of the *American Journal of Psychiatry* for more than 34 years, is to be presented bi-annually by the Prize Committee of Clarke Institute of Psychiatry. Dr. Schneck's contribution was "The History of Psychiatry in the United States of America."

*London*

A celebration of the life and work of William Harvey (1578-1657) is being held in London, August 25-September 9, 1978. Titled "Harvey 400," it is the first in a series of conferences on the British and European medical heritage which are being sponsored and organized by Jenner Old & Rare Medical Books of Oxford, under the personal direction of Dr. Nicholas Dewey, president of Jenner Books. The conference program consists of visits to sites of medical historical interest and includes lectures by Elizabeth Allen, Sir Hedley Atkins, Brian Bracegirdle, Denis Cole, Jessie Dobson, A. Rupert Hall, Richard Hunter, Kenneth Keele, D. Geraint

James, William R. LeFanu, Charles Newman, Allen Paterson, A. H. T. Robb-Smith, Malcolm Rogers, Arthur Rook, Roger V. Short, John L. Thornton and Gweneth Whitteridge. Dr. Walter Pagel is honorary patron of the conference. Further information about the conference can be obtained from Jenner Books, Church Enstone, Oxford OX7 4NL, England.

*Paris*

The third "Colloque international hippocratique" will take place in Paris, September 4-9, 1978. One of the essential aims of the Colloquium is to further the collaboration of physicians and hellenists which began at the first Coloquium in Strassburg and which is fundamental to the study of the Hippocratic writings. The Colloquium will take place at the Centre National de la Recherche Scientifique, 15 quai Anatole France, which is granting a subvention and will publish the *Actes du Colloque*. Further information can be obtained from Dr. M. P. Duminil, Université de Paris-Nanterre, 81 rue Laugier, 75017 Paris.

*Switzerland*

The Henry E. Sigerist Prize for 1977 was awarded to Dr. Markus Hämmerle for his dissertation, "Die Anfänge der Basler chemischen Industrie im Lichte von Arbeitsmedizin und Umweltschutz." The dissertation was written under the direction of Dr. Heinrich Buess, University of Basel, and will be published in the series "Basler Veröffentlichen zur Geschichte der Medizin und der Biologie."

*Death*

The *Bulletin* regretfully notes the death in London, following an accident, of Heinz Norden (1905-1978). An American citizen, he lived in England, and was well known in the States and in Europe as one of the leading experts in the world on "antiques from the healing arts"—his own term for the business which he built up in this field. He was a member of medico-historical societies in Britain and elsewhere, besides being actively engaged as a medical journalist with a specialty in ophthalmology.

# *BOOK REVIEWS*

CHARLES LICHTENTHAELER. *Geschichte der Medizin,* 2 vols. Koln-Lovenich, Deutscher Aerzte-Verlag, 1974, 736 pp. Ill. 98DM.

Dr. Lichtenthaeler's *History of Medicine* has been billed as the first comprehensive German textbook since the publication of T. Meyer-Steineg and K. Sudhoff's book of 1920. Such a view not only ignores the contributions of other authors such as Paul Diepgen's 1955 *Geschichte der Medizin,* but seemingly accords the new publication an importance which, on closer examination, it may not fully deserve.

In his preface, the author emphatically comes out for a "new" medical history, free of "presentism" and placing considerably less stress on the progressive scientific advances. The study of the past should be "living history," always relating and contributing to an understanding of contemporary medicine. Similarly, it must be "open history," free from philosophical straitjackets and stifling modern preconceptions rooted in experimental medicine.

Lichtenthaeler's explicit goal is the provision of a basic orientation about medicine to contemporary physicians who are, for the most part, ignorant about the past of their profession. Moreover, he hopes to reverse the antihistorical sentiment he detects in his German and Swiss students.

The author has divided his text into a twenty lecture format, covering medical events from primitive medicine to the present. Four of these chapters, the initial two lectures in the first volume and the concluding ones in the second volume, deal with questions of historiography and the need for a philosophy of medical history. Their presence in the book can be seen as distracting from the major "Lehrbuch" objective, but nevertheless deserve attention and analysis.

The first chapter of the book deals with the reasons Lichtenthaeler sees for a renewed interest in medical history. The arguments are by now familiar, having received a thorough airing in a recent series of articles in the journal *Clio Medica.* It is, nevertheless, reassuring to note that some of our European colleagues are subscribing to similar views. The potential of the history of medicine in the establishment of a new medical humanism remains a fundamental challenge for the present generation of historians.

A second chapter related to historiographical issues covers more familiar ground and largely repeats already accepted views. The author declares his opposition to pure biographical and anecdotal history, the projection of contemporary ideals into previous medical contexts, as well as proclaiming the need for philological studies in the field.

The actual history of medicine is presented in the remaining sixteen chapters with lectures on primitive and "archaic" medicine, Hippocratic, Alexandrian and Galenic medicine, Byzantine, Islamic and medieval healing. The second volume examines Renaissance medicine and the dawn of modern experimental medicine, each in two chapters. The treatment is uneven, often merely topical, with serious omissions and gaps that jeopardize continuity.

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One obvious drawback is Lichtenthaeler's heavy emphasis on Western medical achievements in the early chapters of the book. Both ancient Hindu and Chinese medical systems are not even mentioned, while Near Eastern developments are briefly subsumed under the term "archaic." It could be argued that these civilizations contributed only marginally to the emergence of Western medicine, but their continuous existence into our own days is certainly of more than just passing interest to contemporary physicians confronted by acupuncture and herbs. A textbook should certainly not omit these topics.

Having eloquently preached the need for social factors in medical historiography, Lichtenthaeler then often succumbs to the who, what, when treatment of the past, especially in the chapters dealing with the period between 1500 and 1800 which is divided into a series of separate currents of thought.

At the end of his work, Lichtenthaeler attempts to detect the fundamental factors which have been operative throughout history in the healing arts. "Instinct," magic, logos—a desire for rational certainty—and charity are singled out as the principal forces shaping events.

This portion of the book and the final concluding chapter seek to discover "the movements" or "tendencies" inherent in medical history. While trying to avoid the formulation of a strict historical pattern, Lichtenthaeler claims to detect a certain rhythmicity, a pendular movement between realistic and idealistic views of medical activity. His "metahistory" of medicine, he suggests, would provide a valuable basis for a future general theory of medicine. Without objecting to the author's intentions to use historical insights for the eventual formulation of a new philosophy of medicine, his analyses remain largely fragmentary and confusing as well as unsupported by the evidence already presented.

As a textbook aimed at historians, physicians and students of the field, Lichtenthaeler's synthesis is meager in actual substance and fragmentary in the selection of major topics. This is in marked contrast to the extensive historiographical and philosophical discussions accompanying the text. The reader may justifiably wonder why the methodological prescriptions were often violated and the final conclusions drawn from such a restricted exposition.

Moreover, Lichtenthaeler's work lacks notes, specific references and selected bibliography needed to encourage further reading and research on the various subjects. Instead, the author has merely assembled an alphabetically arranged bibliography of books and articles by mostly European authors.

The task of writing a general history of medicine is a difficult one, usually shunned in favor of more specialized subjects examined in more detail. Dr. Lichtenthaeler's endeavor, while showing considerable improvements over many previous bio-bibliographical treatments, falls considerably short of its stated objectives.

Reviewed by GUENTER B. RISSE, M.D., Ph.D., Professor of the History of Medicine, University of Wisconsin Center for Health Sciences, Madison, Wisconsin.

JAMES BORDLEY, III and A. MCGEHEE HARVEY. *Two Centuries of American Medicine, 1776-1976*. Philadelphia, London, and Toronto: W. B Saunders Company, 1976. xv + 844 pp. Ill. $19.75.

In the fifty or so years that have elapsed since Francis Packard published his two-volume history of medicine in the United States, several attempts have been made to update it. This present work by Bordley and Harvey is by far the most ambitious effort. It is an oversize volume of 844 pages devoted to the story of American medicine from 1776 to the present. A history inevitably reflects the interest of its author or authors, and the allocation of space in this study is revealing. The first one hundred years of medical history are covered in about 100 pages, another 285 deal with the period from 1876 to 1946, and about 400 are devoted to the last thirty years.

Each of the three sections into which the history is divided begins with a short chapter summarizing the major developments in American history. This introduction is followed by chapters on medical education, medical practice, public health and preventive medicine, hospitals, medical licensure, and medical research. The format varies slightly from section to section with much more emphasis upon medical research in the later ones. That the two authors feel much more at home in the recent period is shown by the chapter references. As the history progresses, the number of references cited for each chapter shows a marked increase—an increase which is greatest for those chapters dealing with modern medical research.

While I agree with Virchow, Sigerist, and Shryock that one cannot write the history of medicine in a vacuum, I have qualms about the three brief chapters giving a textbook sketch of American history. It may be, however, that I overestimate the background of potential readers of medical history. In any event, I think it best to weave whatever social, economic, or other factors may have influenced the course of medicine directly into the story. Part one of the book presents a reasonable brief survey of the first 100 years. Yet the last 30 years have seen a flood of historical studies on this period, and, possibly resulting from my own interest in the early period, I cannot help feeling that this century has been slighted. The two authors have not done a thorough survey of the available historical writings, and, by relying upon secondary works, they have made several errors. For example, on page 32 they quote a grim description of the 1822 yellow fever epidemic in New York City, an outbreak said to be the most severe of a series of seven epidemics. The worst epidemics in New York occurred in the 1790s, and this final one in 1822 was relatively minor for its day. On page 34 malaria is cited as prevalent in the southern states during the first half of the 19th century This is true, but it was also widespread throughout the entire midwest and as far north as New Jersey along the Atlantic coast.

The middle period, 1876-1946, is given a more comprehensive coverage, although approximately one-third of the pages are devoted to medical research. This era, however, witnessed major developments in the basic sciences and clinical research, and the authors can scarcely be faulted. By 1946 the line between medicine and the basic sciences was virtually indefinable, a situation which makes the task of dealing with recent medical history a difficult one.

Bordley and Harvey have met this challenge by discussing recent medical advances under such headings as modern drugs, cardiovascular disease, enzymes and hormones, medical genetics, immunology, virology, cancer, the neurosciences, and psychiatry. Clearly the authors are knowledgeable about modern medicine, and the reader, unless he has a good background in medicine or the biological sciences, may occasionally find himself struggling a little. As a social historian, I would have preferred a little more emphasis upon the interaction between medicine and society, yet the authors handle this aspect reasonably well. The chapter on medical education is an excellent one, undoubtedly reflecting the authors' professional interests and personal experience. They present a well-balanced picture of the role of the AMA in the 20th century, and do a good job of showing the vastly increasing role of the Federal government since World War II in all aspects of medicine.

In the chapter on modern drugs, however, a good deal of credit is given to specific ethical drug companies for their research contributions. While this credit is undoubtedly deserved, the reader cannot help wondering whether or not he is getting a balanced picture. For example, what percentage of the income of these companies goes to research as compared to advertising?

In summary, Bordley and Harvey have not given us a comprehensive history of American medicine, but they deserve credit for making the first attempt to draw together and synthesize the fundamental changes in medicine during the 20th century. They have also provided a useful reference to the work of the many outstanding American medical scientists of this age. Since I have reservations about achieving historical perspective with respect to the recent past, I would have preferred that the authors had balanced their account better and placed less emphasis upon the last thirty years. Their book, with some justification, stresses the role of science, but medicine still deals with human beings, and for all our Rube Goldberg contraptions, it still remains an art.

Reviewed by JOHN DUFFY, Ph.D., Priscilla Alden Burke Professor of History, University of Maryland, College Park, Md.

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OLWEN H. HUFTON, *The Poor of Eighteenth-Century France, 1750-1789.* Oxford: Clarendon Press, 1974. 414 pp. £ 9.00

The subject matter of this book is best defined in negative terms: the "poor" are men and women who lack steady jobs and therefore cannot afford proper shelter, clothes, or tools; who never eat enough and therefore suffer from chronic exhaustion, malnutrition, discouragement and indebtedness. They are not indigents on relief, nor patients in hospitals, nor criminals in jail—but on the verge of turning indigent, sick and criminal. The three parts of this book thus logically treat: Poverty, Relief and Criminality.

For this reviewer and, hopefully, for the readers of this *Bulletin,* the novel aspect of this book is that its concern with the poor extends to ill-health as a cause of poverty. Historians of medicine know only too well that "health" is a relative notion: personal thresholds for discomfort, irritation, noise, pain vary; even a doctor and his patient may disagree: the patient may feel sick, the doctor may find "nothing wrong." The eighteenth-century poor, this book shows in plentiful detail, were inured to constant suffering and accepted it as a condition of life. They lived with sores, rashes and aches; among flees, lice, bedbugs; to the constant accompaniment of coughing, scratching, sneezing and spitting. Malnutrition and fatigue were their ubiquitous condition; modern Western physicians would classify most of them as in ill health.

While statistics are unobtainable, estimates are permissible: Dr. Hufton believes that between one-third and one-half of all Frenchmen were either poor or indigent. Poverty is here defined as a way of life where a poor person is able to subsist by his own efforts—with the help of "an economy of makeshifts" including migration and begging: the first third of the book analyzes these. A poor person could easily become destitute: one bad harvest, the dismissal of one wage-earner in the family, the loss of the family cow—and the family ceased to be self-supporting. "Health," under such marginal conditions, hardly exists as a recognizable blessing to think about, but is rather perceived as the absence of crippling disease. Discomfort, hunger, fatigue, anxiety, even pain did not count as long as they were not disabling. The will to survive prevailed.

The second part of the book deals with "relief," and the historian of medicine is surprised to find that medical aid is barely mentioned. Upon reflection, this seems justified: this half of all Frenchmen never saw a doctor in their lives, and rarely followed a health regulation. The hospital was their last, terrifying refuge when they became destitute and acutely ill, and a place where they went to die. The "hospice" or old-age home, a residential institution, refused them entry unless they were natives to the town, had resided there, were blind, paralytic or otherwise disabled, "deserving" (that is, church-going) or had "protection." Very few qualified. The rest died in their own hovels, without benefit of a physician.

The historian of medicine would of course be interested in a discussion of epidemic disease, morbidity and mortality: Hufton alludes to these but does not analyze them in any detail—it is true that, from the point of view of the *poor,* that is, half of France, they mattered little unless they spelled starvation or death.

In hands less gentle, compassionate and humane than Dr. Hufton's this story could turn sordid. Although she piles facts on statistics to staggering heights, her conclusions are not cold fact but the human condition.

This is particularly true of the last section, devoted to the crimes of the poor which—besides vagrancy, smuggling and theft—analyzes prostitution, illegitimate births and infant mortality including infanticide. No doubt our moralistic attitude toward infanticide could profit from some honest scrutiny. We will obviously not condone murder and our national opinion about abortion is passionately divided. But to understand the historical phenomenon of three to four thousand infants dying yearly in Paris alone, for example, one must understand the mentality and living conditions of the eighteenth-century, poverty-stricken mother.

The birth of an additional child to a poor family on the brink of destitution spelled disaster. If the mother was healthy enough to nurse, could she bring the infant to work? If not, who would tend the baby? There were no clothes, no additional food, no crib, no space, no energy for tenderness. The baby was unwanted because he could not be loved or tended. There was literally no room for him in the family economy. His death would be a relief. Under those marginal conditions, acquaintance with a wet-nurse whose negligence could be relied upon would be a solution to the tragedy that an unwanted baby represented for that family. If the child were abandoned and then "found," the chances for his survival were hardly better: artificial feeding worked badly and the availability of reliable wet-nurses for the poor remained an acute problem at the foundlings' homes. Hence the staggering mortality.

This book about the poor thus analyzes poverty rather than its causes. In Dr. Hufton's perception that crucial cause of poverty, bad health, is often implicit rather than treated in detail. It seemed worth stating that this book is of great interest to readers of this *Bulletin* even if, paradoxically, the medical conditions of eighteenth-century France do not figure prominently in Dr. Hufton's text.

Reviewed by DORA B. WEINER, Ph.D., Associate Professor of History, Manhattanville College, Purchase, N.Y.

JOHN DUFFY, *History of Public Health in New York City, 1866-1966*. Vol. 2. New York: Russell Sage Foundation, 1974. xxi + 690 pp. Ill. $20.00.

Professor Duffy's 689-page second volume on the history of public health in New York completes an enormous project which has taken him more than ten years and covers in all three and a half centuries of the history of New York City. It is an encyclopaedia, a reference book which will be used by everyone writing on public health in North America for years to come. Besides detailed chronological treatment of the history of the Health Department, there are separate chapters on maternal and child health, the health of school children, and changes in the distribution of disease. All of this is extremely carefully documented, from sources which are shown both in footnotes and in a bibliography which brings them together. It lists reports, minutes, departmental records and pamphlets in the many collections he worked through, and also books, journals and newspapers, both contemporary and modern. As Professor Duffy says (p. 656), comment in contemporary newspapers is an important source for the public reaction to official public health policy.

The overall pattern of the book which is repeated in each separate section, is a chronicle of change for the better, from the stinking terrifying tenements of the nineteenth century as described in Jacob Riis' famous *How the other half lives* to the sanitized streets of 1966. The epic hero is the Department of Health itself, served badly or well by its officers, admired or attacked by the public, interfered with or supported by its politicians. The Department outlives its opponents in Tammany Hall, the hostile medical profession, and a public which soon tires of virtue.

The contributions of this book to the history of medicine will be many. Its defect is really only one: its lack of an analytical point of view. It is a lack that Professor Duffy was quite conscious of in writing the book. He says (p. 630) that while he was working, he assumed that a theme for his study would "emerge,"but none ever did; and he has honourably refused to force one on it.

It is a problem that has led him into a number of contradictions in his concluding chapter. The health reforms, he says (p. 619), were a middle class effort; the reform leaders had little success in appealing to a public which was largely illiterate. But the

Draft Riots of 1863 and the attacks on property which followed them were a product of the brutalization and degradation of the working class, and the riots, he says, "shook the complacency of the upper classes and made them realize the need for concessions." (p. 619) But the widening of the franchise to include the immigrants produced the political machines with their corruption and inefficiency (p. 618) and Tammany Hall's effort of 1870 to gain control of all the city agencies is seen as an irruption of venal politics (p. 620) which allowed a general slackness to slown down the department (p. 621). Most of the reforms were opposed by the medical profession. Their point of view was "widely separated" from that of the Health Department, as Dr. Biggs said at the New York Academy of Medicine in 1897 (p. 239); but there were many physicians in the Department: the "summer corps," for example, was a group of 40 to 50 physicians appointed each summer to work in the tenements (p. 87) and usually about a third of the members of the Board and its Commissioners were physicians.

The tables in the Appendices also need a little more analysis. One of them lists ten leading causes of death for 1866, 1916 and 1966, in absolute numbers. The categories do not match and are not defined: does "diarrhœal disease under 5" of 1866 and 1916 correspond to "certain diseases of early infancy" of 1966? Does "accidents, homicides, suicides" of 1866 correspond with "violence" of 1916 and "accidents" of 1966? There are no notes to tell us, and no sources cited for the figures in any of the appendices. They are not very useful in this form.

The lack of analysis, which makes the book difficult to read, will prevent it from being an important influence on historical thinking. But it is a book which will be needed as a guide to sources and persons in the field of public health by every historian who is working in this area.

Reviewed by PAULINE M. H. MAZUMDAR, M.B., Ph.D., Jason A. Hannah Professor of the History of Medicine, University of Toronto, Toronto, Ontario, Canada.

RONALD L. NUMBERS. *Prophetess of Health: A Study of Ellen G. White.* New York: Harper & Row, 1976. xiv + 271 pp. $10.00

Ellen G. White (1827-1915) was the prophetess around whom the Seventh-Day Adventist church finally formed in the mid-nineteenth century For many years she was the spiritual leader of this important and influential sect by virtue of her visions, through which God provided guidance to her and her followers. Even though the visions stopped in the 1870's (when she reached the menopause), she continued to be the major figure in the church.

In the summer of 1972 Ronald L. Numbers, a professional historian of science and medicine, was looking through a book by L. B. Coles, a conventional nineteenth-century American popular health writer. The volume had once been owned by the great Adventist physician, John Harvey Kellogg of Battle Creek. Numbers saw from marginal page keys that Kellogg had discovered that Ellen White's *Christian Temperance and Bible Hygiene* (1890) was based closely on Coles's work. Numbers followed up this glimpse into the health aspects of Adventism, and the present book is the result. It is not a biography, but rather an examination of

White's relationship to health movements both inside and outside of the church. Numbers also helps fill in a picture of which there are large parts in Richard W. Schwarz's works on Kellogg as well as in the standard literature on the earlier health reformers.

Much of the story has familiar elements in it. Plagued by bad health and at the same time involved in the Millerite movement, White responded to the anti-medical health reformers' programs of simple, virtuous dressing, dieting, and living habits. When she and her family benefitted from the water cure, she became devoted to a puritanical version of hydropathy. Not all Adventists followed her, especially when she tried dress reform and abstention from meat, but her devotion to health as well as her well-known temperance crusades were generally acceptable, even outside strictly church groups. Just after the Civil War, the Adventists, under White's leadership, founded their own water cure establishment, the Western Health Reform Institute in Battle Creek. Much of the book traces the interplay between nineteenth century Americans' conventional preoccupations with natural remedies, masturbation, temperance, and the like with changes in both medicine and religion, especially the accommodation between an increasingly scientific medicine, hydropathy, and religious dedication to a sound body.

Numbers shows a great deal of concern about plagiarism and inconsistency in White's writings. It is a needless concern, for her religious leadership and charisma had other bases. What is fascinating is the way that she used health ideas that were in the world around her—especially as those ideas changed. Proscribed foods, for example, at first she condemned because they roused animal passions, but later because they could harbor germs. When health first became a major public concern of White, she denounced the doctors. By the last decades of the century, however, she increasingly depended upon Kellogg, a physician who ultimately founded a medical school to train competent modern Adventist physicians. Only in the last years of her life did she work out systematically the idea that Adventist health care efforts were in fact an important avenue through which religious converts could be attracted when once medicine had allayed their suspicions of evangelists. The present world-wide network of Adventist health institutions is therefore, and properly, a legacy of both religion and medicine.

Numbers compares White with Mary Baker Eddy. Both of them raised the question of health of body and health of spirit but each answered it by turning to different traditions, the one that of mesmerism, the other that of hydropathy and medicine. Numbers does not go on to raise such speculative questions as the nature and reality of illness in the nineteenth century. Nor does he touch such intriguing problems as why Adventist health concern rose just when, according to William B. Walker, popular health reform was dying away. This book is, rather, a scholarly monograph. Almost a fifth of it is devoted to footnotes, which, with the bibliography, reveal an exemplary coverage of both secondary and primary sources, some of the latter from unexploited private archives. Knowledgeable persons may find some of the author's elementary explications of popular health materials tedious, and likewise repetitiousness from one chapter to the next. But presumably a general reader could follow the book easily. The important point is that for the novice and professional alike Numbers conveys to the reader the fascination that first drew him to the subject. He has succeeded in adding a dimension in depth and understanding to our knowledge of the transformations of popular medicine. With an unpredictable main character, choice illustrations, and unusual subjects, this book can serve for pleasurable reading as well as enlightenment.

Reviewed by JOHN C. BURNHAM, Ph.D., Professor of History, Ohio State University, Columbus, Ohio.

RENÉ J. DUBOS. *The Professor, The Institute, and DNA*. New York: The Rockefeller University Press, 1976. 262 pp. Ill. $14.50.

This is more than the story of a man, of an institute or important scientific discoveries. It is a story which epitomizes the creation of medicine's scientific base—the coming of age of science in the United States as illustrated by the unique accomplishments of a group of investigators working in an environment in which there was a true appreciation of the importance of applying the tremendous advances in physics and chemistry to the study of the problems presented by disease. Only a scientist with the rare talents of René Dubos could have so skillfully described the scientific aspects and at the same time portrayed the personality of Oswald T. Avery with such sensitivity.

Dubos has recounted the sequential steps that led to Avery's discovery of DNA as the material responsible for the transformation of pneumococcal types. In addition, he has displayed vividly the scientific approach to problems of an original and creative scientist whose activities were known previously to only a few.

Avery was brought to the Hospital of the Rockefeller Institute by Rufus Cole, its first director, to join the team studying the bacterium causing lobar pneumonia—the pneumococcus. Avery and his co-workers were responsible for making a series of discoveries of basic importance in relation to the chemical basis of biological specificity. With Heidelberger in 1923, Avery showed the antigenicity of the polysaccharide in the pneumococcal capsule—the first demonstration that substances other than proteins possess specificity. In 1944, climaxing a research career already rich in valuable contributions, Avery demonstrated, with Colin MacLeod and Maclyn McCarty, that DNA possessed specificity and was responsible for hereditary transformation of capsular types—a discovery which pushed the starter's button in the race to the double helix.

This biography is replete with lessons for those who are concerned with decision-making for the future of medical research. It is an outstanding chapter in the annals of the men, the institutions, and the impelling motivations which are the basic ingredients of scientific discovery. It gives the reader a clear insight into the way in which science progresses and provides a firm reason for its generous support. We all owe a debt of gratitude to René Dubos for this magnificent volume.

Reviewed by A. MCGEHEE HARVEY, M.D., Professor Emeritus of Medicine, The Johns Hopkins School of Medicine, Baltimore, Md.

WILLIAM R. LEFANU. *Notable Medical Books from the Lilly Library*, Indiana University. Edited by S. O. Waife, Elizabeth McCain, Ruth Weber and Charles Hammond. Indianapolis: Lilly Research Laboratories, 1976. 275 pp. Ill. $6.00.

The significance of the Lilly collection of medical books was first widely appreciated in 1965 when an illustrated catalogue of an exhibition of 200 medical and surgical classics from the collection was published. While the present volume contains many of the same books, the format and content have been significantly changed. *Notable Medical Books* consists of annotated descriptions of 130 books which may justly be called classics of medicine.

The exact purpose of the book and the contribution it makes to the literature of medical history and book collecting are somewhat difficult to define. Several factors account for this and are acknowledged by those responsible for its production. The books included are selected from the Lilly collection, and as one might expect, some major contributions to medical thought are not present. Thus, unlike "Garrison and Morton", it does not pretend to list *the* most important medical books. Bibliographical details are noticeably absent from the descriptions, but this information is readily available to the collector and librarian from other sources. The book is more than a short title catalogue such as the *Bibliotheca Walleriana* or the catalogue of the Cushing collection. Its primary purpose is to illustrate the growth in understanding of the structure and function of the human body in health and disease as documented in the major books of medical science printed over the past four centuries. *Notable Medical Books* has many things in common with H. D. Horblit's *One Hundred Books Famous in Science* published by the Grolier Club in 1964.

Of interest are the circumstances which led to the formation of this remarkable book collection. David A. Randall, late librarian of the Lilly Library, has written a delightful essay on J. K. Lilly, Jr., as a book collector and the growth of his collection. This appeared as an introduction to the 1965 catalogue of the Lilly Library, and it is unfortunate that it was not reprinted in the present volume. The insight which it provides into the collector and his special philosophy of book collecting makes it fascinating reading for any bibliophile. Joseph Kirby Lilly, Jr., former president and chairman of the board of the Eli Lilly pharmaceutical company, began collecting books as a young man. Although he acquired a few items of medical Americana in the 1930s, it was not until the late 1940s that he began to collect the classics of medicine and surgery in earnest. Afforted the luxury of great wealth, cost should have been no object, however Lilly was a very discriminating collector who maintained a budget, albeit a generous one, for his collection. Along with availability, a major challenge to Lilly was the selection of those books which best represented the major developments in medical science over the four centuries of printing. In order to define which books should be sought after, Lilly wisely enlisted the aid of William R. LeFanu, Librarian of the Royal College of Surgeons of England, who prepared a list, "Two Hundred Key Books in the History of Medicine and Surgery." With this list as a guide, and with the assistance of several book dealers around the world, Lilly began acquiring as many of the books on LeFanu's list as he could find that met his high standards in terms of condition and binding. Not all of the books were obtained and some substitutions were made, however, the majority of the books in the present catalogue were included in the LeFanu list.

The 130 books selected for inclusion in the volume are arranged chronologically according to the publication date of the original edition. They range from an incunable, the *editio princeps* of the Aphorisms of Hippocrates (Florence, 1494) to the multi-authored work on antibiotics edited by Florey, et al. in 1949. While the 16th and 17th centuries are well represented, the majority of the books included date from the 18th and 19th centuries. Each book listed is accompanied by an annotation of about 400 words and a half-tone illustration. The annotations are a synthesis of biographical details of the author, description of the contents, occasional mention of the contemporary impact of the book and the general significance of the contribution presented in the book. These observations, derived mainly from standard secondary sources, are well written and lend interesting insight into the diverse backgrounds of their authors and the impact of their publication. The information is quite accurate and typographical errors are happily absent.

The typography is excellent and the illustrations which accompany each entry are generally well-produced. The traditional facsimile title page or frontispiece reproductions have largely been replaced by representative illustrations from the book, or, in a few instances, reproductions of contemporary or more recent artistic interpretations of a medical scene bearing some relationship to the book or its author. The combination of a single illustration and lucid, brief text makes for easy reading.

Since the books selected for inclusion in this volume come from a single collection and have been limited to 130 in number, it is unfair to be too critical of the items chosen. The major discoveries and developments are well-represented, although it is surprising that individuals like Galen and Charles Darwin are not included in the book. Discoveries which have been published in periodicals have been purposely excluded, so the ether discovery and the recent monumental studies dealing with the genetic code are not represented in the volume. This decision is the right of the editors, but the issue which it raises is of interest to physicians, historians, librarians, bibliophiles and book dealers alike. The era of the single-authored biomedical monograph is coming to a close. It is being replaced by an era characterized by the multi-authored text and periodical. The impact of this gradual but distinct transition in the format for the presentation of new biomedical discoveries or interpretations has yet to be appreciated.

This book should be of interest to physicians, librarians, and bibliophiles who appreciate the book-oriented approach to the history of medicine. It is a well-produced, attractive volume which provides insight into the development of the art and science of medicine through a survey of some of its most important printed treasures. The modest cost of the volume ($6.00) brings the book within the reach of all interested readers.

Reviewed by W. Bruce Fye, M.D., Instructor in Medicine and Post-doctoral Fellow in the History of Medicine, The Johns Hopkins School of Medicine, Baltimore, Md.

# THE BULLETIN RECENTLY RECEIVED*

Walter Clement Alvarez. *Alvarez on Alvarez.* San Francisco: Strawberry Hill Press, 1977. 160 pp. Ill. $5.95.

Heinz Balmer. *Albrecht von Haller.* Bern: Verlag Paul Haupt, 1977 (Berner Heimatbucher, 119). 88 pp. Ill. Fr./DM 29.

Patricia Branca (Editor). *The Medicine Show. Patients, Physicians and the Perplexities of the Health Revolution in Modern Society.* New York: Science History Publications, 1977. viii + 280 pp. $14.95.

Chester R. Burns (Editor). *Legacies in Ethics and Medicine.* New York: Science History Publications, 1977. 326 pp. $7.95.

Sandra L. Chaff, Ruth Haimbach, Carol Fenichel and Nina B. Woodside (Compilers and Editors). *A Bibliography of the Literature on Women Physicians.* Metuchen, N.J., and London: Scarecrow Press, 1977. xi + 124 pp. $35.00.

*Circa Tiliam: Studia historiae medicinae Garrit Arie Lindeboom septuagenario oblata.* Leiden: E. J. Brill, 1974. viii + 302 pp. Ill. 76 Guilders.

Julius H. Comroe, Jr. *Retrospectroscope: Insights into Medical Discovery.* Menlo Park, Calif.: Von Gehr Press (P.O. Box 7654, Menlo Park, CA 94025), 1977. 182 pp. Ill.

Allen G. Debus. *The Chemical Philosophy. Paracelsian Science and Medicine in the Sixteenth and Seventeenth Centuries.* New York: Science History Publications, 1977. 2 vols. Ill. $60.00.

Hannah S. Decker. *Freud in Germany. Revolution and Reaction in Science, 1893-1907.* New York: International Universities Press, 1977 (Psychological Issues, vol. 11, no. 1, monograph 41). xi + 360 pp. Ill.

Milorad Dragic, Jovan Tucakov, Dragoljub Divijanovic, Vera Gavrilovic. *Harodna Zdravatvena Kultura u Sr. Srbiji* [Folk Health Culture in the Socialist Republic of Serbia], vol. 1. Beograd: Yugoslav Scientific Society for Medical, Pharmaceutical and Veterinary History, 1976. 214 pp.

Oliver S. Hayward and Elizabeth Thomson (Editors). *The Journal of William Tully, Medical Student at Dartmouth, 1808-1809.* New York: Science History Publications, 1977. xxiv + 88 pp. Ill. $15.00.

Erich Hintzsche (Editor). *Albrecht von Hallers Briefe an Auguste Tissot, 1754-1777.* Stuttgart-Wien: Verlag Hans Huber. 534 pp. Ill. Fr. 128.

Sally Smith Hughes. *The Virus: A History of the Concept.* New York: Science History Publications; London: Heinemann Educational Books, 1977. xix + 140 pp. $6.95.

Frederich F. Kao and John J. Kao (Editors). *Chinese Medicine—New Medicine.* New York: Neale Watson Academic Publications for the Institute for Advanced Research in Asian Science and Medicine, 1977. 91 pp. $7.95.

* The Bulletin reserves freedom of decision as to the publications to be included in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

Robert H. Kargon. *Science in Victorian Manchester. Enterprise and Expertise.* Baltimore: Johns Hopkins University Press, 1978. xi + 283 pp. $16.00.

Erna Lesky (Editor). *Sozialmedizin: Entwicklung und Selbstverständnis.* Darmstadt: Wissenschaftliche Buchgesellschaft, 1977 (Wege der Forschung, Bd. 273). xii + 501 pp.

James C. Mohr. *Abortion in America. The Origins and Evolution of National Policy, 1800-1900.* New York: Oxford University Press, 1978. xii + 331 pp. Ill. $12.50.

Alma M. Myer. *The Golden Page. A Biographical Novel.* Hicksville, N.Y.: Exposition Press, 1977. 231 pp. $10.00.

William A. Nolan. *Surgeon under the Knife.* New York: Dell Publishing Co., 1977. 221 pp. $1.95 (paperbound).

June Osborn (Editor). *History, Science and Politics. Influenza in America, 1918-1976.* New York: Prodist, 1977. 135 pp. Ill. $7.95.

Philip Rhodes. *The Value of Medicine.* London: George Allen & Unwin, 1976. 158 pp. £4.95.

Fred Rosner. *Medicine in the Bible and the Talmud Selections from Classical Jewish Sources.* New York: Yeshiva University Press, 1977 (The Library of Jewish Law and Ethics, vol. V). xiii + 247 pp. Ill. $12.50.

David A. Rottenberg and Fred H. Hochberg. *Neurological Classics in Modern Translation.* New York: Hafner Press; London: Collier Macmillan, 1977. ix + 316 pp. Ill. $22.50.

K. F. Russell. *The Melbourne Medical School, 1862-1962.* Melbourne University Press, 1977 (U.S. Distributor: ISBS, Inc., P.O. Box 555, Forest Grove, OR 97116). xiv + 277 pp. Ill. $30.00.

Nancy G. Siraisi and Luke Demaitre (Editors). *Science, Medicine and the University: 1200-1550. Essays in Honor of Pearl Kibre.* St. Louis: St. Louis University Library, 1976 (Manuscripta, vol. XX, nos. 2 and 3). 76 + 252 pp. Ill.

Leader Stirling. *Tanzanian Doctor.* With an Introduction by Julius K. Nyerere. Montreal: McGill-Queen's University Press, 1977. 138 pp. Ill. $10.95.

E. Ashworth Underwood. *Boerhaave's Men at Leyden and After.* Edinburgh: Edinburgh University Press, 1977. vii + 227 pp. Ill.

Mary Roth Walsh. *Doctors Wanted: No Women Need Apply. Sexual Barriers in the Medical Professor, 1835-1973.* New Haven & London: Yale University Press, 1977. xxiii + 303 pp. $15.00.

Donald J. West and Alexander Walk (Editors). *Daniel McNaughton, His Trial and the Aftermath.* London: Gaskell Books, 1977. vii + 185 pp. Ill. £5.00 (paper); £8.50 (hardbound).

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## CONTRIBUTORS TO THIS NUMBER

DARREL W. AMUNDSEN, M.A., a doctoral candidate in history, University of British Columbia, is Associate Professor of Classics, Western Washington State University, Bellingham, Wash. 98225

LEON BANOV, JR., M.D., is Clinical Assistant Professor of Surgery, Medical University of South Carolina. His address is 103 Rutledge Ave., Charleston, S.C. 29401

GERT BRIEGER, M.P.H., M.D., Ph.D., is Professor and Chairman, Department of the History of Health Sciences, University of California, San Francisco Medical Center, San Francisco, Calif. 94143

MICHAEL W. DOLS, PH.D., is Assistant Professor of History, California State University, Hayward, Calif. 94542

JAMES GOODYEAR, M.A., is a doctoral candidate in the Department of History The Johns Hopkins University, Baltimore, Md. 21218

JOHN NORRIS, PH.D., is Professor of History, University of British Columbia, 2075 Wesbrook Place, Vancouver, B.C., Canada V6T 1W5

JANE M. OPPENHEIMER, Ph.D., is William R. Kenan, Jr. Professor of the History of Science, Bryn Mawr College, Bryn Mawr, Pa. 19010

W. ROBERT PENMAN, M.D. is in the practice of obstetrics and gynecology. His address is Box 537, 20 State Road, Paoli, Pa. 19301

CHARLES G. ROLAND, M.D., Secretary-Treasurer of the American Association for the History of Medicine, is Jason A. Hannah Professor of the History of Medical and Related Sciences, McMaster University, 100 Main Street West, Hamilton, Ont., Canada L8S 4J9

FRANZ ROSENTHAL, Ph.D., is Sterling Professor of Near Eastern Languages, Graduate School, Yale University, New Haven, Conn. 06520

ILZA VEITH, Ph.D., D. Med. Sci., is Professor and Vice-Chairman, Department of the History of Health Sciences, and Professor, Department of Psychiatry, University of California, San Francisco, Calif. 94143

JAMES C. WHORTON, Ph.D., is Associate Professor, Department of Biomedical History, University of Washington, Seattle, Wash. 98105

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# Bulletin of the History of Medicine

THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

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# Bulletin of the History of Medicine

THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
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VOLUME 52 SUMMER 1978 NUMBER 2

## *THE FIELDING H. GARRISON LECTURE**

## TAKING THINGS APART AND PUTTING THEM TOGETHER AGAIN

JANE M OPPENHEIMER

I am greatly honored to have been invited to deliver the Garrison Lecture, and I am happy to pay tribute to Garrison by doing so. I only hope that my esteem for him and for this Association will be adequately expressed by my presentation.

By "taking things apart and putting them together again" I do not have in mind anything so simple as direct surgery. By taking things apart, I mean the isolation of animal parts in culture, at least this is the aspect of it I intend to talk about. By putting them together again, I mean transplantation or grafting in animals and man. I will talk about the procedures of isolation and of transplantation separately, not as related to each other, insofar as history permits me to do so. I am confining myself in this lecture solely to discussion of studies on animals and man. Both in the processes of taking things apart and of putting them together again, investigators who have worked with plants have been among the most important; their progress however has been taken up in other publications. *Embryonic* tissue culture and *embryonic* transplantation did have some historical relationships with each other that I myself have already discussed, in a lecture delivered at the College of Physicians of Philadelphia in 1971, and published in its *Transactions*,[1] so they will receive only brief attention here.

### *Taking Things Apart*

As for taking things apart, in the sense of keeping those things alive, I have written on this subject historically in a number of chapters of books or papers;

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* Delivered at the 50th annual meeting of the American Association of the History of Medicine, Madison, Wisconsin, May 12, 1977

[1] J M Oppenheimer, "Historical relationships between tissue culture and transplantation experiments," *Trans & Studies Coll Phys Phila*, 1971, 4th Ser, *39* 26-33

the 1971 article in the *Transactions of the College of Physicians* is only one of them. I have discussed there and elsewhere, as have a number of others, Ross Harrison's role in establishing tissue culture as a useful method of investigation. Both I and a number of other writers have mentioned investigators who made stabs at the procedure before he did, whether or not he knew it (in some cases he did, in others he did not). It has transpired that a number of times, as I have prepared lectures or articles on other subjects or simply as I have read, I have stumbled upon more and more of his predecessors. Some of these I have mentioned in publications that some members of this Association are unlikely to have read, in articles on highly diverse topics, so I should like to assemble them here. It often seems to me that each time I read about anything I come across something new in this area.

It was in 1907 that Harrison isolated in frog lymph portions of the nerve tube of the larval frog and observed the outgrowth of the nerve fiber.[2] That was the beginning of tissue culture as we know it. He was solving experimentally an embryological problem. Wilhelm Roux early in his career insisted on the methodological importance of studying embryonic parts in isolation, and he himself, in 1888,[3] 1895,[4] and 1896,[5] isolated amphibian blastomeres to study their behavior. Chabry had done the same in 1887,[6] with ascidians; Carl Chun had begun it in Naples in 1880[7] with ctenophores. An even earlier embryologist of sorts who began to study isolated embryonic fragments was Ernst Haeckel of recapitulation fame. In 1869[8] he separated sponge larvae at the blastula stage into halves and quarters to ask a question that we now know cannot be answered by such an experiment: do sponges develop by the coming together of amebae? I came across this bit of information in what even in the dim past when I was a student was a very obscure monograph on sponges. This is apparently the kind of literature that one has to read to know all that one should; not many people even know that it exists today.

---

[2] R G Harrison, "Observations on the living developing nerve fiber," *Anat Rec* , 1907, *1* 116-118, *Proc Soc Exp Biol & Med* , 1907, *4* 140-143.

[3] W Roux, "Beiträge zur Entwickelungsmechanik des Embryo Ueber die kunstliche Hervorbringung halber Embryonen durch Zerstörung einer der beiden ersten Furchungskugeln," *Virchow's Arch f path Anat u Physiol u f klin Med* , 1888, *114* 113-153, 289-291. Reprinted in English translation in B. H. Willier and J M Oppenheimer, *Foundations of Experimental Embryology*, 2d ed. (New York Hafner Press, 1974), pp 2-37.

[4] W. Roux, "Ueber den 'Cytotropismus' der Furchungszellen des Grasfrosches (Rana fusca)," *Roux's Arch f Entw -mech d Org* , 1895, *1* 43-68; 161-202.

[5] W Roux, "Ueber die Selbstordnung (Cytotaxis) sich 'berührender' Furchungszellen des Froscheies durch Zellenzusammenfügung, Zellenentrennung und Zellengleiten," *Roux's Arch f Entw -mech d Org* , 1896, *3* 381-468

[6] L Chabry, "Contribution à l'embryologie normale et tératologique des ascidies simples," *J de l'anat. et physiol* , 1887, *23* 167-319

[7] C Chun, *Die Ctenophoren des Golfes von Neapel*, in *Fauna und Flora des Golfes von Neapel* Monographie I (Leipzig W Engelmann, 1880)

[8] E Haeckel, *Zur Entwickelungsgeschichte der Siphonophoren* (Utrecht C van der Post, Jr , 1869)

I found out during the preparation of a lecture on cells and organizers[9] (1970) that Robert Remak had isolated cells; to learn this, one has to read his 1855[10] monograph which was the first to emphasize the cellular constitution of the germ layers. That is not only not very popular reading today; it has been largely neglected for some time. The only early investigator I came across who ever even mentioned its passage on cell isolation was Walther Vogt, the embryologist who invented vital staining in the 1920s. Vogt referred to it in a paper *he* wrote on isolated cells in 1913,[11] a paper which also seems rarely if ever to have been read by historians of tissue culture. Lewis Rubin,[12] in a current excellent paper of which more will be said below, refers also to attempts by Vulpian, reported in 1859[13] to have grown fragments of frog embryos in water. Also, Stricker in 1864[14] studied the change of cell shape and the division of frog blastomeres in water.

One of my more recent findings on the isolation of embryonic cells goes back to an illustration in a publication that appeared in 1862. It was in the fourth (and final) volume of Louis Agassiz's *Natural History of the United States*, that I was examining while helping a graduate student to write a master's paper on Agassiz.[15] In one of the plates he illustrates what he calls a segment isolated from the 32-cell stage of a developing medusa; he did not yet call it a cell, all those years after Schleiden and Schwann. He described it, in the caption for the figure, simply as "one of the segments, isolated." The isolation went unmentioned in the text, and there is no way to know how long it lived, or whether he tried to maintain it alive.

Listing embryological predecessors is relevant if one looks towards work leading up to Harrison's; Harrison after all was essentially an embryologist. But related work at the time was also being done by physiologists, surgeons, and pathologists. (In fact, some of the embryological work was itself physiological in motive; Remak, for instance, was interested in the membrane activities of cleavage cells isolated in sugar solution, and Stricker in the change of shape of cells in water.)

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[9] J M. Oppenheimer, "Cells and organizers," *Am. Zool.*, 1970, *10* 75-88

[10] R. Remak, *Untersuchungen uber die Entwickelung der Wirbelthiere* (Berlin G Reimer, 1855)

[11] W. Vogt, "Ueber Zellbewegungen und Zelldegeneration," *Anat Hefte*, 1913, *48* 5-64

[12] L P Rubin, "Leo Loeb's role in the development of tissue culture," *Clio Medica*, 1977, *12* 33-56

[13] A Vulpian, "Note sur les phénomènes de développement qui se manifestent dans la queue des très jeunes embryons de grenouille, après qu'on l'a separée du corps par une section transversale," *C R Acad Sci* , 1859, *48*. 807-811

[14] S. Stricker, "Mittheilungen über die selbstständigen Bewegungen embryonaler Zellen," *Sitz -ber d k. Akad Wiss Wien*, math -nat Cl., 1864, *49* (Abt II) 471-476.

[15] L Agassiz, *Contributions to the Natural History of the United States of America*, Vol. IV, Plate XXXI (Boston· Little, Brown & Co., 1862)

Among physiologically-inclined investigators of adult cells, Max Schultze in 1865 designed experiments to test the effect of increase in temperature on the movements of white corpuscles.[16] In the 1971 Philadelphia article I briefly summarized the work of five or six isolationists concerned with adult tissues (Arnold 1887,[17] Leo Loeb beginning in 1897,[18] Ljunggren 1898,[19] Jolly 1903,[20] and Beebe and Ewing 1906[21]). Rubin, whose paper concentrated on Leo Loeb's role in the development of tissue culture, listed also work by K. Bodon which had escaped me; Bodon had reported in 1903[22] on the maintenance of monocytes *in vitro*, according to Rubin. Rubin also refers to attempts by von Recklinghausen[23] to maintain amphibian erythrocytes *in vitro* (1866, the year after Schultze). It seems to have escaped both Rubin and me, earlier, that Wentscher (1898)[24] described experiments not unlike Ljunggren's in an article entitled (I translate): "Experimental studies on the independent life of human epidermal cells outside the organism."

I suppose that tomorrow I may read another book or article that will add to the list, and that Rubin will read a different one. We have neither of us claimed to have drawn up complete catalogues. I am not sure, in any event, what moral there is to draw from what is assembled here. I simply wished to get it all together, as one more illustration of the usual historical fact that pioneers have often been preceded by other pioneers. After all, Harvey did not make anything of it when he studied the living heart in isolation. He simply stated in a matter-of-fact fashion:

---

[16] M. Schultze, "Ein heizbarer Objecttisch und seine Verwendung bei Untersuchungen des Blutes," *Arch f mikr Anat*, 1865, *1* 1-42.

[17] J. Arnold, "Ueber Theilungsvorgänge an den Wanderzellen, ihren progressiven und regressiven Metamorphosen," *Arch. f mikr Anat*, 1887, *30* 205-310.

[18] L Loeb, "Ueber Transplantation von weisser Haut auf einen Defekt in schwarzer Haut und umgekehrt am Ohr des Meerschweinchens," *Roux's Arch f. Entw -mech d. Org.*, 1897, *6* 1-44. *Ueber die Entstehung von Bindegewebe, Leukozyten und roten Blutkorperchen aus Epithel und uber eine Methode isolierte Gewebsteile zu zuchten* (Chicago: Stern, 1897). See Rubin, n. 12 above, for further detail with respect to Loeb.

[19] C. A. Ljunggren, "Von der Fähigkeit des Hautepithels, ausserhalb des Organismus sein Leben zu behalten, mit Berucksichtigung der Transplantation," *Deutsche Ztschr f Chir*, 1898, *47* 608-615

[20] J. Jolly, "Sur la durée de la vie et de la multiplication des cellules animales en dehors l'organisme," *C. R. Soc. Biol*, 1903, *55*· 1266-1268.

[21] S P Beebe and J Ewing, "A study of the biology of tumour cells," *British Med J*, 1906, *2*. 1559-1560.

[22] K. Bodon, "Die morphologischen und tinktoriellen Veränderungen nekrobiotischer Blutzellen," *Virchow's Arch f. path. Anat u. Physiol. u f. klin. Med*, 1903, *173* 485-511

[23] F D von Recklinghausen, "Ueber die Erzeugung von rothen Blutkorperchen," *Arch f. mikr. Anat*, 1866, *2*· 137-139.

[24] J. Wentscher, "Experimentelle Studien über des Eigenleben menschlicher Epidermiszellen ausserhalb des Organismus," *Beitr z. path Anat. u z allg Path*, 1898, *24* 101-162

> [the heart] contracts all over, but particularly to the sides, so that is looks narrower and longer. An isolated eel's heart placed on a table or in the hand shows this well, but it may also be seen in the hearts of fishes and of cold-blooded animals in which the heart is conical or lengthened.[25]

He was equally nonchalant when he described the movements of pieces of the heart separated from each other, and movements of other tissues separated out of the living body:

> The heart of an eel, of certain fishes, and even of other animals, may beat without the auricles Even if it is cut in pieces, the separate parts may be seen to contract and relax. So even after auricular movement has stopped, the body of the heart may beat and pulsate. . . . The same thing is noted in the flesh of eels, which continues to wriggle even after skinning and slicing in pieces.[26]

### *Putting Things Together Again*

Both I, in my 1971 Philadelphia lecture, and Rubin, in his article on Leo Loeb, observed that in the pre-1907 days before Harrison's discovery of tissue culture, isolation experiments were intimately related to grafting experiments, for instance as when Loeb himself showed that grafted epithelium could grow and migrate into blood clots though separated from neighboring cellular tissues. This was not the exception; it was almost the style. In the first section of this lecture, my *terminus ad quem* for isolation experiments has been the year 1907. Born[27] and Harrison[28] began embryonic grafting in the years 1894 through 1897, so that in this section of my lecture my *terminus* will be the year 1897. If that is my omega, my alpha will not be those early skin grafts and rhinoplasties begun long ago by the Hindus, or Trembley's and Baker's grafting experiments in polyps, or John Hunter's transplantation of teeth and other things in the eighteenth century. I documented earlier that Born in his definitive 1896 paper on embryonic grafting knew some of the contemporary literature on mammalian transplantation, but all I said at that time was that "transplantation of adult tissues in vertebrates, including mammals, had a long and honorable history"[29] that I would not then enter into. Here I shall present a few salient facts about some of the attempts at grafting made between the middle and the end of the nineteenth century.

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[25] W. Harvey, *Exercitatio anatomica de motu cordis et sanguinis in animalibus,* trans. C. D Leake (Springfield, Ill , and Baltimore. Charles C Thomas, 1931), p. 29

[26] *Ibid* , p. 42

[27] G Born, "Ueber die künstliche Vereinigung lebender Theilstücke von Amphibienlarven, *72 Jahresber. d Schles Ges. f vaterland. Kultur*, I. Abth , Medicin Sektion, Sitzung von 8 juni 1894, S 79-91, "Ueber Verwachsungsversuche mit Amphibienlarven," *Roux's Arch, f Entw -mech d Org* , 1896, *4* 349-465

[28] R. G. Harrison, "The growth and regeneration of the tail of the frog larva Studied with the aid of Born's method of grafting," *Roux's Arch f Entw -mech d Org* , 1898, *7* 430-485 The experiments were begun in 1897

I suppose that if Rubin and I were to spend long enough at it, we could find most of the pre-explantation literature. It would take a large number of computers to locate descriptions of all the mid-to-end nineteenth-century attempts at grafting. When I was working in Bryn Mawr my own intracranial computer culled, merely from the non-clinical literature in our small Department Library at Bryn Mawr, well over 100 references to pre-1897 transplantation efforts—that was without benefit of the *Index Catalogue* or of any other similar aids. When I finally got as far as Philadelphia I found one single book[30] dated 1901 that contained 250 references over and above our own. The total number must be astronomical and since I cannot begin to cover the whole literature I shall merely single out some of the work that seems interesting to me. All I have to say may be familiar to those of you who are surgeons, but I found some unexpected surprises.

I should like to emphasize that by "putting things together again" in my title I do not mean necessarily putting back the same things that were taken away; that was attempted for noses and ears and fingers way back when. In the latter half of the nineteenth century auto- and homotransplants, even heterotransplants of organs and tissues were performed, as we shall see, but replacement of major compound body parts by themselves in replantations began I think only in 1906 when Carrel and Guthrie amputated a dog's thigh and put it back again.[31] This was not designed as an acute experiment, and the circulation was reestablished, but infection required that the animal be sacrificed at 10 days. Russian investigators grafted whole dog heads (supernumerary ones) in the 1950s or 1960s;[32] these also proved to be short-term experiments. But even in the 19th century complex body parts were used for grafting if not for replantation. Paul Bert, Claude Bernard's best-known student, in 1863 implanted pieces of rats' tails subcutaneously, noting in his dissertation that tails from young donors grew while those from older ones did not, although they were equally well vascularized. When 12-day old rats were used as donors, the implanted tails grew from 2.5 cm to 9 cm in about 3½ months. Bert also implanted rat embryos into the peritoneal cavity, but they were resorbed.[33]

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[29] Oppenheimer, 1971, *op cit* (n 1 above), p 31

[30] F Marchand, *Der Process der Wundheilung mit Einschluss der Transplantation* (Stuttgart: Ferdinand Enke, 1901). Marchand, whose book was dedicated to Virchow for his 80th birthday, devoted 12 out of its 38 chapters to transplantation.

[31] A Carrel and C C Guthrie, "Results of explantation of the thigh," *Science*, 1906, *23* 393-394

[32] R. Baumann, L Sieke and W P Demichow, "Elektroencephalographischer Nachweis einer humoralen Uebertragung nervaler Wirkstoffe auf Transplantatgehirne nach elecktrokortikalen Reizungen der Tragertiere," *Acta Biol Med. Germ* , 1960, *5* 504-544

But let us abandon for the moment attempts to graft complex entities, since the prevailing fashion was to use smaller parts. One of the early nineteenth-century workers to transplant organs was Berthold, who in 1849, in Gottingen, grafted testes ectopically in roosters and with some success.[34] He knew that the grafted testes continued to produce spermatozoa, and thought that they exerted masculinizing effects on the hosts via the bloodstream. We shall return to gland transplants later. Already in his day Berthold accepted as fact that skin could be transplanted from one part of the body to another. Skin was very much a favorite stuff to transplant in those days. Hamilton,[35] in Buffalo in 1854, treated ulcers by what he called anaplasty. Skin was also a particularly popular tissue for grafting in Paris: in 1869 Reverdin[36] showed that skin fragments about 2 to 3 mm square could be grafted onto the granulation layer of healing wounds where they became islands of epithelium, thus accelerating the wound healing. It was about five years later that Thiersch (1874)[37] showed that these islands arose from portions of epithelium remaining in the granulation tissue. Ollier, presented to the Academy of Sciences by Claude Bernard, was using larger fragments several centimeters square, including the whole dermis, in 1872.[38] These he thought closed the wounds themselves, rather than encouraging natural epidermis formation. While these results were presented only in 1872, he had already reported in 1861 that frozen periosteum placed under the skin of another animal could not only recover but produced bone.[39] Accordingly, he froze the cutaneous tissue with ice and salt before he grafted it—an early beginning for cryogenics? Ollier was still reporting on skin grafting in 1898.[40]

[33] P Bert, *De la greffe animale* (Paris: Baillière et fils, 1863).

[34] Berthold [no initial given], "Transplantation der Hoden," *Arch. f Anat., Physiol u. wiss Med Berlin*, 1849, [no vol #] pp. 42-46

[35] F. H Hamilton, "Elkoplasty, or old ulcers treated by anaplasty," *N Y J Med* , 1854, N S *14* 165-173

[36] J. L Reverdin, "De la greffe épidermique," *Arch. gén. de méd* , 1872, *19* 276-301; 555-586; 703-711 This describes Reverdin's first case which he presented at the Société de Chirurgie, 8 December 1869.

[37] C Thiersch, "Ueber die feineren anatomischen Veränderungen bei Aufheilung von Haut auf Granulationen," *Arch f d klin Chir* , 1874, *17* 318-324

[38] L J Ollier, "Des greffes cutanées," *C R Acad Sci* , 1872, *74* 817-819

[39] L J Ollier, "Nouvelle note sur les greffes périostiques," *C R Acad Sci* , 1861, *52* 1086-1089.

[40] L J Ollier, "Des greffes autoplastiques obtenues par la transplantation des larges lambeaux dermiques. De leur stabilité et des modifications tardives qu'elles subissent," *C. R Acad Sci.*, 1898, *126* 1252-1255, "Des modifications subies par les lambeaux dermiques dans la greffe autoplastique et des conditions qui favorissent leur accroisement en surface," *ibid* , *126*· 1316-1319

I believe we are supposed now to pay proper attention to contributions by women. Skin seems to have been the first tissue used by a lady in mammalian transplantation research: Mademoiselle Cl. Deflandre was the junior author, with P. Carnot the senior one, of reports to the Paris Société de Biologie, made in 1896, on the transplantation of pigmented skin in the guinea pig.[41] Transplantation of pigmented skin to nonpigmented areas was already a popular pursuit at that time. It was begun at least as early as 1888 when Karg exchanged skin in man between black and white individuals.[42] Marchand[43] discussed this subject quite extensively.

But Mlle. Deflandre was not the only lady engaged in grafting experiments during our period. The following year Florence Peebles was performing grafts on Hydra, and this at Bryn Mawr College.[44] Slightly beyond our time-limit, in 1908, but still at Bryn Mawr College, she was interchanging limbs of four-day chick embryos by transplantation.[45] Bryn Mawr was broadmindedly supporting male research on grafting too. Thomas Hunt Morgan, before he became the Hero of the Era of the Gene, was performing interspecific transplantation of frog tails while he was teaching there in 1899.[46]

Skin may have been an obvious tissue to begin work with. Perhaps testes were too, known from millennia of animal husbandry to be both easily accessible and also dispensible in domestic animals. More surprisingly, another favorite tissue early to be transplanted was nerve, particularly in experiments by Philipeaux and Vulpian beginning in 1861.[47] Their experiments were carried out in Flourens' laboratory. They placed excised nerve fragments into the inguinal region of dogs or pigs, or below the surface of the thigh of chickens. Their concern was for regeneration and in two cases they thought they observed it, erroneously of course, since they transplanted peripheral nerve only. Vulpian in 1874 was unable to confirm the results,[48] as were Ranvier in

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[41] P Carnot and C Deflandre, "Persistance de la pigmentation dans les greffes épidermiques," *C. R Soc Biol* , 1896, Sér 10, *3* 178-180; "Greffe et pigmentation," *ibid* , *3* 430-432

[42] Karg [no initial given], "Studien über transplantierte Haut I Entwickelung und Bedeutung des Hautpigments," *Arch f Anat. u Phys , Anat Abt* , 1888, *12*. 369-406.

[43] Marchand, *op cit* (n 30 above), pp. 411-413

[44] F. Peebles, "Experimental studies on Hydra," *Roux's Arch. f Entw.-mech. d Org.*, 1897, *5* 794-819

[45] F. Peebles, "On the interchange of the limbs of the chick by transplantation," *Biol. Bull.*, 1910, *20* 14-18

[46] T H. Morgan, "Regeneration of tissue composed of parts of two species," *Biol Bull* , 1899, *1*. 7-14.

[47] J M Philipeaux and A Vulpian, "Note sur la régénération des nerfs transplantés." *C R. Acad Sci* , 1861, *52* 849-851

[48] A Vulpian, "Note sur la régénération dite autogénique des nerfs," *Arch d physiol norm. et path* , 1874, Sér 2, *1* · 704-714.

1878[49] and Korybutt-Daskiewicz the same year.[50] The latter transplanted nerve fragments into frog lymph sacs—an early anticipation of the later use of frog lymph as an isolation medium. Philipeaux and Vulpian in 1870[51] placed nerve sections in gaps in interrupted nerves, without success, but Glück in 1880,[52] von Bungner in 1890,[53] and von Notthaft in 1893[54] all knew that regenerating central fibers could cross the nerve bridges to reach the far side.

One of the more surprising *reported* successes, in 1890, was brain transplantation. An anonymous note in *Science* for that year reports on an article in the *New York Medical Journal* by W. Gilman Thompson, M.D. and as summarized in *Science* part of it reads as follows:

> It occurred to Dr. Thompson recently, while studying cerebral localization in the lower animals, that it would be interesting to graft a piece of brain tissue from one side of a dog's brain to the other, or from one animal's brain into another's, and study its vitality. Of course, he had no expectation of being able to restore abolished function by the operation, but the question of vitality of the brain tissue and the course of its degeneration is a subject which is of very wide interest. The first experiments were preliminary, made in order to ascertain whether the transplanted brain would be immediately absorbed or would slough away.
>
> No microscopic examination was made in connection with these experiments, as it was intended only to determine the possibility of the transplanted tissue adhering. . . .
>
> Dr. Thompson thinks the main fact of this experiment—namely, that brain tissue has sufficient vitality to survive for seven weeks the operation of transplantation without wholly losing its identity as brain substance—suggests an interesting field for further research, and he has no doubt that other experimenters will be rewarded by investigating it.[55]

By 1897, additional vertebrate tissues, organs or their parts grafted included cornea (1818),[56] bone (1863),[57] bone marrow (1870),[58] uvular mucous epithelium (1871),[59] teeth (1872),[60] labial mucous epithelium, periosteum and conjunctiva (1873),[61] Fallopian tube (1874),[62] cartilage (1878),[63] tendon and striated muscle (1881),[64] thyroid (1883),[65] choroid (1884),[66] ova (1890),[67] mucous epithelium of the small intestine (1890),[68] pancreas (1892),[69] ovary (1895),[70] and spleen (1897).[71] Féré and Elias[72] grafted whole embryonic eyes under the skin of birds in 1897. Ribbert, who was soon to become more important, slips under the 1897 wire only by grafting tissues already tried by others, but the following year, after our deadline, he grafted mammary gland, and fragments of liver and kidney which he said only sometimes grew successfully.[73] Whole kidneys had to await the new century and twentieth-century blood-vessel-splicing; kidney was used as a host tissue in 1884 (Zahn).[74]

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[49] L Ranvier, *Leçons sur l'histologie du système nerveux* (Lyon F. Sauy, 1878), *2* 80.

[50] Korybutt-Daskiewicz [no initial given], *Degeneration und Regeneration markhaltiger Nerven* Inaug - diss. Strasburg, 1878, S 31 Cited from E Neumann, "Einige Versuche uber Nerventransplantation," *Roux's Arch. f Entw -mech d Org* , 1898, *6* 526-535

[51] J M. Philipeaux and A Vulpian, "Note sur des essais de greffe d'un tronçon de nerf lingual entre les deux bouts du nerf hypoglosse, après excision d'un segment de ce dernier nerf," *Arch d physiol norm et path* , 1870, *3* 618-620

[52] T. Glück, "Ueber Neuroplastik auf dem Wege der Transplantation," *Arch f klin Chir* , 1880, *25* 606-616

[53] von Bungner [no initial given], *Habilitationsschrift*, Jena, p 62 Cited from von Neumann, *op cit* (n 50 above).

[54] A von Notthaft, "Neue Untersuchungen über den Verlauf der Degenerations- und Regenerationsprocesse am verletzten peripheren Nerven," *Ztschr f wiss Zool* , 1893, *55* 134-188

[55] Anon., "Successful brain grafting " *Science*, 1890, *16* 78-79

[56] F. Reisinger, "Die Keratoplastik (organische Wiederersetzung der Cornea), ein Versuch zur Erweiterung der Augenheilkunde," *Bayer'sche Annalen für Abhandlungen Erfindungen und Beobachtungen aus dem Gebiete der Chirurgie, Augenheilkunst und Geburtshülfe, 1* (1824/25), Stück 1, 207-215.

[57] J Wolff, "Die Osteoplastik in ihren Beziehungen zur Chirurgie und Physiologie," *Arch f klin Chir*, 1863, *4* 183-195.

[58] A. Baikov, "Ueber Transplantation von Knochenmark," *Centralbl. f d med Wiss*, 1870, *8*· 371-373.

[59] V Czerny, "Ueber Propfung von Schleimhautepithel auf granulirende Wundflächen," *Centralbl f d med. Wiss*, 1871, *9* 257-259

[60] C. Legros and E Magitot, "Greffes de follicules dentaires et de leurs organes constitutifs isolément," *C. R. Acad Sci*, 1872, *78*· 357-360

[61] J C van Dooremaal, "Die Entwicklung der in fremden Grund versetzten lebenden Gewebe," *Graefe's Arch f Ophthal.*, 1873, *19*. 358-373. See also n 78 below

[62] W Goldzieher, "Ueber Implantationen in die vordere Augenkammer," *Arch f exper. Path u Pharmacol*, 1874, *2*. 387-404

[63] F W Zahn, "Sur le sort des tissus implantés dans l'organisme," *Congr. périodique internat. d. sci méd, 5e session, Génève, C. R et Mém*, Génève, 1878, p 653.

[64] T. Glück, "Ueber Muskel- und Sehnenplastik," *Arch f klin Chir*, 1881, *26*. 61-66

[65] T Kocher grafted thyroid gland in 1883, according to D. Barfurth, "Transplantation," *Handworterbuch Naturw.*, 1915, *10* 15. Priority is more frequently given to A von Eiselberg, "Ueber erfolgreiche Einheilung der Katzenschilddrüse in die Bauchdecke und Auftreten von Tetanie nach deren Exstirpation," *Wiener klin. Wochenschr*, 1892, [5]· 81-85

[66] F. W. Zahn, "Ueber das Schicksal der in den Organismus implantirte Gewebe," *Virchow's Arch. f. path Anat u. Physiol u f klin Med.*, 1884, *115* 369-387

[67] W Heape, "Preliminary note on the transplantation and growth of the mammalian ova within a uterine foster-mother," *Proc Roy Soc London*, 1890, *48*· 457-458

[68] By Küstner [no initial given] according to Barfurth, *op cit* (n 65 above), *10* 14

[69] O Minkowski, "Weitere Mittheilungen über den Diabetes mellitus nach Exstirpation des Pankreas," *Berliner klin Wochenschr*, 1892, *29* 90-94; E Gley and J. Thiroloix, "Contribution à l'étude du diabète pancréatique Des effets de le greffe extra-abdominale du pancréas," *C R. Soc Biol*, 1892, Sér 9, *4*· 686-688 E. Hédon, "Greffe sous-cutanée du pancréas. Son importance dans l'étude du diabète pancréatique," *Arch de physiol*, 1892, 5 Sér., *4* 617-628

[70] R T. Morris, "The ovarian graft," *N Y Med J.*, 1895, *62*. 436-437.

[71] O Ehrhardt, "Erfolgreiche Transplantation der Milz," Inaug.-diss Königsberg, 1897, cited from E Korschelt, ***Regeneration und Transplantation*** (Berlin. Borntraeger, 1931), *2:* 1457

[72] C H. Féré and H. Elias, "Note sur l'évolution d'organes d'embryons de poulet greffés sous la peau d'oiseaux adultes," *Arch d'anat microsc*, 1897, *1* 417-426.

[73] H. Ribbert, "Ueber Veränderungen transplantirter Gewebe," *Roux's Arch f Entw -mech d Org*, 1897, *6* 131-147.

[74] F. W. Zahn, *op cit.* (n 66 above)

Many of these early attempts that I have identified were doubtless preceded by even earlier ones, and whole lectures might be given about any class of the tissue grafts (most of which would no doubt be called failures by our standards, though transplanted testes were thought to produce spermatozoa and somatic effects). It was just within our period that distinctions were made between auto-, homo-, and heteroplastic transplants by name (1896, Giard).[75]

It might be worth a moment's time, however, to comment on grafting of bone and cartilage. As in the case of skin regeneration, the possibility that apparent regeneration of transplanted bone might be illusory was moot. There was considerable discussion as to whether the new bone was formed by the graft or supplied by the surrounding host tissue. The question was settled by Möller[76] in 1895, perhaps earlier by others I have not read. Bruns was transplanting bone marrow in 1881, to see if it would produce new bone; he grafted periosteum for the same reasons.[77] Ollier had already attempted this unsuccessfully in 1867[78] as had Maas in 1877.[79] Baikov in 1870[80] reported fourteen of 28 bone marrow grafts as successful in autotransplants.

The cartilage grafts were interesting because of their variety. Zahn not only grafted cartilage as others, including himself, grafted bone, but he also included pathological tissues among his transplants—an old lady's endochondroma was resorbed in the anterior chamber of the eye (1884).[81] Goldzieher had placed part of a carcinoma in the anterior chamber in 1874.[82] (The anterior chamber, by the way, had been introduced as an ideal site for grafting by two investigators simultaneously and independently, van Dooremaal in 1873[83] and Goldzieher in 1874).[84] Zahn also reported, without further detail, that he had grafted sarcoma and carcinoma.[85] Thus attempts at tumor transplantation predated the turn of the century. Rubin reports other nineteenth century inoculations or grafts of tumors but not as early as Goldzieher's in 1874. Rubin[86] discusses in some detail the relationships between such studies and Cohnheim's[87] theory of the embryonic origins of tumors.

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[75] A. Giard, "Y a-t-il antagonisme entre la 'greffe' et la 'régénération'?" *C. R. Soc. Biol* , 1896, Sér 10, *3:* 180-184.

[76] Möller [no initial given], *Ueber histologischen Vorgange bei Knochenimplantation*, Inaug -diss Halle, 1895. Cited by S. Saltykow, "Ueber Transplantation zusammengesetzter Theile," *Roux's Arch. f Entw - mech. d Org* , 1900, *9* p. 351, fn 6

[77] P. Bruns, "Ueber Transplantation von Knochenmark," *Arch f klin Chir* , 1881, *26* 661-668.

[78] L. J. Ollier, *Traité expérimental et clinique de la régénération des os et de la production artificielle des tissus osseux* (Paris: Masson et fils, 1867).

[79] H. Maas, "Ueber das Wachsthum und die Regeneration der Röhrenknochen mit besonderer Berücksichtigung der Callusbildung," *Arch f klin Chir* , 1877, *20* 708-770.

[80] Baikov, *op. cit.* (n. 58 above)

[81] Zahn, *op cit.* (n 66 above).

[82] Goldzieher, *op cit* (n. 62 above)

[83] von Dooremaal, *op cit* (n 61 above)

[84] Goldzieher, *op. cit.* (n. 62 above)

[85] Zahn, 1884, *op. cit.* (n. 66 above)

Biologists and physicians owe great debts to animals. The non-human mammals used as donors or hosts in the nineteenth century were the usual laboratory victims: dogs, cats, rabbits, guinea pigs, an occasional pig or goat; I suppose mice must have been used but my earliest records for them are for 1900, after our deadline. Roosters, we remember, were used by Berthold in 1849. They were still being used as hosts for testis grafts in 1895.[88] Pigeons made it as hosts at least as early as 1867.[89] Frog nerves were grafted to frog hosts as early as 1878.[90] Marshall was re-attempting grafts on Hydra in 1882.[91] Verworn performed grafts (nuclear transplants at that) on Protozoa in 1892,[92] in 1896, Giard on Ascidians.[93] In 1897 Crampton performed recombination experiments on moths,[94] and Joest on worms.[95] After 1897 the numbers and varieties of organisms studied increased greatly. In 1931 Korschelt published an enormous 2-volume treatise on transplantation summarizing work up to that time.[96] It contained 1559 pages of which approximately 200 were devoted to references to works issued almost exclusively in the early 20th century.

I have probably enumerated the range of tissues grafted, the kinds of organisms playing donor or host, more than is meaningful. But I have done so as a way to emphasize how extensive the practice of grafting had become. The motives for attempting the experiments were as diversified. Therapeusis was a principal, but not the only one. Skin grafters wanted to close open wounds, but some of them showed considerable scientific curiosity about the causes of change in pigmentation when white skin was grafted onto a formerly black area and vice versa—the same curiosity that was still alive two generations later when Medawar began to study skin graft pigmentation.

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[86] Rubin, *op. cit* (n. 12 above)

[87] J Cohnheim and H. Maas, "Zur Theorie der Geschwulstmetastasen," *Virchow's Arch. f. path Anat u Physiol u. f klin. Med*, 1877, *70:* 161-171.

[88] A. Lode, "Zur Transplantation der Hoden bei Hähnen," *Wiener klin. Wochenschr*, 1895 [8]. 345-346

[89] Ollier, *op. cit*. (n. 78 above), I. 117-118.

[90] Korybutt-Daskiewicz, *op cit* (n. 50 above)

[91] W. Marshall, "Ueber einige Lebenserscheinungen der Süsswasserpolypen und uber eine neue Form von Hydra viridis," *Ztschr f. wiss Zool*, 1882, *37* 664-702.

[92] M Verworn, "Die physiologische Bedeutung des Zellkerns,' *Arch f. ges. Physiol*, 1892, *51* 1-118.

[93] Giard, *op cit* (n 75 above).

[94] H E Crampton, "Coalescence experiments upon the Lepidoptera," *Woods Holl Biol Lectures for 1896-97*, Boston, pp. 219-230.

[95] E. Joest, "Transplantationsversuche am Lumbriciden. Morphologie und Physiologie der Transplantationen," *Roux's Arch f. Entw -mech. d. Org*, 1897, *5* 419-569

[96] Korschelt, *op. cit* (n. 71 above)

Interest in regeneration as related to transplantation was extremely lively, from both clinical points of view, as in the nerve transplant experiments already mentioned, and from the biological aspects, particularly with respect to reversal of polarity as in the Hydra grafts. Mendel's Laws were not rediscovered until 1900, so that direct questions about heredity were not yet posed, but when Heape in 1890 was grafting eggs into foster-mother uteruses he knew that his results, which showed no influence from the foster mother, answered questions that Darwin had raised concerning telegony.[97] And as always, there was the tumor question; remember that Bert had grafted embryos into adults in 1863, and Zahn had grafted malignant tumors before 1884. It is probably safe to say that once the fashion of making grafts in physiological, pathological, and surgical situations was established, the procedures were followed to try to answer all the other fashionable biological and medical questions of the day.

To show us that even the questions that to us today are so fashionable—the ethico-legal ones, I mean—were already raised then, I would like to close by reading part of a discussion that followed a presentation of "a case of heteroplastic ovarian grafting, followed by pregnancy and a living child. Query—who is the mother?"[98] Those last five words, "Query—who is the mother" were part of the printed title. The surgery was performed, I'm afraid, in 1902. In 1906 the host mother was delivered of a 7½ pound daughter. By that time, the question had not yet been solved as to whether or not residual ovarian material might remain after removal of what was visible of the two principal ovaries.

There ensued much discussion about whether the grafted ovaries had taken or the original ones remained in part, and the session closed with some golden words by Professor Sir Halliday Croom, who had presented the case; he said:

> The question was, Who was the mother of the child—the woman who provided the ovum, or the one who gave the blood-supply? They must take it for granted that all the ovarian tissue was removed . . . . His opinion was that the woman who supplied the ovary was the mother, and the child was a bastard. He held that when a man married a woman her ovaries belonged to him; and even after they were transferred to another woman they were still his. So the offspring of the second woman must be a bastard. He did not see how it could be otherwise.

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[97] Heape, *op cit* (n 67 above)

[98] J H Croom, "A case of heteroplastic ovarian grafting, followed by pregnancy and a living child Query—who is the mother?" *Transacs Edinburgh Obstet Soc* , 1905-1906, *31* 194-200

# RUDOLF VIRCHOW, JULIUS COHNHEIM AND THE PROGRAM OF PATHOLOGY*

RUSSELL C MAULITZ

In the mid-nineteenth century pathology was at a disciplinary crossroads, susceptible to being modeled and shaped by new ideas, such as the cell theory, new institutions, such as a succession of new German scientific Institutes, new techniques, such as the improved microscope, and new neighbors, such as physiological chemistry. Among the pathologists Rudolf Virchow in particular was a medical scientist capable of perceiving with clarity the political force of an idea, and of understanding the social dimension of scientific innovation. Virchow sought, through both his intellectual productions and his stewardship of the Berlin Pathological Institute, to engage actively in the recasting of his discipline, pathology, from its early roots in pathological anatomy. He was sensitive to both the theoretical and the institutional imperatives of such an enterprise. In this essay I shall describe in some detail his response to these imperatives, as well as some of the consequences of that response among his key students.[1]

Between 1848 and 1858 Virchow elaborated a theoretical program, his cellular pathology, that was progressively more static and localistic with respect to the explanatory framework in which he sought to deal with the phenomena of pathological change in living tissues. Between 1858 and 1867 Virchow's student, Julius Cohnheim, reformulated his mentor's approach in terms that were dynamic and "organismic," yet he did so with Virchow's blessing. Such a scenario must be understood in the context of a second, complementary program that Virchow had fashioned: a parallel, institutional program through which he single-mindedly built up and strengthened his Pathological Institute. Virchow always ensured that the two programmatic levels on which he was operating, the theoretical/instrumental and the institutional,

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[1] Virchow's acute sensibilities in this regard are discussed briefly in the provocative article by Everett Mendelsohn, "Revolution and reduction: the sociology of methodological and philosophical concerns in 19th century biology," *The Interaction between Science and Philosophy,* ed. Y. Elkana (Atlantic Highlands, N.J . Humanities Press, 1974), pp 407-426

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remained consistent though not necessarily coextensive. In so doing he fostered the development of a pattern of creative tensions in a research environment that would come to serve as a template for many future such efforts.

### *The History and Historiography of the Theory of Inflammation*

In the research milieu that Virchow created, a critical problem, perhaps the central problem, for investigation was that of inflammatory disease. Of the pathological processes identified in the diseased human organism, the nature of inflammation probably most generally exercised nineteenth-century pathological anatomists. The business of accurately characterizing the corpuscle of acute inflammation, the messenger of suppuration and wound healing—the pus cell—spurred medical men from Xavier Bichat to Julius Cohnheim to set their specific research agenda according to the dictates of that larger problem.[2]

Between 1861 and 1867, during his Berlin years as student and *Assistent* in Rudolf Virchow's Pathological Institute, Cohnheim developed the notion of acute inflammation as a process dependent on the action of the mobile white blood cell, the wandering leucocyte.[3] By 1867 his theory of the inflammatory cell and its function came to hinge on a set of arguments about the response of the human organism or the experimental animal as a whole to a local insult or trauma.[4] Such arguments, and hence such a theory, strayed far from the intellectual prescriptions which Virchow, Cohnheim's mentor, had enunciated from 1855 on in a progressively more rigid and dogmatic theory of inflammation.[5]

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[2] I have surveyed the problem of inflammation in the context of tissue pathology in my doctoral thesis, "A Treatise on Membranes. Concepts of Tissue Structure, Function, and Dysfunction, from Xavier Bichat to Julius Cohnheim" (unpublished Ph D. dissertation, Duke University, 1973). While I have there provided an appreciation of the evolution of ideas on, and techniques for studying, inflammation at the microscopic and macroscopic levels between 1800 and 1867, what seems now desirable is a discussion from the point of view (to quote one of its recent proponents) of "the relation between the empirical and the social components in knowledge" (David Bloor, *Knowledge and Social Imagery* [London· Routledge and Kegan Paul, 1976], p. 20) The leading American expositor of such a social relation of medical knowledge is Charles E. Rosenberg His approach has been a plain-language one in which at times Rosenberg has de-emphasized, as Steven Shapin points out in a perceptive essay review of Rosenberg's collected essays (*Times Literary Supplement,* May 20, 1977, p. 613), formal considerations of historical method. But see his forthcoming article, "Towards an ecology of knowledge: on discipline, context and history," to be published in Alexandra Oleson, ed., *Knowledge in America, 1870-1920* (Baltimore Johns Hopkins University Press, in press) For his collected essays see *No Other Gods· On Science and American Social Thought* (Baltimore. Johns Hopkins University Press, 1976). By and large historians have not ventured, however, to apply such a perceptual framework—one focussing on what Shapin calls the "penumbral zones" between intellectual production and social action—to a global history of scientific medicine. Few in particular have trained their sights on the European context. Hence the natal locus of the culture of modern "high" medicine remains ill-understood

[3] The best account in English of this part of the story remains that of Lelland J Rather, *Addison and the White Corpuscles· an Aspect of Nineteenth Century Biology* (London: Wellcome Institute of the History of Medicine, 1972), esp pp. 155-78 A rather large literature has grown up around the inflammation question in Berlin pathology: of interest are *idem,* "Virchow und die Entwicklung der Entzündungsfrage im neunzehnten Jahrhundert," in *Verhd XX Intern. Kong Gesch. d. Med Berlin 1966* (Hildesheim: Olms, 1968), pp 161-77, Paul Diepgen, *Die Lehre von der Entzundung von der Begrundung der Zellularpathologie bis zum Aufkommen der Bakteriologie* (Wiesbaden F. Steiner, 1953); W. H McMenemey, "Cellular pathology with special reference to the influence of Virchow's teachings on medical thought and practice," pp 13-43, in Noel Poynter, ed , *Medicine and Science in the 1860s* (London: Wellcome Institute of the History of Medicine, 1968), Markwart Michler, "Die Anfänge der modernen Entzündungslehre vor 100 Jahren entdeckte Friedrich Daniel von Recklinghausen die Wanderzellen," *Med Monatschr*, 1963; *17:* 743-47; Maria Wolman,

In these pages I shall return to the events which have just been described. But it should be noted at the outset that such a scenario fits uneasily into existing historical treatments of nineteenth-century German pathology. Such accounts are largely concerned with a body of received doctrine. In it, Rudolf Virchow's elaboration of "cellular pathology" is depicted as the pivotal event in the overthrow of speculative pathology.[6] According to this conventional wisdom of nineteenth-century medical historiography, Virchow seized on the cell theory of Schleiden and Schwann—itself weighed down by the metaphysical freight of idealistic dogma and *Naturphilosophie*—and refashioned it. From it he forged the appropriate instrument with which to extirpate from scientific medicine the various humoral, neural, and otherwise ontologically tainted notions of pathological change. Proclaiming *omne cellula e cellula,* according to this canonical view, Virchow established the cell as the pre-eminent structural and functional unit. And in so doing he set forth the principle that disease is but the reflection of life processes under altered circumstances. Pathological processes under this rubric were but normal ones run amok. If pathologically deranged cells were derived, according to the *omne cellula* dictum, from precursor cells at the local site of injury, then the correspondence rules were suddenly rendered simple and elegant: altered circumstances, altered cells.

The formulation just summarized, though compelling, fails to do justice to the texture of German medical experience in the 1850s and'60s. In fact, the logic of this hagiographic view scarcely even achieves internal consistency:

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*Entzundung* (Heidelberg: Hüthig, 1962) For a recently translated appraisal of the role of Viennese pathology in the period under consideration, see Erna Lesky, *The Vienna Medical School of the Nineteenth Century* (Baltimore Johns Hopkins University Press, 1976), pp 498 ff

[4] "Julius Cohnheim, Ueber Entzündung und Eiterung," *Virchows Archiv f path. Anat.*

[5] Rudolf Virchow, "Cellular-Pathologie," *Virchows Arch f. path Anat.,* 1855, *8*. 1-39, translated in the useful collection edited by L. J. Rather, *Disease, Life, and Man* (New York: Collier, 1962), pp 86-115

[6] For this positivist view, see, e g , Alfred Plaut, "Rudolf Virchow and today's physicians and scientists," *Bull Hist Med* 1953, *27* 236-51, and Paul Klemperer, "The pathology of Morgagni and Virchow," *Bull Hist Med* , 1958, *32* 24-38 Other recent scholarship presents a less presentist perspective. see, esp , L J Rather, "Rudolf Virchow's views on pathology, pathological anatomy, and cellular pathology," *Arch Path* , 1966, *82* 197-204, and the still definitive biography of Virchow: Erwin H. Ackerknecht, *Rudolf Virchow Doctor, Statesman, Anthropologist* (Madison, Wis · University of Wisconsin Press, 1953)

the cells, structural units, were at the core of Virchow's new pathology, yet physiology, requiring a functional syntax, was to be the cornerstone of his new method. Method should be empirical, or even experimental, never speculative or system-bound. Yet at this level Virchow became increasingly bound to an essentially static and localistic anatomical idea of cellular pathophysiology. By the late 1850s Virchow's field of vision in cellular pathology had, in fact, become instrumentally narrowed to that afforded by the objective lens of his microscope.

In what follows I shall present a somewhat different interpretation of the evolution of the cellular theory and of cellular pathology, from the standpoint of their institutional context in Virchow's early (1846) rejection of the humoral pathology of Carl von Rokitansky, to Cohnheim's rejection some two decades later of Virchow's (and his own, earlier) localism. It is, then, in small part, a case study in the reception of the cell theory of Matthias Schleiden and Theodor Schwann.[7] But equally significant, particularly with respect to the events initiated by Virchow on his return to Berlin in 1856, is the interplay of men, ideas, and instititutions in the medical science of mid-nineteenth-century Germany.

## *Virchow's Early Years: Physiological Pathology*

The historical issues raised by Virchow's pre-Berlin posture with respect to pathological change, though they are not at the core of my argument, warrant examination. Perhaps the most important such issue, certainly the earliest, revolves around the rejection of humoralism that springs from the pages of Virchow's 1846 review of Karl von Rokitansky's *Handbuch der pathologischen Anatomie*.[8] Again the canonical view omits some important nuances. Humoralistic doctrine was anything but an intellectual backwater of aberrant Idealism through much of the first half of the century. Pathological anatomists, particularly in the French arena where Gabriel Andral still held forth in his *Clinique médicale*, saw the holistic doctrines of humoral pathology as an instrument which had never been more potent in explaining clinical phenomena of sympathy, depletion, and repletion.[9]

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[7] Cf Rather, *Addison and the White Corpuscle, loc cit* and R.C Maulitz, "Schwann's way cells and crystals," *J. Hist Med* , 1971, *26*· 422-37, p 436, n 48

[8] Prof. Rather has translated the review in "Virchow's review of Rokitansky's *Handbuch* in the *Preussische Medizinal*-Zeitung," *Clio Med* , 1969, *4* 127-40; for the Viennese (and antilocalizationalist) perspective see Lesky, *op cit.* (n 3 above), pp 106-17, and Miciotto (n 9)

[9] Robert J Miciotto, "Carl Rokitansky. a reassessment of the hematohumoral theory of disease," in this issue of the *Bull Hist Med* (pp 183-199). see also Rather, *Addison and the White Corpuscles,* Maulitz, "A Treatise on Membranes," and Ackerknecht, *Rudolf Virchow* (n 6 above)

If humoralism was not as intellectually bankrupt as Virchow's victorious critique would have had it, then new questions about his motivation arise. One is tempted to suppose that Virchow had more in mind than the overthrow of the humoralistic notions of *blastema* and *crasis* when he decided to put additional distance between the new and the old, between open-minded science and closed system, between Berlin and Vienna. Indeed, his fulminations do make more sense as an exercise in Prussian institution-building on Virchow's part. It is important to tread lightly with any such argument. One must, I think, avoid trying to prod into submission the scant evidence on this point and content oneself rather with the recognition of certain ironies. One such irony is that already touched on: Rokitansky was conversant with then-current knowledge to an extent that history has denied him while it has better served Virchow. Another irony lies in the fact that Virchow was, without question, himself enamored in these early years of holistic, physiological sorts of explanation. This was most explicit (if slightly beside the point) in his review of Rokitansky's *Handbook;* here he pleaded for pathological physiology based on observation and experiment in the living, "for no information concerning the living is given by the dead alone."[10]

Also related to Virchow's rejection of the humoral theory is a third irony. The task of the pathologist, he asserted, was to combine pathological anatomy and pathological physiology, but at the same time to guard vigilantly against any lapse into the fashioning of sterile system.[11] Initially, as L. J. Rather has shown, Virchow continued to believe in the blastema, the ambient nutritive fluid around the cells, and departed from it only after 1852.[12] Yet within three years Virchow was to develop a cellular pathology which, to the extent that it supplanted humoral pathology with a new theory of local pathological change, in that same measure lapsed into its own doctrinal system-building. The lures of the microscope and, surrounding it, of building new institutional forms, were too tempting. Doubtless Virchow never even recognized that he was falling into the trap he had laid for Rokitansky. For the historian, ironies of this sort are rescued from preciosity only by the more interesting recognition of the manner in which Virchow ensured structurally, in his institutional program, that his students need *not* be so snared.

Finally, the Virchow of the 1840s saw himself, as I have noted, as a pathological physiologist.[13] The discipline of physiology, promoted by Johannes Müller and continued by his students despite their recriminations of 1848, was itself poised in Berlin for institutional routinization and growth.[14] Whether or not Virchow consciously perceived this in 1846, he chose to channel his early energies toward physiology. It is unsurprising, then, that much of his early work was concerned with thrombosis and embolism and found a berth in a journal of experimental pathology and physiology, Ludwig Traube's *Beiträge*. Traube, a consistent proponent of physiological medicine and the clinico-pathologic approach, would become influential in diverting Ludwig Cohnheim in later years from a static *Zellularpathologische* stance.[15]

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[10] Rather, "Virchow's review," (n 8 above), p. 131.

[11] Such was the basis of his rejection of another competing system cf Gernot Rath, "Neural pathology," *Bull Hist Med.*, 1959, *33* 526-41

[12] Rather, "Virchow's review," p 130.

But in 1846 Virchow's investigatory approach was anything but static. Writing in Traube's short-lived journal, he held forth on thrombosis of the pulmonary artery and on pyemia. He lamented the difficulty of making a static microscopic diagnosis in the latter case. Morphologic evidence was insufficient. Further, leukemic and inflammatory infiltrates were indistinguishable in the lung: the problem hinged on "the absolute identity of the colorless blood corpuscles and the pus corpuscles." Hence the final irony: in a passage hitherto unremarked in the historical literature, Virchow advanced the idea which at least tentatively adumbrated Cohnheim's 1867 break with cellular pathology. Virchow never pursued it. The remark was to be devoid of heuristic value for him.[16]

## *Virchow, Cellular Pathology, and the Institute: the 1850s*

By the early 1850s the problem of the identity of the leukocyte and the pus cell faded in interest for Virchow. What did interest him greatly was the elaboration of a program for the shoring up of scientific medicine. Between 1852, when the "cellular pathology" began to take shape in his mind and hence supplant physiological pathology, and 1855, when the article bearing that title emerged on the pages of his own *Archiv*, Virchow began newly to design both an intellectual and an institutional program. The process continued at both levels until it reached an early theoretical watershed with the 1855 statement of cellular pathology, and an institutional turning point with his triumphal return to Berlin the following year. As Ackerknecht noted a quarter century ago, Virchow demanded the establishment of a physically separate Pathological Institute: this was but one precondition he set before he would return to the scene of his colleagues' and his own political discomfiture of the troubled 1848-50 period.[17]

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[13] Ackerknecht, *Rudolf Virchow*, pp 49-53

[14] On the Physiological Institute cf Max Lenz, *Geschichte der Friedrich-Wilhelms koniglichen Universitat Berlin* (Halle: Buchhandlung des Waisenhauses, 1910), pp. 154-64; standard treatments on a more general level of the institutional setting include Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, N.J : Prentice-Hall, 1971); *idem* and A Zloczower, "Universities and academic systems in modern societies," *Eur. J. Sociol.*, 1962, 3· 45-84; Theodor Billroth, *The Medical Sciences in the German University* (New York: Macmillan, 1924), Friedrich Paulsen, *German Universities: their Character and Historical Development* (New York Macmillan, 1895), Rudolf Virchow, "The founding of the Berlin University and the transition from the philosophic to the scientific age," *Smiths Rep.*, 1894 (Washington, 1896), pp. 681-95 On the 1848 group of physiologists see, *i.a* , Paul Cranefield, "The organic physics of 1847 and the biophysics of today," *J Hist. Med & All. Sci* , 1957, *12:* 407-23.

[15] There is no adequate assessment of the role of Traube and "physiological medicine" in nineteenth-century scientific medicine, but see Knud Faber, *Nosography in Modern Internal Medicine* (New York: Hoeber, 1923), pp 59-94

[16] Maulitz, "Treatise on membranes" (n. 2 above), p. 114.

That the troubles at mid-century had made an indelible imprint on Virchow, and that his institutional energies were focused as a result, may be discerned in an 1853 essay on "Authorities and Schools." "What around us yet stands firm. . . . What order securely grounded?" he asked, since "the greatest of shocks have passed over humanity [and] ideas were trampled underfoot with brutal disdain." The Revolution of 1848, he went on, had left a legacy of confusion, interrupting but not suppressing the reaction against the Hegelians and the medical system-builders. Virchow's *Archiv*, begun in 1847 with Benno Reinhardt, had embraced (he claimed) the goals of those who challenged the established order.[18] The present counter-reaction plunged the minds of men into a "period of the deepest intellectual depression, during which life retreat[s] toward material and mechanical interest." He feared that this reaction would extend from social groupings concerned with art, business, politics and the like, into natural science and sever it from its social moorings. But across this pessimistic tapestry he traced a thread of hope. Invoking what was to become the ideology of late nineteenth-century scientific medicine, and for him a formulaic credo, he intoned the redemptive virtues of medical science: "And yet it moves," said Rudolf Virchow in 1853.[19]

Within two years Virchow had a new theoretical formula, the cellular pathology which was to leave little room for the sort of physiological style of research and explanation which he had earlier advocated. And within three years or so he had his new Institute. Did he shift his intellectual style in order to build up and legitimize his new edifice? That the innovations were so closely aligned in time makes the affirmative argument plausible, but conclusive documentation is lacking. A counter-argument, that he lost interest in physiological pathology, relying instead on his lieutenants to nurture and perpetuate it, while defensible, begs the question. It fails to explain the single-mindedness with which Virchow now began to champion the theoretical agenda inherent in his 1855 article and in the better known 1858 lectures which it prefigured.[20]

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[17] Ackerknecht, *Rudolf Virchow*, p 13; cf also *idem Beitrage zur Geschichte der Medizinalreform von 1848* (Leipzig, 1932)

[18] Rudolf Virchow, "Autoritäten und Schulen," *Virchow's Archiv*, 1853, *5:* 3-12

[19] *Ibid.*, p 8

The 1855 article was a fresh break. Now, for Virchow, the cells were the loci of all vital phenomena, normal or abnormal. They were the prime movers, living and irritable, interdependent and equal, dependent on nourishment derived locally from neighboring parts, and formed by local generation from prior cells: *omne cellula e cellula*.[21]

The impact of such a theoretical posture on the study of basic mechanisms of disease was clear-cut. So, too, were the institutional imperatives dictated by the cellular pathology. Disease processes, most notably acute inflammation, were to be studied by medical microscopists with pathological training, not by students of the *clinique* given to "crasiology" in the humoralist tradition, or by proponents of nerve-energetic explanations in the "neural" tradition. Inflammation "cannot be traced back to increased innervation."[22] Rather, it ought to be studied experimentally, in the cornea of the eye, for example, where "understanding . . . must be allowed to develop through continuing histological studies."[23]

Statements of this sort implied a surge of institutional change which cannot have been lost on Virchow or his colleagues. The implication was sharpened in the *magnum opus*, the *Cellular Pathology*.[24] The work is well known to the general reader; lectures 17 through 19 repay a repeated glance with a reminder of the subtlety and refinement Virchow brought to his localism by 1858. What remains clear is his commitment to the microscopist's perspective. The pus cell, he asserts, derives locally from an epithelial cellular element (a nod to Jacob Henle) or, far more commonly, from a connective tissue cellular element.[25]

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[20] On this shift see L J Rather, "Rudolf Virchow and scientific medicine," *Arch Int Med* , 1957, *100*. 1007-14.

[21] Virchow (tr Rather), "Cellular pathology," in *Disease, Life and Man* (n. 5 above), pp 96-102

[22] *Ibid.*, pp. 109-12.

[23] *Ibid.*, p 129, on the importance of the cornea as an experimental model, see Maulitz, "Treatise on Membranes," pp 125-27 and *passim*

[24] Rudolf Virchow, *Die Cellularpathologie in ihrer Begrundung auf physiologische und pathologische Gewebelehre* (Berlin Hirschwald, 1858). Frank Chance's English translation was of the second edition of 1859, published in London in 1860 and in Philadelphia, by Lippincott, in 1863 I have used here the Dover reprint (New York, 1971) of the latter, to which a useful new introduction has been added by Prof Rather Hence I shall be citing the 1863 American edition, in which only the pagination differs from the 1859/60 (2nd) edition, I have collated the three pre-1867 editions, however, to ensure textual consistency between them Regarding the 4th (1871) edition and Cohnheim, see pp 181-82 below

And what of the claims of William Addison and Gustav Zimmerman that a strange resemblance obtained between pus cells and white blood cells? Forgetting his own 1846 findings, Virchow now complained that Addison and Zimmerman had "imagined they had found colorless blood corpuscles when they were really looking upon pus. . . . If one would seek a distinction . . . there is no other criterion than to determine whether the cell arose at a spot where a colorless blood-corpuscle might be expected to arise, or at one where it ought not be produced."[26]

The initial legitimacy of Virchow's new Institute hinged upon the persuasiveness of this theoretical program. It was critical for Virchow to demonstrate the uniquely useful values inherent in a new pathology that relied intellectually on the microscope. And clinicians like Theodor Billroth were quick to see the utility of the new posture.[27] Virchow, in turn, cultivated their response. Not surprisingly, surgery led other branches of medicine in assimilating this particular strain of the new "scientific" medicine. Surgical pathology was of growing importance as cellular pathology made inroads into the diagnosis of abscess and tumor. Poised between university and hospital, between *Wissenschaft* and the clinical bustle of the Berlin Charité, Virchow through his institute stood ready to investigate the productions of each in the terms of the other. His theoretical program not only bridged the gap between science and medicine, but specified bricks and mortar from which the bridge was to be built.

### *Virchow, Cohnheim, and the Institutional Program*

With bricks and mortar in place, the edifice constructed in intellectual as well as physical space, Virchow set about the rather different job of stabilizing and extending it. His choice of *Assistenten* in the first few years of the Institute is particularly telling in this regard. The first, Félix Hoppe-Seyler (1825-95), became *Assistent* in 1856 upon his arrival from Greifswald, where he had taught physiological chemistry and pathology as a *Privatdozent*.[28] Hoppe-Seyler set up a laboratory of chemistry, at Virchow's behest, under the roof of the Pathological Institute. The existence and prosperity of that laboratory provide testimony to the institutional direction—eclectic and expansionist—in which Virchow was looking. The proximity of such a laboratory, in turn, ultimately would have a reflexive effect, through the labors of Willy Kühne and Julius Cohnheim, on Virchow's narrower intellectual posture.

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[25] *Ibid*, pp 490-96

[26] *Ibid*, p. 527

[27] See, e g, Theodor Billroth, *Beiträge zur pathologische Histologie nach Beobachtungen aus der koniglichen Universitats-Klinik zu Berlin* (Berlin: Reimer, 1858)

[28] On the role of the *Privatdozent* in German academic culture, see Alexander Busch, "The vicissitudes of the *Privatdozent*: breakdown and adaptation in the recruitment of the German university teacher," *Minerva*, 1962-63 *1* 319-41

Another early *Assistent* to enter the Institute was the Westphalian Friedrich von Recklinghausen (1833-1910). He had been educated in Bonn in Virchow's Würzburg, and in Berlin, where he took the M.D. in 1855 with a dissertation on theories of pyemia. Now, in 1858, von Recklinghausen became another *Assistent* under Virchow, two years after Hoppe-Seyler. Unlike Hoppe-Seyler, von Recklinghausen was a committed practitioner of Virchow's localist tissue pathology. In his first year he published a first communication in the *Archiv* which was to prove critically important on two counts. First, it was his letter of credentials in the localistic pathology to which he would adhere through the 1860s. And second, it was an earnest of his own special genius for devising laboratory methods and techniques. Applying the salts of various metals (including silver) to membranes such as the cornea, he investigated those tissues' susceptibility to inflammation and other pathological affections, explaining them in terms of local pathological change. For six years von Recklinghausen would continue in this vein, "tagging" inflammatory cells with materials such as heavy metal salts. In 1864 he would be named in rapid succession to full professorships at Königsberg and Wurzburg. His Berlin successor in that year was to be Julius Cohnheim.[29]

This early trio of physicians—Virchow, Hoppe-Seyler and von Recklinghausen—embodied in human terms the expertise which Virchow envisioned filling out his institutional program. Having established his Institute on the foundations of his cellular pathology, he saw the utility of promoting it through the infusion of other styles and approaches. By this means he would create an infrastructure within the confines of the Institute which, through internal expansion and division, would add outward strength to the enterprise. Between 1856 and the turn of the century, when he handed the reins over to his successor Johannes Orth, Virchow was most successful in this entrepreneurial endeavor. By 1905, when Orth moved the Institute into its second new building, there were at least six internal divisions: anatomy, histology, bacteriology, experimental biology, chemistry, and the Museum.[30] Virchow elaborated the precursors of at least three of these divisions in the nascent Institute of the late 1850s and early 1860s.[31] Only hints of the resistance he must have met are found in the reminiscences of his early students and collaborators. One telling and useful such account is that of Edwin Klebs (1834-1913), who entered the Institute as *Assistent* in the same year (1861) as did Cohnheim, and who would later serve as interlocutor between pathology and bacteriology.[32] In a necrology written on the occasion of Cohnheim's untimely death in 1884, Klebs remarked on the influence within pathology as a discipline defined by its apotheosis, the Institute, exerted by the neighboring fields of histology and physiology. He recalled the controversy over the establishment of a course in normal histology; contested when it was first instituted, the course had ultimately proved to be an extraordinarily fruitful coup of Virchow's.[33] And Klebs, writing in a journal representing German expertise at the interstices of experimental pathology and pharmacology, mentioned only in passing the *chemische Arbeit* being fostered by Virchow.[34]

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[29] Willy Kuhne, "Zur Erinnerung an Julius Cohnheim," pp. vi-xlvi in E. Wagner, ed , *Gesammelte Abhandlungen von Julius Cohnheim* (Berlin Hirschwald, 1885); and William Councilman, "Friedrich Daniel von Recklinghausen (1833-1910)," *Proc. Amer Acad Arts & Sci* , 1917-18, *53:* 872-75.

## *Cohnheim and the Problem of Inflammation, 1861-67*

Between 1858 and 1862 debate raged among German pathologists over the nature of inflammation. But the debate was couched entirely in terms of Virchow's cellular pathology model. within the localistic model, the question about inflammation devolved into one of characterizing the local cell of origin whence pus derived.[35] Virchow himself favored the connective tissue as the generative tissue, further underscoring the importance he had long attached to this mother lode of cell development.[36] Others favored the epithelial cell layer. Arriving in late 1860 from Albrecht von Kolliher's Wurzburg histology laboratory, Cohnheim entered the fray in 1861 and proved himself a faithful disciple of the Berlin master. He did so in the time-honored manner: he presented an M.D. thesis, in scholarly Latin, on pyogenesis in serous membranes.

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[30] Hans-Heinz Eulner, *Die Entwicklung der medizinischen Spezialfacher an den Universitaten des deutschen Sprachgebietes* (Stuttgart Ferdinand Enke, 1970), p. 108. This encyclopedic work of superb scholarship, along with Erna Lesky's account of the Vienna School (n 3 above), represents the best in a continuing tradition of meticulous Austro-German medical history It deserves the translation effort which Lesky's book recently received

[31] Cellular pathology, chemistry, and normal histology were each represented by separate courses or laboratories See Edwin Klebs, *infra*

[32] Edwin Klebs, "Julius Cohnheim, gestorben am 15 August 1884," *Arch f exp Path u. Pharmakol* , 1884, *18*, 3-4 Hft.: i-x

[33] He neither gave an exact date nor named Virchow's opponents. It is difficult to imagine, however, needing to look much beyond the anatomists *per se*

[34] Klebs, "Cohnheim," (n 32 above), pp iii-iv

[35] See , e g , Ernst Neumann, "Ueber die Bildung des Eiters," *Konigsb med Jbuch*, 1859, *2* 227-47, Rudolf Virchow, "Die Bindegewebsfrage," *Virchows Archiv*, 1859, *16*. 1-20; Eduard Rindfleish, "Beitrage zur Lehre von den Entzündungen seröser Membranen," *Virchows Archiv*, 1862, *23* 519-26; Ernst Neumann, "Die Eiterbildung auf Schleim und seroser Hauten," *Virchows Archiv*, 1862, *24* 202-205

> From the information available at the beginning of this generation, Bichat established the nature and peculiarities of the serous membranous organs with which this paper will deal, as well as their composition, so far as the knowledge at that time allowed. Anatomists have disputed much about these things and have carried on debates. They have all chosen the name given to these organs by Bichat, yet they have disagreed a great deal among themselves as to what parts of the body in particular are given this task.[37]

Invoking the shades of an earlier, pre-microscopic tradition, Cohnheim thus launched his exposition of the Virchowian model as it might be applied to the experimental study of inflammation.[38] After tracing the history of that earlier tradition, from Xavier Bichat to Gabriel Valentin, he pressed the connective tissue model of local inflammatory change to its limits. He summarized: "The serous membranes correspond to connective tissue."[39] The pus in experimental serositis, induced by physical, chemical or thermal injury of the membrane, was a product of that tissue, regardless of the particular membrane or laboratory animal chosen:

> When I set animal experiments by means of producing suppuration in the serous coat, I understood well the merit of formulating results from them. Anyone who raises animals for experiments is amply convinced of how conflicting, indeed how amazing, their reaction is with respect to traumatic injuries; how small lesions often are borne with more difficulty than more serious ones, how in varying manner one species of animal reacts to one kind of injury and another species of animal to another injury; how great a diversity of resistance to some process is found within the *same* species of animal. Here, however, . . . the matter is such that I am able to choose any species of vertebrate animal, certainly any *mammal*, for experimentation. *For the*

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[36] On the connective or "cellular" tissue, see the remarks by J Walter Wilson on the *tissu cellulaire* "fallacy". "Cellular tissue and the dawn of the cell theory," *Isis*, 1944, *35*. 168-73, and "Virchow's contribution to the cell theory," *J Hist Med. & All. Sci* , 1947, *2*. 163-78 But see also Maulitz, "Treatise on Membranes," ch. 3

[37] Julius Cohnheim, *De pyogenesi in tunicis serosis*, M D dissertation (Berlin G Lange, 1861), p 5. Only Paul Diepgen appears to have noted the significance and pivotal position of this thesis in Berlin pathology "Die Lehre von der Entzündung" (n. 3 above), p. 75

[38] On the fate of the "membrane" tradition, see Maulitz, *op cit*. (n 2 above), ch 1-3

[39] Cohnheim, *De pyogenesi*, *loc cit*

*pleura, the pericardium, the peritoneum of all mammals and of men clearly employ the same structure, whereby one could transfer histological observation from any mammal.*[40]

Later in the year Cohnheim published a revision of his thesis appropriately enough in Virchow's *Archiv*. There were few changes. The most significant change lay in his having pared away his survey of the instrumental and theoretical perspectives which antedated and undergirded his own perspectives dating back to Bichat. He covered his tracks. But he also moved to shore up his essential histologic argument: both animal and human observations bore out the notion that connective tissue proliferation initiated pyogenesis. Round cells, first singly and later multiply nucleated, emerged. The epithelium either disintegrated or was lifted away, playing no active role. The pus then poured forth into the body cavity affected, there to disintegrate itself.

What is important is this early foray into experimental pathology, aside from its clear-cut adherence to Virchow's localism, is the fact that it serves as the theoretical baseline from which Cohnheim would later depart. But why, some six years later, did he depart from the Virchowian approach? What happened between 1861 and 1867? Any answer to this question is necessarily incomplete.[41] But I would submit that any explanation of this process of innovation must take into account the institutional program of the Pathological Institute of Berlin, as it was developed by Rudolf Virchow.

The years 1862-64 were filled with pressing and dolorous personal matters for Julius Cohnheim. In early 1862 he fetched his father, a picaresque figure given to nomadic adventures, back to Berlin from the German coast and nursed him until his death of cancer in July. It was a crushing blow and only the first of a series of jarring personal dislocations. Eighteen months later Cohnheim found himself pressed into service in Bismarck's army in the Danish-German conflict over Schleswig-Holstein. During those operations, almost two years to the day from his father's death, his younger brother Albert was shot in the head in battle at Alsen. One of the first to fall, Albert Cohnheim left Julius the sole support of their mother and sisters.

Life changed abruptly. Cohnheim curtailed his military career. He converted to Christianity. His mother applied successfully to the king for intercession in the matter of his remaining military obligation. He returned to

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[40] *Ibid.*, pp 23-24

[41] Von Recklinghausen, for example, had nearly the same range of disciplinary exposures as did Cohnheim, with vastly differing outcomes See, e g , von Recklinghausen, "Arbeiten aus dem chemischen Laboratorium des pathologischen Instituts in Berlin," *Virchows Archiv*, 1858, *14* 466-82 These chemical researches, von Recklinghausen noted, had been "performed under the special direction of Herr Hoppe "

Berlin, to the Virchow assistantship. He stepped into the shoes of Friedrich von Recklinghausen, who now went to Königsberg.

Intellectually as well as personally Cohnheim was changing. In 1863 and 1864, as prosector and surgeon to the wounded, he encountered clinical material in veritable waves. In the role of military physician he was fresh from the exposure he had had to the Berlin clinical staff, particularly to Ludwig Traube (1818-76). Traube especially had impressed on Cohnheim the desirability of bringing the physiological approach to the bedside, just as one might strive to bring an awareness of clinical problems to the laboratory and dissection table. In addition, Cohnheim had moved over laterally, both physically and professionally, for a period just prior to his conscription, to work in the Institute's chemical laboratory, now under Willy Kühne. In so doing he cemented not only a lasting personal and professional relationship, but also part of the future of pathology as an experimental discipline.[42]

*Von Recklinghausen, Cohnheim, and* Wanderzellen: *1863-1867.*

As I have noted, von Recklinghausen was already by 1858 employing heavy metals in his histological investigations. In 1862 he published his first work on "The lymph vessels and their significance in connective tissue."[43] Here he used silver salts to demonstrate the anatomical continuity of the lymphatic vessels with lymph pockets or spaces in the animal organism. His use of the silver salts and his conversancy with the lymph spaces of his laboratory animals were to stand him in good stead. In the following year he published two significant papers, the second of which fully exploited these technical features. This second paper, "On pus and connective tissue cells," brought von Recklinghausen to the brink of the discovery and proof of the white cells' role in inflammation. He was too consummate a histologist in the localist morphological tradition, and too loyal a follower of Virchow, to superimpose on his experiments an outlandish holistic approach. But some four years later Cohnheim would quietly insinuate just such an approach into the laboratory arrangement devised by von Recklinghausen.

In the longer paper of late 1863 von Recklinghausen directly confronted the problem of the pus cells. The manuscript reveals how very near he was in 1863 to the conclusions which Cohnheim later reached. Von Recklinghausen began with the injured cornea. He applied a silver nitrate stick to the center. The caustic produced a cloudy opacification typical of acute keratitis. He then sampled the cloudy area by puncturing it with a glass tube, and found the cloudiness due to numerous formed corneal cells. He admitted that these cells corresponded in size and shape to pus cells, yet he categorically denied their identity—they "differ strikingly."[44]

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[42] Kühne, "Zur Erinnerung" (n 29 above)

[43] Friedrich von Recklinghausen, *Die Lymphgefässe und ihre Beziehung aum bindegewebe* (Berlin. Hirschwald, 1862).

Von Recklinghausen now took slivers of excised frog cornea, used as an example of normal connective tissue, as his next object of study. The corneal cells, which he regarded to be mainly fusiform in shape, changed shape in the moist chamber in the same manner as the pus cells visible in the aqueous humor. Almost as an aside he then remarked that these cells also *wander—like the pus cells*—in the course of their changing shape.[45]

To elucidate the problem further, von Recklinghausen now turned to the more complicated technique which was to push him to the very limit of insight which would be afforded by the prevailing localism. He placed corneal slivers from the frog in one of its lymph sacs, usually that of the thigh, and watched the progression in time of the inflammatory process. Over a four to five day period the ratio of motile to fixed cells increased in the corneal slices. A band or zone of opacity appeared, similar to that found in the intact cornea when inflamed. This area turned out to consist of a profusion of motile cells. What of their origin? Either a remarkable proliferation of the corneal cells had occurred, or corpuscles had invaded from the lymph sac. Again von Recklinghausen the comparative morphologist came to the fore, substituting dog and squirrel corneas for those of the amphibian host. He knew he could distinguish the pus cells of mammals from those of the frog. The technique should therefore distinguish between the two possible modes of origin. This it indeed did—in favor of the latter possibility: a local process, still, but exhibiting the local incursion rather than metamorphosis of cells.[46]

Von Recklinghausen adduced further support for his proposed mode of pus cell origin by adding an additional, exceedingly important technical feature: "tagging" his target cells with particles of dye. Noting Haeckel's observations on the blood cells of lower animals, and his own observations on frog blood and fat globules, he described the use of pulverized cinnabar in the lymph sac. Now the opacity which moved centripetally into the corneal slice was pale red. The pus cells had taken up the dye. The inescapable conclusion was that the cells migrate inwards. But for von Recklinghausen, as Rather has pointed out,[47] the question was one of comparative morphology, permeated as his thinking was by the more static and localistic approach which had become acceptable in the Berlin circle. Thus he next turned to a disquisition on contractility, the property shared by white blood cells, pus cells, lymph cells, and certain cells in normal connective tissue, in lower as well as higher forms. He dismissed out of hand the notion that contractility might impart *long-range* motility; the term "traveling cells" *spazierende Zellen* favored by "a young friend of this image" was more "pure fantasy" than anything else.[48] And with this von Recklinghausen retreated to the position espoused by his predecessors, emphasizing the role of the connective tissue. He allowed that he could not offer proof for his position on the matter, except insofar as there appeared to be transitional forms in the cornea between pus and connective tissue. But no such forms, he admitted, were to be found in *normal* connective tissue. He seemed to be on the verge of discarding the conventional wisdom, only to return to the fold.

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[44] *Idem*, "Uber Eiter- und Bindegewebskörperchen," *Virchows Archiv*, 1863, *28:* 157-97; p 157.

[45] *Ibid.*, pp. 169-70

[46] *Ibid.*, pp 182-83

[47] See Rather, *Addison and the White Corpuscles*, p 159.

As early as 1861 Cohnheim had availed himself of the experimental model provided by the serous membranes of the laboratory animal. Two further pre-conditions were necessary for him to be moved toward working out the problem of inflammation. One such conditon involved learning von Recklinghausen's elaborate histological staining techniques. The other condition was the adoption of a holistic, or physiological style of research to which he was exposed in Kühne's chemical laboratory.

At von Recklinghausen's side he learned the techniques of silver staining. On his return from Schleswig-Holstein to take von Recklinghausen's place, he began to develop even further the mosaic of techniques which were to serve him in his inflammation studies. Doing this was in part, quite simply, Cohnheim's job. Like his predecessors in the role of assistant in the Institute, he was now called upon to perform many chores in a bewildering variety of practical and research areas. Amidst a flurry of activity in 1866-67, he began to publish his observations on the materials which crossed his bench, both in normal and pathological histology.[49]

It will be well to mention at this point that Cohnheim was also aware by late 1866 of direct observations on capillary walls being performed in 1865-66 by Salomon Stricker, the Viennese pathologist. Stricker had studied with both Rokitansky and Brücke, and, according to Prof. Erna Lesky, tried to hybridize Virchow's cell theory with certain of the humorally tinged observations of Rokitansky. According to Lesky, in 1865 and again in the summer of 1866, Stricker demonstrated the diapedesis of red cells in another frog membrane, the nictitating membrane. On this latter occasion a group of young German scientists was in attendance, perhaps including Cohnheim.[50] Cohnheim himself only noted, in his 1867 paper, that he had observed phenomena *like* those reported by Stricker—which indeed by then he had.[51] Whether present physically or not, however, Cohnheim was quite clearly cognizant of Stricker's observation of diapedesis, since (as Lesky notes) he abstracted the latter's report at some length for the 12 May 1866 edition of a weekly professional journal, the *Centralblatt für die medizinische Wissenschaften.*[52] According to Cohnheim most of Stricker's interest lay in the phenomena of the micro-vasculature, e.g. capillary dilation in response to injury. But at the end Stricker recorded the lodging of a red cell in the vessel wall. The cell appeared to "bore through" the wall in such a way that at one point its two halves seemed connected by a narrow "neck."[53] These remarks were casually made and dropped, seemingly with little fanfare on Cohnheim's part. But the image of the directly observed capillary and the diapedesing cell must have remained with him.

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[48] Von Recklinghausen, *op cit* (n. 43 above), p 189

[49] See, e.g., Julius Cohnheim, "Ueber den feineren Bau der quergestreiften Muskelfaser," *Virchows Archiv*, 1865, *34:* 606-22

It is not sufficient, however, to "explain" Cohnheim's arrival at his new notion of inflammation solely in terms of the techniques, experimental apparatus, and tissue preparations available to him. These technical features, after all, had been available by and large to von Recklinghausen as well. The idea stated by most historians of pathology, that Cohnheim was the most experimentally oriented pathologist among Virchow's immediate successors, is of some help.[54] But even this is not quite the missing link in the explanation, since others indulged in experiment with less innovative theoretical results. To understand this final theoretical pre-condition, we must turn briefly to the 1867 paper itself. A glance at this paper and at its two conceptual bases will illustrate the extent to which Cohnheim's contribution reflected neither a radically new conceptual formulation, nor an experimental innovation *per se*, but rather an effort to embrace the entire research program available in the Berlin Institute.

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[50] Lesky, *op cit.* (n. 3 above), p 501

[51] Cohnheim, "Entzündung und Eiterung" (n. 4 above), p 42 This critically important essay was reprinted in Wagner's edition of Cohnheim's collected works (n. 29 above), in Sudhoff's *Klassiker der Medizin* (Bd. *23*), and in an abridged but useful translation by Dr Saul Jarcho "Cohnheim on inflammation," *Amer J Cardiol*, Feb 1972, *29*: 247-49, and Apr 1972, *29* 246-47

[52] *Idem,* no title, *Cblatt. f.d med Wiss.*, 1866, *4*: 339-40.

[53] S Stricker, "Studien über den Bau und das Leben der capillaren Blutgefasse," *Wien acad. Sitzungsber math -naturwiss. Classe 2,* Abth. iii. 379-94

[54] See, e g , Paul Diepgen, *Unvollendete vom Leben und Wirken fruhverstorbener Forscher und Aerzte aus anderthalb Jahrhunderten* (Stuttgart: Thieme, 1960), p. 51

The first conceptual base was Cohnheim's painstaking observation of the diapedesis, or outward migration through capillary walls, of leukocytes in experimentally inflamed serous membranes. This work could still be construed to have been in a sense essentially localistic. In any event, it hardly sprang *de novo* from Cohnheim's fevered brow; he was aware of the observations made in Vienna in 1865-66 by Stricker, not to mention the earlier work of William Addison.

The second base of Cohnheim's argument rested in turn on a constellation of animal models and technics for straining cells and membranes in the abdominal cavities and in the eyes of the experimental animals. This line of investigation rested on inferential reasoning about dye-tagged pus cells migrating from afar into the avascular cornea of the eye. The *experimentum crucis* was his injection of dye into a site—such as the lymph sac of the thigh previously exploited by von Recklinghausen—which was distant from the point of pyogenic injury in the experimental animal, and *then* injuring the intact cornea. (Von Recklinghausen had always transplanted the injured cornea *into* the lymph sac: the question of biological "action-at-a-distance" was simply not a real problem for him.) When Cohnheim left the cornea intact and then induced inflammation in it through chemical or thermal means, and in the same animal injected dye into the lymph sac, he noted an odd and patently anomalous phenomenon—anomalous in terms of Virchow's notion of local pathological change stemming from local injury and local cellular elements. In the event, dye-tagged pus cells appeared in the cornea and simultaneously in the bloodstream, like so many motile infusoria.

Whatever prompted Cohnheim to make the assumption that the cells might arise from afar and migrate to the site of inflammation must provide the critical link, the theoretical precondition for his experimental innovation. Perhaps it was an excessive preoccupation with the amoeboid analogy, the notion that motile cells move over distances like so many infusoria. On the fact of it, however, I doubt that this was the paramount factor, since Cohnheim himself gave it minimal play.[55] Rather, Cohnheim stressed that "ohne Gefässe, keine Entzündung": without vessels, there is no inflammation.[56] Most likely the factor which prompted Cohnheim to ask the new question of action at a distance was a particular approach to the physiology of the whole organism. It was an approach learned at the side of the physiological chemists with whom Cohnheim had been in intimate contact in the Berlin Pathological Institute.

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[55] Von Recklinghausen emphasized this aspect to a much greater extent

[56] Cohnheim, "Entzündung und Eiterung," p. 75

Most notable among the physiological chemists was Willy Kühne, who was *Assistent* in chemistry within the Institute from 1861 to 1868.[57] Kühne was to be Cohnheim's principal biographer, but the physiological chemist was apparently too modest to emphasize the impact that he had upon Cohnheim. One must turn instead to a necrology by Carl Weigert, the Leipzig pathologist who was Cohnheim's assistant and intimate for ten years, for Kühne's relationship to become clear. Weigert also recorded Cohnheim's close friendships with Rudolf Heidenhain, Hugo Kronecker, and other physiological chemists.[58] Of the associations Cohnheim formed with medical scientists in "nearest-neighbor" disciplines, the strongest seem to have been these linkages with the physiological chemists—men who perforce formulated many problems in regard to the integrative aspects of the organism.

Through his institutional program Virchow had actively encouraged collaboration between pathological anatomists and physiological chemists, which reinforced his intellectual vision of the relevance of chemistry to the understanding of vital processes. He supplied structural support for this vision by providing niches for practitioners of both disciplines within his Institute. Virchow also saw a golden opportunity in the potential results of such interactions. Capitalizing on such efforts, he could both expand the boundaries of his own Institute and exploit an area that was neglected in more physically oriented physiological research programs, especially that fostered by Du Bois-Reymond in the neighboring Berlin Physiological Institute. As noted above, Virchow had demonstrated this institutional commitment as early as 1856 by attracting Felix Hoppe-Seyler to conduct investigations in physiological chemistry. After Hoppe-Seyler left for Tübingen in 1861, Willy Kühne was named as his successor. Kühne, significantly, had studied previously with Claude Bernard in Paris. It is not surprising, then, that he led Cohnheim to a research problem dealing with the "sugar-forming ferments."[59]

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[57] I know of no adequate historical appreciation of Kühne But see K E Rothschuh, "Wilhelm Friedrich Kühne," *Dictionary of Scientific Biography* (N.Y.: Scribners, 1970 *et seq.*), VII, pp 519-21. In his earlier general *History of Physiology* (Berlin: Springer, 1953; Eng. trans Guenter Risse [Huntington, N.Y : Robert E Krieger, 1973]), Rothschuh does not mention the critical matter of context. that during his Berlin years, Kühne's mandate to carry out research in physiological chemistry was primarily in the setting of the Pathological Institute (to which he moved early on from Du Bois-Reymond's Physiological Institute). On Kühne's later relations with Hoppe-Seyler, and on the static versus dynamic modelling of another of the nascent "basic sciences," see R.E Kohler, "The enzyme theory and the origin of biochemistry," *Isis*, 1973, *64* 181-96

[58] Carl Weigert, "Nekrolog von Julius Cohnheim," *Berl. klin Wschr* , 1884, *21* 564-65

Modern historians have not adequately stressed the importance of this connection; in 1937 E. B. Krumbhaar mentioned in passing the French influence on Cohnheim through Kuhne.[60] What was critical in this influence was the emphasis on an approach to the whole organism which was vivisectional and global. Such a holistic viewpoint was instrumental in that it played a crucial permissive role in Cohnheim's manner of formulating pathological problems. Despite the pervasive localism of the pathologists around him, Cohnheim produced a hybrid model of inflammation because his was a hybrid method of scientific medicine, drawing from both the localizationalism of the histologist and pathologist, and the holism of the physiologist. It was this holism, rather than experimentalism *per se* which was critical. One could experiment in other ways and with other sorts of results, as von Recklinghausen had shown and as Cohnheim himself was to demonstrate in the 1870s in his approach to experimentally induced tuberculosis. But the particular organismal approach to physiology was a characteristic feature of Bernard's laboratory when many German physiologists were occupying themselves with the function of isolated organ-preparations.[61]

It may be mentioned in this regard that the injection into physiology of the notion of action at a distance, which was critical for Cohnheim, had also been the fruit of the French physiological tradition. That this may be so appears to be a logical enough extension of the French approach to the organism as a unified whole. But more precision in formulating this idea seems to be called for. Joseph Schiller has traced this strand of the physiological tradition back to Henri Dutrochet, showing it to have ramified subsequently into other leading areas of interest.[62]

## *Conclusion*

In 1871 Virchow published the fourth and final edition of his *Cellular Pathology*. A quarter century since his theoretical program had begun to drift away from the physiological mode, Virchow now accomodated his view on pathological change to Cohnheim's findings. This accomodation to a view of the living organism, dictated by the physiological style of research, represented a turnabout, a coming full circle for Virchow: it was incommensurable with his program of cellular pathology, which he now adjusted accordingly.

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[59] Eulner, *Die Entwicklung* (n 30 above), p 80, on Bernard and the chemical tradition see F L Holmes, *Claude Bernard and Animal Chemistry* (Cambridge, Mass · Harvard University Press, 1974).

[60] E B Krumbhaar, *Pathology* (Clio Medica, vol. 19) (N Y Hoeber, 1937), p 114

[61] Though it has been criticized on certain points, the classic treatment of national positions and "styles" in physiology remains that of Owsei Temkin, "Materialism in French and German physiology of the early nineteenth century," *Bull Hist Med* , 1946, *20* 322-37

[62] Joseph Schiller, "Les frustrés de l'histoire Henri Dutrochet," *Episteme*, 1971, *3* 188-200, cf esp pp 195 ff

But in another sense Cohnheim's work on inflammation was for Virchow a vindication. The Berlin professor had long ago provided the critical permissive factor: an institutional program flexible and resilient enough to foster creative intellectual tensions. The resulting negotiation between theoretical and institutional imperatives did not foreordain the modern notion of leukocyte function in inflammatory disease. In the early years of Rudolf Virchow's Pathological Institute, however, the social boundaries of medical knowledge were so arrayed that to seize on such a notion was possible. Julius Cohnheim was the intellectual "commuter" who first seized it.

# CARL ROKITANSKY: A REASSESSMENT OF THE HEMATOHUMORAL THEORY OF DISEASE*

ROBERT J. MICIOTTO

The New Vienna School of Medicine flourished throughout much of the nineteenth and early twentieth century. Although heir to the Old Vienna School, the new movement, as did French clinical medicine, emphasized the correlation of physical diagnosis and pathological anatomy.

Carl Rokitansky (1804-1878), a Bohemian whose medical education was acquired in Prague and Vienna, and who was influenced by the methods of the French School, is usually regarded as the most important individual in initiating the reorientation of medicine in Vienna during the third decade of the nineteenth century.[1] Rokitansky's methods of correlating the results of clinical findings with autopsy differed, however, from those of the French. In the Paris hospitals, the physician was trained as a clinician and pathological anatomist. He was expected to reach a diagnosis based on the correlation of the signs and symptoms of disease with what he himself had discovered at autopsy. But Rokitansky had been trained within a different tradition, that of the pathological anatomist or prosector. In Vienna this was a separate appointment that resulted in little clinical contact, but required primary devotion to the performance of autopsies and the collection and cataloging of gross pathological specimens for the museum of pathological anatomy. Rokitansky was successful in seeking closer relations between the clinician and the pathological anatomist, though he believed (unlike the French) that the duties of the two should be separate.[2] According to Rokitansky, the pathologist should perform the autopsy, and the clinician test his conclusions by being present at the post-mortem and by discussing the findings with the pathologist from whose cumulative experiences he would profit. The physician in turn would be relieved of spending excessive time in the dead house, and yet still have the benefits of the autopsy and the help of the pathologist. With the cooperation of Viennese clinicians, especially Josef Škoda (1805-1881), Rokitansky's ideas were eventually

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[1] Carl August Wunderlich, *Wien und Paris* (Stuttgart: Ebner and Seubert, 1841), p. 74

[2] Carl von Rokitansky, *Selbstbiographie und Antrittsrede*, ed Erna Lesky (Graz-Vienna-Cologne Hermann Böhlaus Nachf., 1960), p 80.

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implemented. In fact, Rokitansky and Škoda (a fellow Bohemian and therapeutic skeptic whose forte was physical diagnosis) were so successful that news of the changes in Vienna rapidly spread, and by the early 1840s critiques of the school and its methods by visiting physicians were being published.[3]

Regardless of Rokitansky's importance in the history of pathology, his reputation has suffered because he was the author of a hematohumoral theory of disease. It is a fact which seems to be mentioned as often as his role in founding the New Vienna School and his performance of an enormous number of autopsies.[4] Ridiculed by Virchow, the theory was subsequently regarded by many of his contemporaries and later generations of physicians (including some medical historians) as the last manifestation of humoralism[5]—an outmoded theory of disease and a temporary aberration by an otherwise gifted pathological anatomist.

Rudolph Virchow, who later became the doyen of nineteenth-century medicine, characterized Rokitansky's hematohumoralism as an *ungeheurer Anachronismus*—monstrous anachronism—in his famous review of December 1846 in the *Preussische Medizinal-Zeitung*.[6] Virchow's condemnation, usually accepted without question, has probably satisfied the curiosity of those who might otherwise be tempted to understand the origins of Rokitansky's ideas. But the weight of Virchow's opinion is not surprising when one considers that his doctrine of cellular pathology emerged in the decade following the review as one of the most important medical concepts of the nineteenth century.[7]

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[3] Wunderlich, *op cit.* (n 1 above); William R. W Wilde, *Austria, its Literary, Scientific and Medical Institutions etc.* (Dublin· W. Curry, Jr., and Company, 1843), from Max Neuburger, *Die Wiener medizinische Schule im Vormarz* (Vienna: Rikola, 1921), ch 8, pp. 247-253

[4] Rokitansky's department of pathological anatomy performed approximately two thousand autopsies a year, plus those resulting from his position as Supervisor of Medico-Legal Autopsies. Erna Lesky, *Die Wiener medizinische Schule im 19 Jahrhundert* (Graz-Cologne: Hermann Böhlaus Nachf., 1965), p. 137

[5] Esmond R. Long, *A History of Pathology* (New York: Dover Publications, 1965) pp 107, 108 Virchow however did not suggest that Rokitansky's hematohumoralism was anything other than nineteenth-century in origin "His remarkable theory of hemorrhagic exudates stems from Laennec, his view of capillary phlebitis belongs to Cruveilhier and . . . his humoral pathology has its roots in Magendie." L J Rather, "Virchow's Review of Rokitansky's *Handbuch* in the *Preussische Medizinal-Zeitung*, December 1846," *Clio Medica*, 1969, *4* 132.

[6] Rudolph Virchow, "Rokitansky, Handbuch der pathologischen Anatomie," *Medizinische Zeitung des Vereins* für *Heilkunde in Preussen, Literarische Beilage zur medicinischen Zeitung*, December 1846, no. 49, pp 237f., no 50, pp 243f.

[7] Lelland Rather, "The Place of Virchow's 'Cellular Pathology' in Medical Thought " This is an introductory essay to a republication of the English translation of Virchow's *Cellular Pathology* (New York· Dover Publication, 1971), p v.

The basic contention of this paper is that part of Virchow's judgement of Rokitansky's theory failed to properly acknowledge that his fundamental premise—blood proteins as ultimate arbiters of disease—represented a portion of the theoretical mainstream of medicine at that time. In the 1820s, 1830s, and 1840s two of the outstanding medical experimentalists of France, François Magendie (1783-1855) and Gabriel Andral (1797-1876), were writing that the results of their work indicated an increased role for the constituents of the blood in causing disease.[8] In addition, Rokitansky's inclusion in his theory of the blastema doctrine of cellular genesis (as formulated by Schwann) and his insistence on the importance of physiological experimentation, and of chemical analyses of the blood, as well as his use of the microscope, lend further credence to the belief that he stood well within the confines of orthodox medical thinking during this period.[9] Thus the popular impression of Virchow as the saviour of nineteenth-century medicine from the horrors of humoralism (a concept which he sometimes promoted) should be reconsidered.[10] There are few indications that Rokitansky's theory met with instant rejection, or that Virchow's review was immediately accepted as the final word on Rokitansky's ideas. In a review in an American medical journal of his textbook in 1856, ten years after Virchow's comments, there is still caution in its treatment of the hematohumoral theory: "The above theory will not perhaps, be adopted in all its particulars, by other pathologists . . . still it is an effort in the right direction."[11] Also to be remembered is that as late as 1853 (seven years after the review, and two years prior to *omnis cellula a cellula*), Virchow himself was still expressing belief in intracellular blastemas as a productive source of cellular contents.[12] In fact, one of the ironies of Virchow's depiction of many of Rokitansky's ideas as outmoded and unproven occurs in his negative response to the Rokitansky suggestion that an alternative method of cellular production may be, "The propagation of nuclei and cells . . . within a parent cell."[13] It is a concept which the father of cellular pathology derides as having been concluded "by means of the most arbitrary playing with facts and explanations. . . ."[14]

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[8] Gabriel Andral, *Précis d'anatomie pathologique*, vol 1 (Paris: Chez Gabon, 1829), pp 1-10, 524-567, *passim*. *Idem*, *Essai d'hématologie pathologique* (Paris: Fortin, Masson, 1843), pp 5-7 François Magendie, *Lectures on the Blood and the Changes Which it Undergoes During Disease* (Philadelphia: Haswell, Barrington, and Haswell, 1839), p. 15

[9] Carl Rokitansky, *A Manual of Pathological Anatomy*, vol 1, trans William E Swaine (London: Sydenham Society, 1854), pp xii, 4, 363

[10] Rudolph Virchow, "One Hundred Years of General Pathology," trans. L J Rather, *Disease, Life, and Man* (Stanford Stanford University Press, 1958; reprint ed , 1971), pp. 205, 206.

[11] John C Dalton, *A Manual of Pathological Anatomy*, by Carl Rokitansky, *The American Journal of the Medical Sciences*, 1856, *31* 136.

[12] " . . und das überall nur endogene Proliferation . . also aus Inhalts-Blastem zugestanden wird " Rudolph Virchow, "Ueber ein zusammengesetztes, gallertartiges Cystoid mit ausgezeichneter Recidivfahigkeit," *Virchow Archiv*, 1853, *5* 239

## II

Rokitansky's theory of disease was described for the first and only time in volume one of his textbook on pathological anatomy, *Handbuch der pathologischen Anatomie*. Published in 1846, it was the third and final volume of a set of texts which had appeared in reverse order.[15] The first two volumes had been devoted to a description of the gross (and occasional microscopic) features of the organ systems, while the final volume (actually volume one) considered the general principles of pathological anatomy including the blood crasial theory of disease. The second edition of the textbook, published between 1855 and 1861, deleted the theory—a fact which Virchow notes in his second and more complimentary review.[16]

Rokitansky conceived of most disease as the result of an improper balance of the protein components of the blood plasma: fibrin and albumin. Although he did not state the origin of his concept of health and disease, the idea is of course similar, at least in one respect, to that found in the Hippocratic work on the *Nature of Man*. Here the term crasis (*κρᾶσις*) is used to indicate a state of health as reflected in an equal distribution of the four humors: "Now he enjoys the most perfect health when these elements are duly proportioned to one another in respect of compounding, power and bulk, and when they are perfectly mingled."[17] Rokitansky's idea of health was similar (the balance of fibrin and albumin as opposed to that of the four humors) though fundamentally different from that of the Greeks.

By the time William Osler published his famous textbook in 1892, an excess of fibrin as one characteristic of disease was an old story. "A striking feature in the blood-slide is the richness and density of the fibrin network" wrote Osler in describing pneumonia. "This corresponds to the great increase in the fibrin elements, which has long been known to occur in pneumonia, the proportion rising from four to ten parts per thousand."[18] Did any of the reviewers of *The Principles and Practice of Medicine* accuse Osler of reviving Greek humoral pathology?

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[13] Rokitansky, *op. cit* (n. 9 above), p 102

[14] Virchow, *op. cit* (n. 5 above), Rather trans , p 134.

[15] Carl Rokitansky, *Handbuch der pathologischen Anatomie*, 3 vols (Vienna: Braümuller and Seidel, 1842-1846) vol 1: 1846, vol. 2: 1844, vol. 3: 1842.

[16] "Wie viele Gelehrte mag es geben, welche eine solche Resignation und zugleich eine solche Ausdauer besitzen, um so viel aufzugeben und nach so grossen Verlusten noch die Neigung zu behalten, weiter fortzuarbeiten! wie hoch müssen wir den Mann schätzen, der so viel vergessen und so viel lernen konnte, und dem es nicht genügte, seine Irrthümer zuerkennen. sondern der die mühe nicht scheute, sie durch neue Resultate sorgfaltigerer, mehr methodischer Forschungen auch bei seinen Schülern zu verdrangen!" Rudolph Virchow, "Die neue Auflage von Rokitansky's allgemeiner pathologischer Anatomie." *Wiener medizinische Wochenschrift*, June 1855, no 27, p. 416.

[17] Hippocrates, *Nature of Man*, vol. 4, trans, W H S Jones (Cambridge Harvard University Press, 1967), pp 11, 13

Rokitansky was also influenced by the work of Magendie and Andral, and agreed with their conclusions on the importance of the plasma proteins in disease. He was, however, less equivocal in his statements on their etiological role: "Not alone has pathological anatomy demonstrated the existence of blood disease in unlooked for detail. . . . It has, we think, decided in favor of a humoral pathology. . . ."[19] He maintained that disease represented either a qualitative anomaly or a quantitative imbalance of the blood proteins which was uniquely characteristic for each disorder, and then used a system of nomenclature in which diseases (cancer, typhus, tuberculosis, etc.) were labeled as dyscrasias or conditions of crasial imbalance. (He used the term *crasis* to mean both a normal and abnormal balance of fibrin and albumin.)[20] Thus the blood protein condition that produced tuberculosis could be described as a tuberculous crasis or dyscrasis.

In order to explain the lesions of dyscrasias, Rokitansky used the cytoblastema concept as developed by Theodor Schwann.[21] Schwann had theorized that the cells which comprised the tissues of the body originated in the plasma which he believed normally passed from the blood vessels into the intercellular spaces.[22] Rokitansky explained the origin of the cells which comprised lesions in the same manner, but unlike Schwann, he believed that the lesion was an expression of a disordered plasma protein balance which developed from what he labeled a "blastema," although other equivalent terms were often used: blastemata, exudate, inflammatory exudate, and secretion.[23] Blastemas or blastemata could be solid or liquid and consisted primarily of fibrin and albumin.[24] The process of blastemic deposition (the passage of plasma from the vascular tree) was most often achieved by inflammatory exudation and explained according to the neuropathological theory of Henle: paralysis of the vascular nerves caused stasis of blood flow with consequent outpouring from the vessels. Rokitansky further explained that the nerve paralysis itself was due to the effect of the dyscrasial blood proteins: "by the irritating effect of the dyscrasial blood upon the sensitive nerves. . . ."[25]

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[18] William Osler, *The Principles and Practice of Medicine* (New York, 1892), pp 520-521

[19] Rokitansky, *op cit*. (n 9 above), p 362

[20] Crasis is defined in the *Oxford English Dictionary* as "The blending or combination of elements, 'humors,' or qualities, in the animal body, in herbs, etc ", *s v.* "crasis."

[21] "We will name this substance in which cells are formed, cell-germinating material (Zellenkeimstoff), or cytoblastema. The cytoblastema, or the amorphous substance in which new cells are to be formed, is found either contained within cells already existing, or else between them in the form of intercellular substance." Theodor Schwann, *Microscopical Researches into the Structure and Growth of Animals and Plants*, trans Henry Smith (London Sydenham Society, 1847), pp. 40, 168

[22] *Ibid* , p. 169.

[23] Rokitansky, *op. cit* (n 9 above), pp 88-109, 362-381 *passim*

All dyscrasial lesions were classified as "Anomalien der Textur" (tissue) which, as "new growths," were then subdivided into those lesions which were "organized" and those which were "unorganized." Organized new growths represented the majority of lesions (including malignant and benign) and were classified on the basis of having enough gross and microscopic characteristics (cells, fibres, etc.) to allow the use of the term "organized" to be applied to their description: "Nor do they differ less widely and essentially [from normal tissues] as to origin and development. In this respect they often, it is true, follow the laws of cell-formation—cytoblastema, elementary granule, nucleus, cell fibre."[26] A much smaller and less pathologically significant number of lesions were the acellular non-organized new growths, which included concretions, horny substances, encysted colloid, fats, etc.[27]

Rokitansky also divided the blood dyscrasias into two basic types, the "primitive," and the "consecutive."[28] Primitive dyscrasias, representing the most common and basic type of crasial imbalance, were considered congenital in origin and thus permeated the entire vascular system. Consecutive dyscrasias (which could be due to unnamed external factors) were evident in only one area of the body, though they were capable of spreading and becoming general or primitive:

> This happens—(a) Through resorption of the effused products by means of the lymphatics, or immediately into the veins. (b) Through reception into patent [sic] blood vessels . . . of products thrown out into the cavity of larger blood vessels,—pus, for example. (c) Most of all, through the off-flowing and the return into the veins of plasma degraded in the local process, in a manner corresponding with the quality of the exudate.[29]

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[24] *Ibid.*, pp 88, 93.

[25] *Ibid.*, pp. 121, 131.

[26] *Ibid.*, p 80

[27] *Ibid* , pp 329, 330

[28] *Ibid* , p 364

[29] *Ibid.*, p 365.

Rokitansky maintained that both normal and abnormal crases could convert to form other dyscrasias—a process which he termed metaschematism, and which took two forms, the protopathic and the deuteropathic.[30] In protopathic metaschematism a *normal* crasis changed into a dyscrasis, while in deuteropathic metaschematism one dyscrasis changes into another, i.e. cancer to tuberculosis. Deuteropathic crases, Rokitansky wrote, "can occur in the simplest manner, as impoverishment of the blood in one or more ingredients, [fibrin, albumin] drained away by excessive deposition into textures or upon membranous expansions."[31]

Despite the confusing terminology, Rokitansky's theory rested upon a simple concept: disease was caused by an innate and characteristic imbalance of plasma proteins which caused lesions to form from blastemic pools. The pathological characteristics of such lesions indicated to Rokitansky that his theory was correct, though he apparently believed that chemistry would ultimately prove the validity of his impressions: "In fine, both kinds of research [pathological anatomy and chemistry] should go hand in hand. For, although a deeper insight into the changes suffered by the blood may be reserved for chemistry, it must needs be based upon sound anatomico-humoral premises."[32]

He was seeking a greater union of pathology and chemistry in medicine, and called for a "rational standard of pathological chemistry."[33] In so doing, Rokitansky was reflecting an awareness of the emergence and growing importance of chemistry at this time, as well as demonstrating a realization of the relevance of chemical studies for medical science, particularly pathology.[34] Rokitansky himself offered some chemical explanations in his text, but was usually content to rely on more sophisticated investigators for some of his basic ideas. Thus the Dutch chemist G. J. Mulder's concept of the oxidation of protein resulting in different grades of fibrin was used by Rokitansky to explain the differences

[30] *Ibid.*

[31] *Ibid.*

[32] *Ibid.*, p. 363.

[33] *Ibid.*, p. 4.

[34] As has been pointed out by Lawrence Holmes in his introduction to Liebig's *Animal Chemistry*, the decade 1840-1850 (volume one of Rokitansky's *Handbuch* appeared in 1846) was an extremely important period in the history of physiology as regards its eventual application of chemical concepts and techniques in laboratory research. Also within this context is the generally positive reception of Liebig's *Animal Chemistry* in 1842. Frederic L. Holmes, "Introduction," Justus Liebig, *Animal Chemistry or Organic Chemistry in its Application to Physiology and Pathology*, ed. W. Gregory and J. Webster (New York: Johnson Reprint Corporation, 1964), pp. vii, lviii-lxiii.

in various fibrin crases—[35]an explanation which met with sharp criticism from Virchow.[36]

Virchow's review appeared in two supplements of the *Preussische Medizinal-Zeitung* in December 1846.[37] At age twenty-five he was at the beginning of his long career in medicine, and had just succeeded Robert Froriep as prosector at the Charité Hospital in Berlin. Probably aware of the opportunity for recognition, Virchow in his review was extremely critical, filled in fact with ridicule for Rokitansky's ideas. "This is no longer the simple pathological anatomist whom we have learned to know in the earlier parts: it is the recognized master for whom his territory has become too small. We know indeed from where this mania for enlargement takes its origin."[38] Not only did he maintain that the substance of the theory was incorrect, but accused Rokitansky of such academic sins as inadequate citation of references, dogmatic manner of presentation, and unfamiliarity with the use of sound scientific method.[39]

The majority of Virchow's criticism was directed at the hematohumoral concept of the theory itself, although a significant portion was also devoted to its chemical and histologic underpinnings. Contesting Rokitansky's statement that pathological anatomy had advanced the study of chemical pathology, Virchow accused him of describing chemical differences "where only physical and morphological are present. . . ."[40] Then rather systematically, Virchow listed some of Rokitansky's chemical errors and suggested that certain statements were so obscure as to be unintelligible.[41]

When he discussed Rokitansky's use of the microscope, Virchow noted that it was only in 1844 that microscopical observations had appeared in volume two of Rokitansky's text on special pathology. How, he inquired, was it possible for someone to have made so many microscopical discoveries in so short a period? "We consider it impossible to have made such great discoveries in such a short time, discoveries of which all of the microscopists in the world taken together have had no conception."[42] His reference was not only to observations related to the theory of disease, but to other statements, such as Rokitansky's proposal for four types of cellular genesis and fourteen ways that tissue fibers may develop.[43] Virchow concluded that Rokitansky, like Martin Barry, an English embryologist, had seen much under the microscope, but nevertheless had interpreted everything arbitrarily.[44]

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[35] Rokitansky, *op. cit* (n 9 above), pp 36, 136.

[36] Virchow, *op cit* (n. 5 above), Rather trans., p. 133

[37] See above n 6

[38] Virchow, *op cit* (n. 5 above), Rather trans., p. 130

[39] *Ibid* , pp 132 f

[40] *Ibid*

[41] *Ibid* , p 133

[42] *Ibid* , p 135

Virchow also questioned the logic of a humoral explanation of disease which made use of neural pathological views.[45] Neural pathology, the leading solidist disease theory at this time, was based on the belief that all disease could be traced to an abnormality in the nervous system. As indicated, Rokitansky used the so-called neuropathological theory of Henle to explain the decreased blood flow and exudation of the inflammatory response. "What kind of logic is it for a humoral pathology to begin with a neural pathological theory of inflammation?"[46] However, as Lelland Rather has pointed out, these two theories were not mutually exclusive, and a certain sharing of ideas was not uncommon.[47]

Virchow's few words of praise for Rokitansky were saved for the final paragraph. After mentioning that "great powers have a natural tendency toward arbitrariness, toward despotism . . . ," he then lauded Rokitansky's post-mortem descriptions of typhoid victims and alcoholics ("masterpieces of presentation").[48] However, he believed that the real climax of the book ("Glanzpunkt des Werkes") was Rokitansky's suggestion that protein is capable of undergoing a chemical change into fat.[49] This was a conclusion which he somewhat immodestly added had been verified by his own work in Berlin.

Despite its sarcasm and polemical tone, Virchow's review was essentially accurate. Rokitansky had violated the canons of sound scientific method and engaged in speculation beyond mere scientific theorizing. But in spite of his errors, Rokitansky's idea of the blood proteins as the crucial elements of disease was a concept which represented a strong and medically legitimate area of investigation during this period. In addition it should be noted that Virchow's criticism of Rokitansky's theory as being anachronistic for raising the issue of solidary versus humoral pathology failed to observe the intimate relation and dependence of the hematohumoral theory upon the solidistic proofs of pathological anatomy: "Pathological anatomy strives not only to understand the fully developed disease but also to investigate its *origin* [my italics], its further development, its decline and generally its ways of termination in the material substrate. . . ."[50] Rokitansky's humoral pathology, though based upon hematological factors, was dependant for its verification on the solidism of pathological anatomy.

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[43] *Ibid* , p. 134.

[44] *Ibid.*, p. 135.

[45] *Ibid* , p 137.

[46] *Ibid*

[47] Rather, *op cit*. (n. 7 above), p x.

[48] Virchow, *op. cit* (n 5 above), Rather trans., p 139

[49] *Ibid*

Erna Lesky has suggested that Rokitansky's frustration in attempting to assign an organ locale for all diseases caused him to seek a more general and pervasive etiological factor:

> Other diseases of a general nature must be in existence apart from the diseases of the organs proper, and it should be possible to localize them too. There was no disease process without changes occurring in material substance, but only *one* material substance could be thought of which was always present everywhere in the body—blood.[51]

As regards the ultimate origins of the hematohumoral theory, Rokitansky himself provided few overt clues,[52] although it is obvious that some of his ideas were similar to those of Magendie and Andral. They in turn represented a trend in medical experimentation and thought which began as early as the seventeenth century, finally culminating with the work of Rokitansky.

## III

Writing in chapter sixteen of *De motu cordis*, William Harvey offered as further evidence of his hypothesis of the circulation of the blood, the idea that the spread throughout the body of the morbid signs and symptoms of disease often resulted from a local wound or lesion:

> The phenomena alluded to are those that present themselves in connexion with contagious, poisoned wounds, the bites of serpents and rabid animals, lues venera and the like. We sometimes see the whole system contaminated, though the part first infected remains sound. . . . Whence it appears that the contagion impressed upon or deposited in a particular part is by and by carried by the returning current of blood to the heart, and by that organ is sent to contaminate the whole body.[53]

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[50] L J Rather and Eva R. Rohl, "An English translation of the hitherto untranslated part of Rokitansky's *Einleitung* to volume one of the *Handbuch der allgemeinen Pathologie* (1846), with a bibliography of Rokitansky's published works," *Clio Medica*, 1972, *7:* 219.

[51] Erna Lesky, *The Vienna Medical School of the 19th Century*, trans. L Williams and I S. Levij (Baltimore. The Johns Hopkins University Press, 1976), p. 110

[52] In both reviews Virchow maintained that Rokitansky had been influenced by *Naturphilosophie*, a charge belied by Rokitansky's *Antrittsrede* of 1844 in which he indicated an awareness of its limitations. Rokitansky, *op cit* (n 2 above), p 77

[53] William Harvey, *An Anatomical Disquisition on the Motion of the Heart and Blood in Animals*, trans Robert Willis, *Classics of Cardiology*, ed. Frederick A Willius and Thomas E. Keys (New York. Dover Publications, 1961), vol 1, p 68.

This intimate association of the blood and disease in so influential a medical text encouraged medical theorizers as early as 1695[54] to suggest that the blood was not merely a vehicle for the spread of disease, as indicated by Harvey, but was itself one of the most important substances of the body involved in the morbid process. Examples of this trend were Friedrich Hoffmann (1660-1742) and John Hunter (1728-1793), major medical experimentalists with hematohumoral sympathies.

Hoffmann, who spent most of his career at the University in Halle, developed a more formalized theory of disease than did Hunter. In his *Fundamenta medicinae*, he applied a mechanistic interpretation to the physiology and pathology of the body, and, reflecting the influence of Harvey, maintained that the proper understanding of disease should be sought both in the circulation of the blood, and intrinsically, in the presence in the blood itself of certain particles which he considered to be among the ultimate arbiters of disease: "The most important circumstance of life and health lies in the circulation of the blood. Here therefore should be sought the origin of many diseases."[55] As for the role of the unseen particles themselves, Hoffmann believed that disease developed when they were altered within the bloodstream: "A malignant disproportion . . . arises when the mixture of particles in the blood is destroyed, in respect to position, movement, and order."[56] It should be added that although these ideas were fundamental to Hoffmann's theory, it would be an oversimplification to describe him as nothing more than a hematohumoralist. He also stressed the importance of "nerve fluid" in disease (an idea which gained momentum) and emphasized the relationship between the fluids and solids of the body in some pathological conditions.[57]

John Hunter also believed in the close relationship of the blood and disease. But unlike Hoffmann's, his ideas were not given systematically nor made within the context of a carefully conceived theory of disease. In his *Treatise on the Blood, Inflammation, and Gunshot Wounds*, he described the two methods by which he believed the blood could best be studied.[58] The first was simple observation of the blood during circulation, and the second, "that of examining the blood when it is out of the body, [which] enables us to observe whatever relates to its spontaneous changes and separation. . . . Its chemical properties become likewise known by this second mode. . . ."[59] Accordingly, portions of his book describe the results of Hunter's subjection of the blood of animals and humans (normal and diseased) to various chemical and physical manipulations, such as the mixing of serum with wine, and exposing it to heat, in an attempt to understand coagulation.[60] Hunter did not consider the blood as the source of all disease, but rather, the most desirable substrate upon which the effects of a morbid process could be observed: "The nature and appearance of the blood is more expressive of disease when removed from the body than any of the solids, and undergoes changes which the solids do not. . . ."[61] That these ideas were still viable and shared a generation later in France can be seen in the work of Gabriel Andral and François Magendie.

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[54] The original publication date of Friedrich Hoffmann's *Fundamenta medicinae*

[55] Friedrich Hoffmann, *Fundamenta medicinae*, trans. Lester S King (New York American Elsevier Publishing Company, 1971), p 58

[56] *Ibid* , p 40

[57] *Ibid* , p. 39

[58] John Hunter, *A Treatise on the Blood, Inflammation, and Gunshot Wounds* (Philadelphia: Haswell, Barrington, and Haswell, 1840), p 21 The original date of publication was 1794

Andral was the author, in 1829 and 1843, of two works which addressed the question of the role of blood in disease. Both his *Précis d'anatomie pathologique* and his *Essai d'hématologie pathologique*[62] reflect his feeling that he was part of an era which was resurrecting the study of the blood as an important factor in disease: "One of the glories of the present medical epoch . . . [is that] it has returned to the study of the alterations of the blood; it has recognized their existence, and has afforded them an influence in the production of disease."[63] Working in an era in which solidistic pathology had been dominant since the seventeenth century,[64] Andral believed that the study of the role of the blood in disease had been maligned, and suggested that many of the body's abnormalities ultimately would be traced to a disturbed interaction between the blood and the solids: "Every alteration of the solids must be succeeded by an alteration of the blood just as every modification of the blood must be succeeded by a modification of the solids"[65]—an idea similar to that of Friedrich Hoffmann's.[66]

In volume one of his *Précis d'anatomie pathologique*, Andral described the fundamental actions of all living matter as capillary circulation, nutrition, and secretion.[67] The blood was the chief component of each function, and the entire system coordinated through the action of the nerves. Based on this framework, Andral characterized disease as "various modifications of those actions which have already been ennumerated as essential to man in health."[68] Thus lesions of capillary circulation could alter the supply of blood to an organ, resulting in either anaemia or hyperemia, while lesions of nutrition affected the solids, and included tissue hypertrophy and ulcerative lesions, such as gangrene.[69] Andral's lesions of secretion, like Rokitansky's blastemic exudates, originated in the blood and were then deposited outside the vascular system to give rise to lesions.[70] Although Andral stresses the importance and coordinating capacity of the nervous system, he did not, as did Rokitansky, explain inflammatory secretions on the basis of capillary paralysis.[71]

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[59] *Ibid* , p 23

[60] *Ibid* , p. 57

[61] *Ibid* , p 21

[62] See above, n. 8.

[63] Gabriel Andral, *An Essay on the Blood in Disease*, trans J F. Meigs and Alfred Stillé (Philadelphia Lea and Blanchard, 1844), pp 15 f It should be noted that Andral's hematohumoralism was fundamentally different from Galenic humoralism, although both shared the belief that the blood was a causative factor in disease For a differing view see Walter Pagel, "Humoral pathology A lingering anachronism in the history of tuberculosis," *Bull Hist Med.*, 1955, *29*· 307 f

[64] Erwin H. Ackerknecht, *A Short History of Medicine* (New York Ronald Press, 1968), p. 122

[65] Gabriel Andral, *A Treatise on Pathological Anatomy*, vol 1, trans Richard Townsend and William West (New York. Samuel Wood and Sons, 1832), p 391

[66] "Disproportion does not affect the solid parts except as it is derived from the fluid parts." Hoffmann, *op cit*. (n. 52 above), p. 39

In both his *Précis* and *Essai*, Andral mentioned two important figures in French medicine, Xavier Bichat and Magendie, as examples of individuals who realized the importance of humoral factors as causes of disease. Andral quotes Bichat, at the beginning of his section on lesions of the blood in the *Précis*, as someone who has noted that the study of the humors had heretofore been slighted in favor of the solids.[72] Magendie, because of his specific work on the blood, and his contemporary stature as a respected experimentalist, had a more direct influence on Andral than Bichat.[73] He had written an extensive analysis of the results of his hematological researches which he initially expounded in a series of twenty-four lectures at the Collège de France in 1837 and 1838. Published first in English, Magendie's *Lectures on the Blood* consisted primarily of descriptions of the results of exposure of the blood and vascular system to physical and chemical stimuli in order to ascertain various characteristics and properties.[74] One of his more trenchant statements, especially as related to the thought of Andral and Rokitansky, was given in his second lecture:

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[67] Andral, *op cit* (n 65 above), pp 3 f

[68] *Ibid.*, p 5.

[69] *Ibid.*, pp 11, 56, 123, 143

[70] "The different secretions which are separated from the blood in the parenchymatous structure of the organs, or on their surface, may be altered in their quantity, or in their quality, and hence the lesions of secretion " *Ibid* , p. 5

[71] See above, p. 188.

[72] "At a period when an exclusive solidism was the prevailing theory in France, Bichat, in the introduction to his *Anatomie generale*, wrote as follows: 'The humeral [sic] pathology has, no doubt, been carried too far; but it is founded on truth, *and in a great many cases*, we must allow that all should be referred to morbid humours ' This idea was in a manner lost in his work, without being followed up either by himself or his contemporaries " Andral, *op cit*. (n. 65 above), p 388.

[73] "To progress we must analyze the blood: such is the idea which guided M Magendie when, by modifying artificially its composition, he proved by these means that we can create disease " Andral, *op. cit.* (n 63 above), p. 16 "By the direct analysis of the blood of the sick, I have arrived, in this respect, at results similar to his [Magendie's] own." *Ibid* , p 17.

> Nor can the important truth, I feel convinced, have escaped you, that in ultimate analysis the majority of questions bearing on the causes and mechanism of disease, are more or less closely connected with the study of chemical, physical, and vital properties of the blood.[75]

This was a concept which influenced Andral's publication devoted solely to diseases of the blood, *Essai d'hématologie pathologique*.[76] Published in 1843, this work of Andral attempts to characterize the blood primarily as to its cell and fibrin content in various disease states.[77] He cites as his basic methods of examination, chemistry and the microscope, although he provides only a few details of descriptive technique.[78]

The importance of this work as related to Rokitansky's theory is that it provides a conceptual link with his belief in the importance of the fibrin content of the blood in various diseases.[79] Although Andral is never as definitive as Rokitansky in his statements, nonetheless he leaves little doubt that he considers the fibrin and blood cells the two most important components of the blood in disease:

> To what false results then should we not arrive, were we to take as our point of departure in diseased animals, the physiological proportions of the globules and fibrine of man, in order to calculate the changes of composition that the blood may have undergone in these animals, as a consequence of disease.[80]

The extent of the influence of these ideas on Rokitansky is evident only in the similarity of the concepts, and from a fleeting reference which Rokitansky makes to Andral in his discussion of the fibrin-crases and the importance of qualitative fibrin changes in disease.[81]

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[74] Examples of Magendie's lecture titles included: "Effects of the Injection of Cold Water into the Veins" and "Action of Oil on the Blood," Magendie, *op. cit.* (n. 8 above), pp. 6 f

[75] *Ibid* , p. 15

[76] See above, n. 8.

[77] This included: plethora, anaemia, pyrexia, phlegmasiae (inflammations), hemorrhages, dropsies, organic diseases, and neuroses. Andral, *op cit* (n 63 above), pp 38, 43, 52, 60, 92, 107, 117, 128.

[78] *Ibid* , pp 15-35, *passim*.

[79] The fibrin crases of Rokitansky are the most numerous of the altered protein states of the blood

[80] Andral, *op. cit.* (n. 63 above), p. 29.

[81] "However certain may be the excess of fibrin, its qualitative deviation becomes more and more distinctly pronounced in proportion as in the series of fibrin-crases [as] the forms recede from the characters of *true* fibrin This, with the exception of a few hints thrown out by Andral, has been hitherto ignored " Rokitansky, *op cit*. (n. 9 above), pp 366 f

When Rokitansky's text appeared in 1846, it was apparent that he had been influenced by the ideas of Magendie and particularly Andral. He recommended the continued testing of the blood and implied that the ultimate vindication of his theory would be made in the laboratory.[82] But Rokitansky also believed that his own work in pathological anatomy had formed the basis of hematohumoral pathology by its identification of the blood crasial protein imbalance as the fundamental mechanism of disease.

> Not alone has pathological anatomy demonstrated the existence of blood diseases in unlooked for detail; it has at the same time solved a problem of the weightiest import. It has, we think, decided in favour of a humoral pathology, by demonstrating a primitive anomaly of the blastemata; by demonstrating the endogenous impairment of the blood within the vascular system . . . lastly, by demonstrating the dependence of local morbid action upon pre-existent impairment of the general circulation.[83]

An ambivalent theory, Rokitansky's concepts had the attraction of one clearly visible, and experimentally accessible, locus of disease. Yet the inadequacy of its explanation proved dismaying to many, including Rudolph Virchow. Despite its deficiencies, however, the theory was a partial extension of ideas cultivated by such respected medical scientists as Schwann, Magendie, and Andral. Judgement of Rokitansky's theory should also be tempered with the realization that his connection of bodily fluids with disease was even acknowledged by Virchow[84] to be partially correct—a position that has gained further acceptance during the present century.[85]

## IV

Rokitansky's motivations for going to extremes in his theory still remain unclear. He had taken legitimate, but as yet unsettled areas of biological and chemical inquiry and incorporated them with his own work in pathological anatomy in order to create a definitive system of disease. Lester King has written that Rokitansky's motivations and methods reflected his tendency to view pathology with a vision more akin to the previous century, that his "intellectual spectacles gave an eighteenth century tint to everything he saw."[86] This is both true and untrue. His system was similar to the eighteenth-century attempts to base medical doctrine, especially the explanation of disease, on a key concept which explained all the ills of the body.[87] It should be added that Rokitansky was not alone in the first half of the nineteenth century in proposing a unitary concept of disease. Both F. J. V. Broussais (1772-1838) and Jean Cruveilhier (1791-1874) also believed that disease could be traced to one source, and shared the opinion that inflammation was the most important pathological response of the body. For Broussais most diseases were thought to originate from the inflammation of mucous membranes.[88] Cruveilhier, the holder of the first chair of pathological anatomy in Paris, wrote in 1834 that capillary phlebitis was the most pervasive etiological factor in disease, and a conceptual link between ancient and modern humoralism.[89] But it should be stressed that although Broussais and Cruveilhier were both unitarians, neither carried his ideas to the extreme degree of Rokitansky.

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[82] See quote above, p. 189.

[83] Rokitansky, *op cit*. (n 9 above), p. 362.

[84] "Practical reason indeed postulated a fusion of solidary and humoral pathology " Virchow, *op cit* (n. 5 above), Rather trans , p 136.

[85] "Virchow was the first man to recognise the importance of the cell as a unit, both in health and disease He introduced 'cellular pathology,' and laid the foundations of pathology as a rational science. During the nineteenth century the rival school of thought was that of humoral pathology, which attributed disease to alterations in the chemical composition of the body fluids Advances in biochemistry and physiology have reconciled these opposing views, and shown that each contained part of the truth " M. J. R. Dawkins and K R. Rees, *A Biochemical Approach to Pathology* (London Edward Arnold Ltd., 1959) p v In this regard Lelland Rather's comments are also relevant. "As for the value of Rokitansky's contribution, the biochemically oriented pathologist today is apt to find his attempt to assign various pathological roles to diseased proteins circulating in the blood plasma (from which blastemas were derived) a genuine if insufficiently based *aperçu* " Rather, *op cit* (n 5 above), p. 129

For those who believe that Rokitansky's theory reflects the influence of transcendental thought,[90] it should be noted that in his introductory comments he insisted that pathological anatomy should only be based on observable organic change.[91] Here he presented a philosophy of pathology which was grounded in the belief that structural alterations, i.e. lesions, are the most important and only objects of pathological inquiry; and, that although so-called dynamic forces may exist, they can only be known through the study of the material substrate of disease, the lesion itself:

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[86] Lester S. King, *The Growth of Medical Thought* (Chicago. The University of Chicago Press, 1963, reprint ed., Chicago Midway, 1973), p. 197

[87] This had been done for instance by John Brown (1735-1788) of Edinburgh who believed that states of nervous excitement formed the basis of all disease John Brown, *The Elements of Medicine* (Philadelphia. James Webster, 1814), pp. 22, 29, 31 This book was originally published in 1780.

[88] F. J V. Broussais, *Examen des doctrines médicales et des systèmes de nosologie*, vol. 1 (Paris Delaunay, 1829), pp. xxx-xxxvii, *passim*

[89] "La phlébite domine en quelque sorte la pathologie tout entière: elle est le lien qui unit l'humorisme aveugle et comme instinctif des anciens, avec l'humorisme rationnel des modernes." *Dictionnaire de médecine et de chirurgie pratiques*, 1834 ed., *s v.* "Phlébite," by Jean Cruveilhier, vol. 12, p. 637.

[90] In addition to Virchow (see above, n. 52), this included William E. Swaine, the translator of volume one of the *Handbuch* Rokitansky, *op cit* (n. 9 above), p. viii For a similar opinion see: Martin Müller, "Rokitansky's Krasenlehre," *Sudhoffs Archiv für Geschichte der Medizin*, 1930, *23* 16-21

[91] Rokitansky, *op. cit*. (n. 15 above), vol. 1, p. 2

> It follows that every phenomenon is determined by matter, and that our knowledge of those forces (of which we have in general some little understanding) is limited to sense-accessible changes in the material substrates that determine their development. . . . Accordingly, matter alone can give us satisfactory information about forces.[92]

Rokitansky's apparent unawareness of the methods and limitations of sound scientific theorizing may be traced to the inadequacies of his medical education, especially in the physical and biological sciences. He himself described the deficiencies of his medical school courses at Prague in his autobiography.[93] Once Rokitansky reached Vienna, clinical training dominated his final two years at the University of Vienna before he turned to his new duties in pathological anatomy in 1827. With little training in the propriety of restrained expression in scientific theorizing, he made the mistake of letting his enthusiasm exceed his proof. Perhaps he also felt the pressures of recognition which he and the New Vienna School were receiving during this period,[94] and thus allowed himself to believe that the moment was opportune and the evidence sufficient to justify his theory.

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[92] Rather and Rohl, *op cit*. (n. 50 above), pp. 217 f.

[93] Rokitansky, *op. cit*. (n 2 above), p. 47.

[94] See above, notes 1 and 3.

# J.B.A. CHEVALLIER, PHARMACIST-CHEMIST: A MAJOR FIGURE IN NINETEENTH-CENTURY FRENCH PUBLIC HEALTH*

ALEX BERMAN

In 1824, the permanent secretary of the Paris Academy of Medicine, Étienne Pariset, presented his views on the role of pharmacists in the Academy. He pointed out that their goal was not confined simply to working for the progress of pharmacy, but rather to utilize all the resources of chemistry. "Under the modest name of pharmacy," he proclaimed, "it is chemistry itself that the King has wished to make an essential part of your institution."[1]

In fact, a significant number of French pharmacists made outstanding contributions during the nineteenth century to animal and organic chemistry, to the isolation of important constituents from natural products, to halogen chemistry, and to public health and legal toxicology.[2] The largest concentration of these pharmacist-chemists was in Paris, where their influence was felt in many learned societies, including the Academy of Medicine and the Health Council. Their status was anomalous with respect to the majority of pharmacists who devoted themselves almost entirely to compounding medicines and to drug distribution.[3]

In the Paris Health Council, the tradition begun by Charles-Louis Cadet de Gassicourt, Parmentier, and Deyeux, pharmacist members of the Council at its inception in 1802, was continued by the succeeding twenty-three titular pharmacy members, including Chevallier, appointed during the course of the nineteenth century.[4] Indeed, Chevallier seems to have possessed many of

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[1] "Discours prononcé par le secrétaire perpétuel, dans la séance inaugurale, le 6 mai 1824," *Mém. Acad Méd.*, 1828, *1* 73

[2] Alex Berman, "The scientific tradition in French hospital pharmacy," *Am J Hosp Pharm.*, 1961, *18:* 110-119; and Berman, "The pharmaceutical component of 19th century French public health and hygiene," *Pharmacy in History*, 1969, *11* 5-10.

[3] Alex Berman, "Conflict and anomaly in the scientific orientation of French pharmacy, 1880-1873," *Bull Hist. Med*, 1963, *37:* 440-462.

[4] Chevallier was promoted from associate to titular status in 1837 Among his distinguished pharmaceutical colleagues on the Council with whom he served were Joseph Pelletier, H. F. Gaultier de Claubry, A G.

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the qualifications mentioned by Parent-Duchâtelet for a seat on the Council. These included a grasp of the topography and geology of Paris and its environs, an insight into occupational health, and an understanding of the environmental impact of industrial establishments on plants, animals and man. Parent-Duchâtelet stressed the need to give chemists, especially those with applied and industrial competence, leading positions on the Council.[5]

The jurist, C. P. Collard de Martigny, concluded in 1832 that the vast majority of pharmacists as well as physicians did not have the necessary chemical skills to qualify before the courts as legal toxicologists. In the case of physicians, "most remained absolute strangers to chemical manipulation." As for pharmacists, they had a theoretical knowledge, but most lacked laboratory experience, being content only to compound prescriptions. He was nevertheless quick to point out that "it is among pharmacists that one finds most of the chemists: but these call themselves *pharmacist-chemists*, thus separating these two qualifications."[6] Although the majority of pharmacists in France during the nineteenth century did not have the special expertise mentioned by Pariset, Parent-Duchâtelet and Collard de Martigny, there was an important minority that did. Among these was Jean Baptiste Alphonse Chevallier.

## *Biographical Note*[7]

Chevallier's formative years evoke a familiar story: the poor and talented boy from the provinces coming to Paris, enduring many privations and, by dint of perseverance and luck, achieving eminence in his chosen profession. He was born in Langres (Haute-Marne), July 19, 1793. At the age of four-

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Labarraque, L. R. Lecanu, A A B Bussy, A F Boutron-Charlard, Eugène Soubeiran, F H Boudet, Apollinaire Bouchardat, A B. Poggiale, J F. Persoz, and N. T. Gobley For a complete listing of titular pharmacy members on the Paris Health Council, see Paul Dorveaux, "Pharmaciens membres du Conseil d'hygiène publique et de salubrité du département de la Seine," in *Centenaire de l'Ecole supérieure de Pharmacie de l'Université de Paris 1803-1903* (Paris A Joanin, 1904), pp. 370-371.

[5] A J B. Parent-Duchâtelet, "Quelques considérations sur le Conseil de Salubrité de Paris," *Annales d'hygiène publique et de médecine légale* (hereafter referred to as *Annales*), 1833, *9*. 248-9 For excellent discussions of the Paris Health Council, see Dora B Weiner, "Public health under Napoleon. the Conseil de Salubrité de Paris, 1802-1815," *Clio Medica*, 1974: *9*· 271-284, and Ann Fowler La Berge, "The Paris Health Council, 1802-1848," *Bull. Hist. Med* , 1975, *49*. 339-352 See also Erwin H Ackerknecht, "Hygiene in France, 1815-1848," *Bull Hist. Med* , 1948, *22*. 117-155

[6] C P Collard de Martigny, "Commentaire sur les art. 43 et 44 du Code d'instruction criminelle, en matière d'expertise médico-légale," *Annales*, 1832, 7 170-71

[7] A helpful account of Chevallier's life and work is by his student, Guillaume Sicard, *Notice sur la vie et les travaux de M. Jean-Baptiste-Alphonse Chevallier* (Paris Impr Félix Malteste, 1880) See also, Théophile Gallard, "Alphonse Chevallier," *Annales*, 3rd ser , 1880, *3*· 181-187, and A. Goris *et al.*, *Centenaire de l'internat en pharmacie des hôpitaux et hospices civils de Paris* (Paris: Impr. de la Cour d'Appel, 1920), pp 300-317

teen, he came to Paris and for the next five years worked successively as a pharmacy helper, an apprentice in the *officine* of the prominent pharmacist, P. F. G. Boullay, and as a laboratory assistant to Vauquelin at the Museum of Natural History. Called up for military service in 1812, Chevallier was wounded in Germany and, after many hardships, made his way back to the French capital. There he served a pharmacy internship at the Saint-Louis, La Pitié and the Midi Hospitals, working for a time for his old preceptor, Boullay, and finally in 1822, brilliantly completing his formal education at the Paris School of Pharmacy. Shortly thereafter, Chevallier opened his own pharmacy, which he directed until 1835. He then left the practice of pharmacy and established a private research and analytical lavoratory on the Quai Saint-Michel. That same year, he accepted a position as assistant professor of pharmacy at the Paris School of Pharmacy.

Chevallier was now forty-two years old and had already received considerable recognition for his contributions to public health, toxicology, and forensic medicine. Elected to the pharmacy section of the Academy of Medicine in 1824, he became a member of the public health section of the Academy in 1835. By this time he had also been admitted to the Council of Hygiene and Health of the Department of the Seine.[8] Chevallier joined the editorial board of the *Annales d'hygiène publique et de médecine légale* (1831) and became one of its most frequent contributors. For many years he also edited the *Journal de chimie médicale*.

Claude Lachaise, the outspoken commentator on the Paris medical establishment, recorded his impression of Chevallier speaking before the Academy of Medicine. Although he found Chevallier's manner dry and peremptory, he nevertheless praised Chevallier as one of the distinguished chemists of Paris and singled out his practical application of chemistry in resolving many important questions relating to the "*économie sociale*," by which he meant social amelioration.[9] Chevallier was indeed keenly interested in improving the human condition, as this account of his work will show.

In Paris, Chevallier was active in the Health Council, the Academy of Medicine, the School of Pharmacy, the Society for Encouragement of National Industry, as well as in his private laboratory on the Quai Saint-Michel. Among his collaborators in public health and legal toxicology were Parent-

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[8] As associate member (*membre adjoint*). Dates given for his election vary: 1831 (Sizard, p. 11); 1832 (V. de Moléon, *Rapports généraux sur les travaux du Conseil de salubrité du département de la Seine 1827-1840*, vol. 2 [Paris, 1841], p. ix), 1834 (Gallard, p. 184).

[9] C. Sachaile [Claude Lachaise], *Les médecins de Paris jugés par leurs oeuvres* . . . (Paris: chez l'auteur, 1845), pp. 189-190

Duchâtelet, Orfila, Charles-Prosper Ollivier, Tardieu, Marc, Lassaigne, Barruel and Labarraque.

After a life of unremitting labor, Alphonse Chevallier died on November 29, 1879, leaving behind an impressive record of accomplishments and a prodigious body of writings.[10] Productive to the end, he corrected the proofs of his final article for the *Annales d'hygiène* on his death bed. He was eighty-six years old.[11]

Chevallier is remembered today mostly for his classic work on the detection of adulterated and fraudulent food, drug and commercial products (*Dictionnaire des altérations et falsifications des substances alimentaires, médicamenteuses et commerciales,* 1850-1852 and later editions), but his contributions encompassed a wide range of interests. These were reflected in hundreds of reports, articles, memoirs and monographs dealing with a variety of problems: urban and environmental health, occupational health and industrial toxicology, consumer health protection, legal toxicology and other interests.

## *Urban and Environmental Health*

Among the most pressing public health problems for Paris was the wretched system of waste disposal, which despite some improvement by mid-nineteenth century, did not undergo significant progress until the Second Empire.[12] Many public health workers attempted to upgrade the conditions of sewers and cesspools during the first half of the nineteenth century. Names come to mind such as Labarraque, H. F. Gaultier de Claubry, Adelon, Marc, Girard, Pelletier, D'Arcet, and, of course, the most famous of all, Parent-Duchâtelet. To these names must be added that of Chevallier.

One of the first acts of the special commission appointed in 1826 by the Prefect of the Seine and the Prefect of Police for the purpose of cleaning up some of the worst Paris sewers was to retain Chevallier. His responsibility was to disinfect the sewers, protect the workers and local inhabitants against noxious gases, and to render first aid in case of accidents. The commission presided over by D'Arcet, and with Parent-Duchâtelet as its secretary-reporter, published its report in 1829. Chevallier was cited for his outstanding contribution: "in response to the intentions of the Prefect, it [ the commission], at its first meeting, selected Monsieur Chevallier, young chemist and distinguished pharmacist, whose interesting research has resulted in his admission to the Royal Academy of Medicine . . . the commission is duty bound to declare that he has rendered in these circumstances the greatest services, as much by his zeal and activity, as by his rare intelligence, and that the success of its operations is in part due to him."[13]

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[10] Extensive listings of Chevallier's work are given by Sicard and Goris

[11] Gallard, *op cit* (n 7 above), p 182

[12] David H. Pinkney, *Napoleon III and the Rebuilding of Paris* (Princeton, N.J. Princeton University Press, 1958) pp. 19-21, 127

Chevallier had used hypochlorites for the disinfection of these sewers, and the experience, no doubt, influenced him to describe their utility in a book published in 1829, and favorably reviewed by Alphonse Devergie.[14] His authority in this field brought him in demand during subsequent years for many important projects such as the burial and exhumation of bodies following the bloody street fighting in Paris in 1830, and for problems concerning cesspools and sewers.[15] This interest in disinfection culminated in 1862 with the publication of his comprehensive treatise on disinfectant agents.[16]

Often, Chevallier extended himself beyond his chemical laboratory to work on projects that required a thorough practical grasp, as well as great persistence and energy. A case in point is the report made by Chevallier in 1832 on the request of the Prefect of Police concerning the accumulations of mud and fetid organic material in the Saint-Martin Canal. After carefully investigating all the basins of the canal, having soundings made and samples taken, Chevallier presented a rigorous analysis of the situation, along with well-reasoned recommendations for improvements.[17] Later Chevallier made a comparative study of sewers in Paris, London, and Montpellier, and in 1848 he published his findings on the sewer serving the Bicêtre, with suggestions for revamping the system, and prefaced by an excellent historical introduction.[18]

Of the other urban and environmental health problems which Chevallier attempted to deal with, the following are representative: pollution of rivers and localities by industrial wastes; the purity of drinking and mineral waters;

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[13] "Rapport sur le curage des égouts Amelot, De La Roquette, Saint-Martin et autres. . . ," *Annales*, 1829, *2* 9-10; 59.

[14] *L'art de préparer les chlorures de chaux, de soude et de potasse . .* (Paris Bechet jeune, 1829). For Devergie's review, see *Annales*, 1829, *1* 293-295

[15] A. J B Parent-Duchâtelet, "Note sur les inhumations et les exhumations qui ont eu lieu à Paris à la suite des événements du mois de juillet 1830," *Annales*, 1830, *4* 66. See also, A. G Labarraque, J. B A. Chevallier, and A J B Parent-Duchâtelet, "Rapport sur les améliorations à introduire dans les fosses d'aisances, leur mode de vidange, et les voiries de la ville de Paris," *Annales*, 1835, *14*. 258-331; J. B. A Chevallier, *Note sur de nouveaux moyens employés pour la désinfection des matières fécales dans les fosses* (Paris F Loquin, 1835)

[16] *Traité des désinfectants sous le rapport de l'hygiène publique .* (Paris: P Asselin, 1862).

[17] "A M le préfet de police sur l'envasement du Canal Saint-Martin," *Annales*, 1832, *7*· 59-74

[18] "Mémoire sur les égouts de Paris, de Londres, de Montpellier," *Annales*, 1838, *19*: 366-424, "Notice historique sur l'égout dit le grand puisard de Bicêtre," *Annales*, 1348, *40*· 110-121.

the use of human waste as fertilizer; and, the transportation, storage and sale of inflammable and explosive materials.[19] He was particularly interested in the rapidly growing petroleum industry which he believed, if not sufficiently regulated, could pose a potential for catastrophic accidents. But he met unexpected opposition when he sought to develop stronger safety guidelines. "These studies had led us to think," he wrote in 1872, "that one could lessen the dangers . . . but we have been stopped in our investigations by . . . the difficulties that have been raised against us by persons of great merit who rejected the public security precautions we proposed; they opposed us with the high-sounding words of *free enterprise*."[20] There is a familiar ring to Chevallier's complaint!

It is still possible to read with profit three historical essays by Chevallier that appeared in the *Annales d'hygiène*. The first, published in 1849, traces the development of legal, administrative and police measures to keep Paris clean from the twelfth century on.[21] The second, which appeared in 1851, deals with similar measures relating to the distribution of water in the French capital going back to the fourth century.[22] Finally, the third essay, written with Gustave Lagneau, published in 1873, is a demographic study of Paris beginning in the seventeenth century.[23]

## *Occupational Health and Industrial Toxicology*

In a bold critique published in 1835, Chevallier attacked the prevailing literature dealing with occupational health:[24]

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[19] See for example, "Des inconvéniens qui résultent de l'écoulement, sur la voie publique des eaux hydrosulfurées provenant des bains . . ," *Annales*, 1841, *25*. 27-32; with J A. Guérard, "Mémoire sur les résidus liquides provenant des établissemens industriels," *Annales*, 1846, *36* 99-120; "Des inconvénients qui résultent de l'écoulement des eaux de décapage dans les riviéres . . ," *Annales*, 2nd ser., 1864, *21* 334-339, "De l'eau, des moyens de la purifier pour la rendre potable," *Annales*, 2nd ser , 1874, *42* 60-80, *Essai sur Chaudes-Aigues, département du Cantal, et analyse chimique des eaux minérales-thermales de cette ville* (Paris: Impr royale, 1828), "Sur la présence de l'arsenic dans les eaux minérales . ," *Annales*, 1848, *40* 110-121, "Essai sur la possibilité de recueillir les matières fécales, les eaux vannes, les urines de Paris, avec utilité pour la salubrité, et avantage pour la ville et pour l'agriculture," *Annales*, 2nd ser., 1860, *14* 97-129, "Dangers de l'emmagasinage et du maniement des substances explosives," *Annales*, 2nd ser., 1869, *32* 104-121; "Des dangers et des inconvénients que présentent les usines où l'on purifie les huiles brutes de pétrole . . ," *Annales*, 2nd ser., 1864, *21* 324-334

[20] "Recherches sur le pétrole . . ," *Annales*, 2nd ser , 1872, *38*· 277-278

[21] "Notice historique sur le nettoiement de la ville de Paris depuis 1184 jusqu'à l'époque actuelle," *Annales*, 1849, *42* 262-319

[22] "Notice historique sur la police et la distribution des eaux dans Paris depuis 360 . . ," *Annales*, 1851, *45* 5-71

[23] "Quelques remarques sur le mouvement de la population de Paris à un et deux siècles d'intervalle," *Annales*, 2nd ser , 1873, *40*· 54-63

[24] "De la nécessité de faire de nouvelles recherches sur les maladies qui affligent les ouvriers, et observations sur celles qui se font remarquer chez les imprimeurs," *Annales*, 1835, *13* 304-344

> The reading of memoirs and special works published on the illnesses of workers, and the comparison of the opinions expressed in these works with facts and observations that we have been able to make and collect, has unfortunately convinced us that most of these documents were exaggerated to the point of rendering them quite useless. . . .[25]

To support this assertion, he used the printing trade as an example, singling out especially discrepancies in Patissier's adaptation of Ramazzini, and in the work of the Englishman, C. T. Thackrah. According to Chevallier, reliable information on occupational diseases could be arrived at only through personal observations at sites of work, interviews with workers and management, consultations with physicians who had treated these workers, and finally, from the compilation of accurate statistical data from hospital records and tables of mortality. Chevallier used these methods in his investigation of Paris typesetters and pressmen. His conclusions were that printers actually lived much longer than had been claimed, and that many alleged occupational diseases attributed to the printing trade could not be substantiated.

In subsequent years, Chevallier devoted much effort to investigating the toxic effects of lead and copper compounds in industry. In a long article in the *Annales d'hygiène* in 1836, he discussed the illness contracted by workers in factories manufacturing white lead.[26] This was followed by an exhaustive report to the Prefect of Police in 1837, made by Chevallier and N. P. Adelon as representatives of the Paris Health Council. Reflecting painstaking research, a voluminous correspondence and a heavy reliance on hospital records, this report provided not only much useful data but also measures for the protection of workers.[27] Later, Chevallier continued to publish articles on lead-related illness in industry and to compile statistics on lead poisoning among both workers admitted to Paris hospitals and those stricken in factories.[28]

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[25] *Ibid* , 305-306

[26] "Recherches sur les causes de la maladie, dite *colique de plomb*, chez les ouvriers qui préparent la céruse," *Annales*, 1836, *15:* 5-67.

[27] "Rapport adressé à M le Préfet de Police sur les maladies que contractent les ouvriers qui travaillent dans les fabriques de céruse," *Annales*, 1838, *19* 5-38.

[28] See, for example, "Notes statistiques sur les ouvriers atteints de la colique de plomb, traités dans les hôpitaux de Paris en 1840," *Annales*, 1841, *26*· 451-453; "Note statistique sur le nombre des ouvriers atteints de coliques métalliques reçus dans les hôpitaux de Paris pendant les années 1844, 1845, 1846," *Annales*, 1847, *37*· 469-471; "Sur l'emploi du carbonate de plomb dans la préparation des dentelles dites de Bruxelles . ," *ibid* , 111-120; "Observations sur les ouvriers," *Annales*, 1847, *38* 452-456; "De la fabrication du blanc de céruse," *Annales*, 1852, *47* 314-324; "Note sur les accidents saturnins observés chez les ouvriers qui travaillent à l'émaillage des crochets de fer destinés à supporter les fils télégraphiques," *Annales*, 1861, 2nd ser , *15*. 70-93.

It is interesting to note that by the middle of the nineteenth century, a number of safety measures had been introduced in France for the manufacture of white lead and its use in the paint industry. This stemmed at least in part from the competition with the new zinc oxide industry, which did not present the toxicity problems of the former. Chevallier pointed out:

> The improvements made in the last few years in the large-scale manufacture of zinc oxide, and the more or less successful substitution of this oxide in everyday and even in artistic painting, have stimulated the zeal of manufacturers of white lead, and have prompted them to redouble their efforts to improve their products and to make their preparation less dangerous for workers.[29]

Industries utilizing copper, its alloys and compounds, also attracted the interest of Chevallier. Two studies in particular are worth mentioning. The first, published in 1847, was an essay on the toxicity of copper arsenite, especially when combined with copper acetate in the manufacture of wall paper, a combination known as "vert de Schweinfurt."[30] The second was a lengthy investigation carried on with Boys de Loury, physician to the Saint-Lazare prison, dealing with the occupational health of persons working with copper and its alloys. Published in 1850, this joint research was accorded an honorable mention by the Academy of Sciences.[31] Chevallier's other papers covered a diversity of manufacturing trades such as: Iodine and bromine, mercury fulminate, quinine sulfate, phosphorous, potassium dichromate, and coal tar products.[32]

Seventeen years after his critique of 1835, Chevallier glumly remarked:

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[29] "De la fabrication du blanc de céruse," *Annales*, 1852, *47* 314 See also, E Bouchut, "Mémoire sur l'industrie et l'hygiène de la peinture au blanc de zinc," *ibid.*, 5-55, and J B. A Chevallier, "Extrait du rapport fait par M. Chevallier, à l'Académie nationale de médecine sur l'industrie et l'hygiène de la peinture au blanc de zinc," *ibid.*, 55-68.

[30] "Essai sur les maladies qui atteignent les ouvriers qui préparent le vert arsenical . ," *Annales*, 1847, *38* 56-78.

[31] "Mémoire sur les ouvriers qui travaillent le cuivre et ses alliages," *Annales*, 1850, *43* 337-373, and 1850, *44* 27-48.

[32] See, for example, "Note sur les influences de l'iode et du brome sur les ouvriers qui préparent ces substances," *Annales*, 1842, *27* 313-317; "Sur la santé des ouvriers qui manipulent le fulminate de mercure . . ," *Annales*, 1844, *32* 332-325, "Essai sur la santé des ouvriers qui s'occupent de la préparation de sulfate de quinine," *Annales*, 1852, *48* 5-33; "Sur la substitution du phosphore amorphe au phosphore ordinaire . . et soustraire à la nécrose les ouvriers qui fabriquent les alumettes chimiques," *Annales*, 2nd ser , 1855, *3:* 124-171, with Bécourt, "Mémoire sur les accidents qui atteignent les ouvriers qui travaillent le bichromate de potasse," *Annales*, 2nd ser , 1863, *20* 83-95, "De la benzine, de la nitrobenzine et de l'aniline, inconvénients qu'elles présentent dans la fabrication et pour la santé des ouvriers . ," *Annales*, 2nd ser , 1865, *24.* 374-387, "De la fuchsine . . des accidents qui peuvent en résulter relativement aux ouvriers ," *Annales*, 2nd ser., 1866, *25* 12-37

> The study of diseases that attack workers is not always easy: often observers lack information; often, too, they are refused clarification in their research and strangely, this refusal is often due to the worker whose health is being investigated. We can state here that the researches which lead to the truth are difficult and laborious. . . .[33]

## *Consumer Health Protection*

No aspect of public health was more assiduously pursued by Chevallier than that of consumer health protection. Many of his publications dealt with adulteration and fraudulent practices in the sale of food, drugs and commercial products, and it was here that he achieved preeminence, especially after the publication by mid-century of his authoritative *Dictionnaire des altérations et falsifications*.

Not content with simply writing about methods of detecting substandard or deleterious products, Chevallier also waged an active campaign to enact more stringent food and drug laws. In 1848 he petitioned the National Assembly on the need for stricter legislation, pointing out that he had been sending similar appeals to the Chamber of Deputies since 1833, but to no avail. Adulteration and fraud, he observed, were widespread and growing. This state of affairs affected all citizens, but especially the poor. Chevallier documented his petition with specific cases of fraudulent practices, citing as examples many basic foods, drugs and commercial products.[34] Three years later, in 1851, the National Assembly finally responded to the appeals of Chevallier and his supporters by passing needed legislation. The text of the new law providing severer penalties for infractions was prominently featured in Chevallier's *Dictionnaire* with the comment: "here is the text of the law . . . whose urgency we have not ceased to proclaim for more than twelve years, in a number of petitions. . . ."[35]

Many of Chevallier's writings on consumer health protection were concerned with gross adulteration and fraud peculiar to the nineteenth century and rarely encountered today in advanced industrial countries. Thus, in his petition to the National Assembly in 1848, he wrote: "Food, beverages, condiments destined to sustain the life of men and to support their daily existence, are objects of frauds that can, in a great number of circumstances, be harmful to those who use them. . . ." Drugs, he declared, "are the object of a shameful traffic." As examples of the former, he cited the adulteration of flour, bread, milk, beer, table salt, sugar, chocolate, honey, coffee, wine, vinegar and other foods. Among the latter, he mentioned opium, cinchona bark, laudanum and quinine sulfate.[36]

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[33] "Sur l'hygiène des ouvriers en général, et sur celle des cérusiers en particulier," *Annales*, 1852, *48:* 333.

[34] "Pétition sur les falsifications, adressée à l'Assemblée Nationale," *J. de chim. méd* , 3rd ser., 1848, *4*. 510-524 See also, "Falsification des alimens Pétition adressée à la Chambre des députés à ce sujet," *Annales*, 1834, *12.* 170-179; "Sur les altérations et les falsifications des substances alimentaires A messieurs les membres de la Chambre des pairs, à messieurs les membres de la Chambre des députés," *Annales*, 1844, *32*. 452-464

[35] "Loi tendant à la répression plus efficace de certaines fraudes dans la vente des marchandises (des 10, 19 et 27 mars 1851)," in *Dictionnaire des altérations et falsifications des substances alimentaires, médicamenteuses et commerciales*, 3rd ed (Paris Béchet jeune, 1857-58), II, pp 689-691.

Chevallier also studied and exposed the rampant fraud in the sale of leeches. Not only was there a question of quality, but there was the additional problem of artificial engorgement practiced by unscrupulous dealers to increase their weight and hence their price. The commerce in leeches was still considerable in France by mid-nineteenth century, although their importation had dropped from a peak of over 57 million leeches in 1832 to about 15 million in 1844.[37]

But Chevallier's interest in consumer protection was much wider, as evidenced by his publications on the dangers of toxic color additives in food, candy and cosmetics.[38] He also warned against the risks to children's health of toys colored with poisonous pigments, and he urged legislation to outlaw their sale.[39]

## *Legal Toxicology*

The rapid development of chemistry in France during the first decades of the nineteenth century was a crucial factor in the growth of legal toxicology as an important branch of forensic medicine.[40] Chevallier's early treatise on chemical reagents, published jointly in 1822 with the industrial chemist,

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[36] "Pétition sur les falsifications . ." pp 511-522

[37] "Note sur le commerce des sangsues, et sur les fraudes nuisibles pratiquées dans la vente de ces qu'elle présente," *Annales*, 2nd ser., 1867, *27*· 344-354, "Empoisonnements causés par divers produits

[38] See (with F. Habert), "Sur la nécessité d'indiquer légalement . . . les matières colorantes . ," *Annales*, 1842, *28*· 55-72, "De la vente des sucreries coloriées et de diverses autres substances et des dangers qu'elle présente," *Annales*, 2nd ser., 1867, *27* 344-354; "Empoisonnements causés par divers produits alimentaires colorés par l'aniline," *Annales*, 1874, 2nd ser., *41* 371-379: "Des dangers qui peuvent résulter de la coloration des substances alimentaires par les couleurs d'aniline," *J d'hygiène*, 1876, *1* 279-281, and 291-292; (with E. A. Duchesne), "Des dangers que présente l'emploi des papiers colorés avec des substances toxiques," *Annales*, 2nd ser , 1854, *2* 66-82; "Note sur les cosmétiques . . ," *Annales*, 2nd ser , 1860, *13* 89-123.

[39] "De la fabrication des jouets d'enfants colorés par des préparations toxiques . ," *Annales*, 1863, 2nd ser., *19* 304-311, "Note sur des cas d'empoisonnements dus aux matières colorantes toxiques des jouets d'enfants," *Annales*, 2nd ser., 1874, *41* 92-101

[40] Erwin H Ackerknecht, "Legal medicine becomes a modern science (19th Century)," *Ciba Symposia*, 1950, *11* 1299-1304; Giovanni Bass, *Die Gerichtsmedizin als Spezialfach in Paris von 1800 bis 1850*, Zürcher Medizingeschichtliche Abhandlungen, neue Reihe nr 22 (Zürich· Juris-Verlag, 1964), pp 20-25.

Anselme Payen, undoubtedly helped to establish him in this field.[41] He soon acquired a wide reputation as an expert toxicologist and was frequently consulted by the courts. Théophile Gallard has left a vivid description of him at work:

> . . . no one ever brought more care, more precision to the work with which he was charged. It was rare that he proceeded alone, because it was the custom then, unfortunately since lost, to assign several experts at a time to study the same case, and it was with Barruel, Bayard, Flandin, Lassaigne, Lesueur, Ollivier (d'Angers) or with Orfila that he most often met. One had to see him in his small laboratory on the Quai Saint-Michel at work with one or the other of these eminent legists . . . to understand the importance and gravity of their medico-legal expertise in order to remain forever impressed by the responsibilities involved. The seals on the evidence to be examined were broken only before all the gathered experts; the description of the material was immediately entered in a special register; then, although they might feel sure about the purity of the reagents, they never hesitated to test them again before each analysis, particularly if it involved an important criminal case; then, to avoid the possibility of any confusion, all other work ceased in the laboratory. . . .[42]

A striking feature of this procedure was the team approach; no final report was issued without unanimous agreement on the findings by all the participating experts.[43] And, as Gallard pointed out, most of Chevallier's reports were published jointly with other colleagues. Thus, in cases of arsenic poisoning, he collaborated on different occasions with Bayard, Lassaigne, Lesueur, Mialhe and Tardieu.[44] In criminal proceedings involving poisoning with copper compounds, he had as co-worker Lassaigne.[45] For the investigation of deaths due to sulfuric acid, he had Ollivier (d'Angers) as collaborator, and for gas asphyxiation, Tardieu and Legrand du Saulle.[46] In addition to those mentioned, Chevallier presented expert testimony at one time or another with Boys de Loury, Barruel, Orfila, Marc, E. O. Henry, Ollivier (d'Angers), Devergie, and Personne. It is safe to say that Chevallier had worked with most, if not almost all, of the eminent French medico-legal authorities of his day.[47]

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[41] *Traité élémentaire des réactifs, leurs préparations, leurs emplois spéciaux et leur application à l'analyse* . (Paris: Bachelier, 1822); 2nd ed 1825, 3rd ed. 1829-1830, reprinted in 1841 with a special supplement by Chevallier.

[42] Gallard, *op cit* (n. 7 above), pp 184-185

[43] Gallard writes. "On arrivait ainsi à avoir des oeuvres véritablement collectives et homogènes que chacun pouvait défendre en en acceptant l'entière responsabilité. . ," *ibid.*, p 185.

[44] See for example (with Bayard), "Suspicion d'empoisonnement par l'arsenic," *Annales*, 1846, *35*. 149-157; (with Bayard), "Recherches médico-légales sur l'absorbtion de l'arsenic," *Annales*. 1846, *36* 131-160, (with Bayard), "Empoisonnement par l'arsenic," *Annales*, 1848, *39* 419-432, (with Lassaigne and Lesueur), "Suspicion d'empoisonnement d'un enfant," *Annales*, 1849, *41* 180-198, (with Tardieu), "Empoisonnement par l'arsenic," *Annales*, 1851, *45* 131-144; (with Mialhe and Tardieu), "Empoisonnement par l'arsenic," *Annales*, 1852, *47* 446-455.

[45] See for example, (with Lassaigne), "Empoisonnement par le cuivre," *Annales*, 1848, *39* 408-419, (with Lassaigne), "Empoisonnement par le cuivre," *Annales*, 2nd ser., 1856, *5* 444-452.

[46] (With Ollivier d'Angers), "Sur un cas d'empoisonnement par l'acide sulfurique," *Annales*, 1845, *33* 179-187, (with Tardieu and Legrand du Saulle) "Double asphyxie attribuée au gaz d'éclairage," *Annales*, 2nd ser , 1870, *33*. 60-65.

Chevallier was among the first French toxicologists to employ the Marsh test for arsenic early in 1838 and he published an authoritative manual on the subject in 1843.[48] He also wrote a treatise on legal toxicology which appeared in 1868.[49] That same year, he became a founding member of the Société de médecine légale, and in 1869 was elected one of the vice presidents of the society.[50] For a long time, Chevallier had suggested the establishment of laboratory courses in legal toxicology or, more specifically, a school of toxicology and legal medicine that would confer a diploma after rigorous practical and theoretical examinations.[51] It is interesting to note that his last article to appear in the *Annales d'hygiène* in 1879 reiterated this idea.[52]

## *Other Interests*

Some of Chevallier's publications displayed a concern for the hardships of the working class and the petite bourgeoisie, whose struggle for survival impressed him deeply. Many of his duties in connection with the Paris Health Council had revealed to him the grimmer aspects of life in the French capital. Thus, in a moving communication written in 1847 to the *Annales d'hygiène,* he emphasized the need to have waiting rooms for visitors to Paris hospitals, citing the example of working mothers whom he had seen drenched in a torrential rainstorm, huddled on the Rue de Sèvres, waiting for the doors of the Hôpital des Enfants Malades to open.[53] In 1849 he published a sixteen-page brochure addressed to the members of the Legislative Assembly, urging a number of reforms to benefit workers such as: a national policy to alleviate alcoholism, tax-supported retirement homes, free employment offices, an agricultural public works program, and several other suggestions, including low-cost housing.[54] Chevallier became increasingly preoccupied with the plight of the low-income inhabitants of Paris who were often left homeless during the demolition and rebuilding of large parts of the city by Haussmann.[55] The increased fares on railroads and their unsatisfactory sanitary and security facilities, particularly as they affected the poor, were criticized by Chevallier. He was especially incensed about the open and unheated third-class cars, whose unfortunate passengers had to travel at the mercy of inclement weather.[56]

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[47] See for example (with Boys de Loury), "Rapports sur un cas d'empoisonnement et d'assassinat," *Annales*, 1833, *9* 337-379; (with Barruel), "Taches de sang," *Annales*, 1833, *10*· 160-167, (with Orfila, Marc, Barruel and Boys de Loury), "Affaire d'assassinat. Exhumation des débris d'un cadavre après 11 années," *Annales*, 1834, *11* 117-178, (with Orfila and Barruel), "Taches de sang. Rapport médico-légal," *Annales*, 1835, *14*. 349–370, (with Barruel and O. Henry), "Affaire Gilbert et Rodolphe . ," *Annales*, 1840, *23*. 387-409; (with Orfila and Ollivier d'Angers), "Triple accusation d'empoisonnement . ," *Annales*, 1842, *28*· 107-168; (with Lassaigne and Boys de Loury), "Sur une double asphyxie . . ," *Annales*, 1848, *40* 190-223; (with Devergie), "Cas d'avortement suivi de mort," *Annales*, 1852, *48* 397-415; (with Boys de Loury and Personne), "Homicide par suspension et empoisonnement," *Annales*, 2nd ser., 1873, *40* 113-137

[48] A Chevallier, "Emploi de l'appareil de Marsh dans des cas de médecine légale," *J. chim méd* , 2nd ser., 1839, *5*· 36-38; (with Jules Barse), *Manuel pratique de l'appareil de Marsh* . (Paris: Labé, 1843).

[49] *Traité de toxicologie et de chimie judiciaire* (Paris: P. Asselin, 1868).

[50] *Annales*, 2nd ser , 1868, *30* 189, Gallard, *op cit* (n. 7 above), p. 185, *Annales*, 2nd ser , 1869, *32* 435

[51] *Traité de toxicologie et de chimie judiciaire*, p 14.

[52] "Nécessité de créer des cours pratiques de chimie médico-légale pour former des experts près les tribunaux," *Annales*, 3rd ser , 1879, *2*· 385-391

As an expert involved in judicial proceedings, Chevallier dealt often with police magistrates. Because of financial reasons and higher bureaucratic ineptness, these officials working directly under the Prefect of Police were frequently situated in the most unlikely places: locations that could be reached only with difficulty, sometimes by way of alleys; on the top floor of tenement buildings; in stores; and sometimes in suspicious-looking houses ("des maisons d'apparence douteuse"). Chevallier had dealings with one police magistrate whose office, located at the rear of a courtyard, could barely accommodate three people. These conditions prompted Chevallier to write an essay in 1855 advocating special buildings that would house not only the police magistrates and their staffs, but would be large enough to include a fire-fighting company and a first-aid station.[57]

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Dr. Adrien Proust, father of Marcel Proust, speaking on behalf of the Academy of Medicine at the grave of Chevallier in 1879, admitted that France lagged behind some foreign countries in public health, but stressed that this was not the fault of "those who, like Chevallier, made these questions the dominating preoccupation of their lives."[58]

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[53] "De la nécessité d'établir dans les hôpitaux de Paris une salle d'attente pour les personnes qui viennent visiter les malades," *Annales*, 1847, *37* 218-219.

[54] *A MM. les représentants à l'Assemblée législative Propositions relatives à la moralisation des ouvriers, à leur bien-être, etc .* (Paris. V. Penaud, 1849).

[55] "De la nécessité de bâtir des maisons pour loger les classes moyennes (les ouvriers). . . ," *Annales*, 1857, 2nd ser., *8*· 100-112.

[56] "Dangers que courent les voyageurs en chemin de fer .," *Annales*, 2nd ser., 1861, *15* 228-230.

[57] "De la nécessité sous le rapport de l'hygiène publique et de la salubrité de construire à Paris des établissements spéciaux destines aux commissariats de police," *Annales* 2nd ser., 1855, *3* 302-306.

Although Chevallier had left the practice of pharmacy in 1835, he continued to identify closely with the profession, both through his long teaching career at the School of Pharmacy and by his staunch defense of pharmacy in the pages of the *Journal de chimie médicale*. Many of his students were strongly influenced by the training they received in his private laboratory.

Long after Chevallier's death, a prominent French provincial pharmacist complained that fate had dealt capriciously with pharmacy: "Saturn endowed Janus with two faces. I do not know what god—or what demon—has judged us worthy to possess three: we are scientists, we are artisans, and we are also merchants." Like many other eminent French pharmacists of the nineteenth century, it was the face of the scientist and the savant that Chevallier presented to the world.[59]

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[58] *Bull Acad. de méd.*, 2nd ser , 1879, *8*· 1222. That France had by this time lost its leading and innovative position in public health achieved during the Napoleonic era has been noted by Dora Weiner in "Public Health under Napoleon . ," p 279; see ref. 5 above.

[59] George Lepine, "La profession pharmaceutique, sa grandeur et ses misères," *Bull des travaux de la Soc de pharm. de Bordeaux*, 1934, *72*· 260.

# THE SIGNIFICANCE OF THE NAME APULEIUS TO THE *HERBARIUM APULEI*

LINDA EHRSAM VOIGTS

An indication of the enormous popularity of the *Herbarium Apulei* in the early Middle Ages is the number of manuscripts which survive.[1] Howald and Sigerist discuss some forty-seven codices and fragments,[2] Beccaria lists twenty-five from the period before the twelfth century alone,[3] and four manuscripts survive of the Old English translation, one of the first instances in medieval Christendom of a vernacular herbal.[4] This widely used book of

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[1] I am grateful for the assistance provided me in the preparation of this article by the staffs of the Department of MSS. of the British Library, Reference Division, the Photographic Collection of the Warburg Institute, the Linda Hall Library of Science and Technology, and the Clendening History of Medicine Library at the University of Kansas.

[2] These manuscripts, most of which Howald and Sigerist examined for their remarkable edition, are discussed in Sec. 1, "De Codicibus," of *Antonii Musae de Herba Vettonica Liber, Pseudo-Apulei Herbarius, Anonymi de Taxone Liber, Sexti Placiti Liber Medicinae ex Animalibus, etc.*, ed. Ernst Howald and Henry E. Sigerist, Corpus Medicorum Latinorum, Vol. 4 (Leipzig: Teubner, 1927), v-xiv. The material in the "Praefatio" to this edition should be supplemented by three subsequent articles by Sigerist: "Materia Medica in the Middle Ages," *Bull. Hist. Med.*, 1939, *7:* 417-23; "The Medical Literature of the Early Middle Ages," *Bull Hist Med.*, 1934, *2:* 26-52; and "Zum Herbarius Pseudo-Apuleius," *Sudhoffs Arch f. Gesch. d Med.*, 1930, *23:* 197-204; and by *The Herbal of Pseudo-Apuleius from the Ninth Century MS. in the Abbey of Monte Cassino—Codex Casinensis 97—Together with the first printed Edition of Joh. Phil. de Lignamine—Editio princeps Romae 1481—both in Facsimile*, ed. F.W T. Hunger (Leyden: Brill, 1935).

[3] Augusto Beccaria, *I codici di medicina del periodo presalernitano* (Rome: Edizioni di Storia e Letteratura, 1956). For Latin MSS. after the twelfth century, see Lynn Thorndike and Pearl Kibre, *A Catalogue of Incipits of Mediaeval Scientific Writings in Latin*, rev ed (Cambridge, Mass: Mediaeval Academy of America, 1963). Hunger observes (*Herbal of Pseudo-Apuleius*, p. xix) that most European libraries hold one or more MSS. of this widely used book of remedies.

[4] The four surviving OE MSS. are Bodley Hatton 76, and three B.L. MSS.: Cotton Vitellius C. iii (Illustrated); Harley 585; and Harley 6258b. The last named MS. has previously been considered EME (see "Das *Herbarium Apulei* nach einer frühmittelenglischen Fassung," ed. H. Berberich, *Anglistische Forschungen*, 5 [1902], and "Medicina de Quadrupedibus: an early ME Version," ed. J Delcourt, *Anglistische Forschungen*, 40 [1914]), but H.J. DeVriend (*The Old English Medicina de Quadrupedibus*, Diss. Groningen 1972 [Tilburg: H. Gianotten, 1972], has argued, convincingly, I think, that it is a copy of the OE text. The only edition of the complete OE text is to be found in volume I of *Leechdoms, Wortcunning and Starcraft of Early England*, ed. Thomas O. Cockayne, Rolls Series, Vol. 35 (London, 1864-66). This work was re-issued (London: Holland Press, 1961) with a new Introduction by Charles Singer, but the reprint cannot replace the Rolls Series volume since it omits both the errata page and the Introduction by Cockayne that contains the indispensable keys to the apparatus of the text. The *Herbarium Apulei* portion of the enlarged herbal was re-edited by A.J.G. Hilbelink (*Cotton MS Vitellius C III of the Herbarium Apuleii*, Diss Amsterdam [Amsterdam: Swets and Zeitlinger, 1930]), and, as mentioned above, H.J. DeVriend has recently prepared a new edition of the Sextus Placitus portion of the OE text.

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simples was usually transmitted in its enlarged form; that is to say, the 130 core chapters of the Pseudo-Apuleius herbal which deal with the cures to be obtained from particular plants[5] are usually supplemented with three shorter treatises. In most codices, the Pseudo-Apuleius material is preceded by a treatise on betony, *De Herba Vettonica,* falsely ascribed to Antonius Musa, physician to Augustus; and the core herbal is frequently followed by an anonymous treatise on the medical uses of the badger, *De Taxone,* and a work, *Medicina ex Animalibus,* ascribed to a certain Sextus Placitus.[6] Some manuscripts also contain, between the *Herbarium* and *De Taxone* sections of the enlarged text, a number of illustrated chapters on simples deriving, in part at least, from Dioscorides.[7]

Although this popular remedy book draws on Pliny, it may utilize other sources, and some of them may have been Greek. Nevertheless, the text of the *Herbarium Apulei* was almost certainly composed in Latin.[8] To be sure, the Greek tradition of plant illustration lay behind the illuminations, but the text is a different matter; recensions of antique illustrations seem to be largely independent of textual traditions.[9] This herbal was written in the fourth century,[10] and it may have been written in North Africa.[11] We have no inkling as to the identity of the author, but we can be certain that it could not have been the man whose name the herbal bears, Apuleius of Madaura (A.D. 125-?), self-named Platonicus, philosopher, sophist, and novelist. The unknown herbalist is now referred to as Pseudo-Apuleius Platonicus or as Apuleius Barbarus.

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[5] Howald and Sigerist, *op. cit.* (n. 2 above), treat ch. 131, on the mandragora, as a likely interpolation. See p. xviii.

[6] *Ibid.* p. xvii.

[7] These chapters, which may deal with as many as 71 medicinal herbs, are usually referred to as the *Liber Dioscoridis de Herbis Femininis*. The work was edited from three MSS, by H.F. Kästner in *Hermes,* 1896, *31*· prescriptions always use the Latin plant names, because the Greek plant names in chapter headings are see Sigerist, "Materia Medica," pp. 419-20, and "Medical Literature," p. 36. For other MSS., see Beccaria as well as Thorndike and Kibre.

[8] The question of whether the work was originally written in Greek and translated into Latin has been one of some controversy. The primary proponent of original Greek authorship was Charles Singer, "The Herbal in Antiquity," *J. Hellenic Studies,* 1927, *47*· 32. He argued for a Greek origin on the basis of a papyrus fragment of a Greek illustrated herbal of ca. A.D. 400 which contains two illustrations resembling plant figures in the *Herbarium Apulei*. Singer's view is not widely held. The most cogent rebuttal is to be found in Sigerist's "Zum Herbarius Pseudo-Apulei," pp 199-200. Sigerist acknowledges that the author of this herbal utilized other herbals in composing his in Latin and he most certainly took the drawings from extant prototypes. Nevertheless, the work can not be a translation from the Greek because the formulation of a number of the remedies comes from Pliny, because the introductory letter follows the example of Pliny, because the prescriptions always use the Latin plant names, because the Greek plant names in chapter headings are interpolated synonyms, and because there is no linguistic evidence of translation from Greek. He also (p. 204) counters Simonini's argument for a Greek original based on a *Herbarium Apulei* MS. which contains a Greek alphabet by listing a number of MSS. of undisputed Latin origin which do the same. Sigerist concludes, "Es scheint mir daher immer noch am wahrscheinlichsten, dass der Herbarius Pseudo-Apulei zwar in Anlehnung an griechische Vorbilder aber direkt in lateinischer Sprache entstanden ist." In agreement with Sigerist are Hunger, *Herbal of Pseudo-Apuleius,* pp xvii-xviii, Charles Talbot, *Medicine in Medieval England* (London: Oldbourne History of Science Library, 1967), p. 15, and Claus Nissen, *Krauterbücher aus funf Jahrhunderten* (Munich: Wölfle, 1956), p 15.

[9] The difference between textual and illustrative sources, as in the case of this herbal, poses problems in dealing with MS. traditions. The exchangeability and migration of illustrations, particularly in medical MSS.,

Apuleius Platonicus is the name which appears in the incipits and/or the ornamental titles of most of the *Herbarium Apulei* manuscripts. It appears sometimes in variant forms such as Apuleius Platonis, Platonis Apoliensis, Apuleius Plato, Apoliensis Plato, and Apuleius Apoliensis. In other instances, the designation Plato is used for Apuleius. In the eleventh-century illustrated manuscript of the Old English translation of the *Herbarium*, B. L. Cotton Vitellius C. iii, we find on f. 19ᵛ a title in a circular ornament; the title reads, "HERBARIVM APVLEI PLATONICI QVOD ACCEPIT AB ESCOLAPIO ET ALCHIRONE CENTAVRO MAGIS[T]RO ACHILLIS."[12] On the recto of that same leaf is to be found a full-page illustration, an author-portrait depicting three figures who are designated in uncials below the miniature as "ESCOLAPIVS. PLATO. CENTAVRVS"[13] This conflation of Apuleius Platonicus and Plato, or at least this use of the name Plato to designate the putative author Apuleius, is not unique to Cotton Vitellius C. iii; it occurs in a thirteenth-century manuscript of the *Herbarium Apulei*, Vienna Nationalbibliothek 93.[14] Granted, there are occasional references to Plato as a physician in

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seems to be a constant of classical rolls and codices. For a thorough discussion, see Erich Bethe, *Buch und Bild im Altertum* (Leipzig: Harrassowitz, 1945); and two works by Kurt Weitzmann, *Illustrations in Roll and Codex*, Studies in Manuscript Illumination, No 2 (Princeton: Princeton Univ. Press, 1947), and *Ancient Book Illumination*, Martin Classical Lectures, Vol. 16 (Cambridge, Mass.: Harvard Univ. Press, 1959).

[10] Howald and Sigerist, *op. cit.* (n. 2 above), p. xx.

[11] Hunger (*Herbal of Pseudo-Apuleius*, p. xviii) suggests that the nature of the author's Latin, the use of some particular plant names, and the portrayal of two identifiable reptiles in the herbal may suggest that the unknown author wrote in North Africa

[12] This colorful ornamental title has been reproduced in black and white by Hunger, *ibid* , p 5 See the plates and my discussion of both the title ornament and the author portrait in "An Anglo-Saxon View of the Classical Gods," *Studies in Iconography*, 1977, *3.* 3-16.

[13] Plates of this illustration can be found in Hunger, *ibid.*, p. 2; in David Talbot Rice, *English Art. 871-1100*, Oxford History of English Art, Vol. 2 (Oxford: Clarendon, 1952), Pl. 88; in Charles Singer, "Greek Biology and its Relation to the Rise of Modern Biology," *Studies in the History and Method of Science, II*, ed. Charles Singer (Oxford: Clarendon, 1921), Pl. 4; in Fritz Saxl and Hans Meier, *Manuscripts in English Libraries*, ed. H. Bober, Verzeichnis astrologischer und mythologischer Handschriften des lateinischen Mittelalters, Vol. III (London. Warburg Institute, 1953), Tafel I, Abb. 1; and in Fritz Saxl and Rudolf Wittkower, *British Art and the Mediterranean* (London: Oxford Univ. Press, 1948), Sec. 31, Pl. 1.

[14] See Georg Swarzenski, "Mittelalterliche Kopien einer antiken medizinischen Bilderhandschrift," *Jahrbuch des kaiserlich deutschen archaologischen Instituts*, 1902, *17:* 50-52, and Hermann J. Hermann, *Die fruhmittelalterlichen Handschriften des Abendlands* (Leipzig: K W. Hiersemann, 1932), I, 13. This MS. is now available in facsimile: *Medicina Antiqua Libri Quattuor Medicinae. Codex Vindobonensis 93 der Oesterreichischen Nationalbibliothek*, Codices Selecti, Vol. 27 (Graz: Akademische Druck, 1972)

medieval Western and Islamic writing,[15] but in the case of the *Herbarium Apulei* manuscripts, both the name Apuleius and the name Plato certainly refer to the single author to whom the herbal is wrongly attributed.

Beyond the Plato/Apuleius confusion, and indeed more intriguing and more vexing than it, is the basic question of the assignment of the name of the flamboyant second-century orator and writer to a fourth-century remedy book. There is no question why a herbalist would borrow the name Dioscorides or Antonius Musa to lend authority to his work, and it is not surprising to find Apollo given as the author of a medical treatise,[16] but the medical authority of Apuleius Platonicus is not so transparent, and the assignment of this name to the herbal deserves further consideration than it has hitherto received. It behooves us to consider what we know about the real Apuleius and his reputation, to consider those speculations that have attempted to explain the attribution, and to consider a possible explanation that yet remains unexplored.

Apuleius, sometimes called Lucius Apuleius, although not on good evidence, was born ca. A.D. 125 at Madaura in Numidia, the son of a Roman provincial official.[17] He studied at Carthage and in Athens, traveled widely, and practiced rhetoric in Rome. Returning to Africa, he fell ill in Oea and was nursed to health by a wealthy widow, the mother of one of his friends from days of former study in Athens. Apuleius married this woman, many years his senior, and was consequently charged by her relatives before the proconsul with having practiced magic to gain the widow's affections. His *Apologia* is the defense he made against the charges, a defense which presumably resulted in acquittal. It is from the *Apologia* that we know of his life up to that point. Our information on his later life is less certain, but we know that he moved to Carthage, where "his brilliant discourses, his amazing fluency in both Greek and Latin, his scintillation in those extempore displays which were part of the Sophists' art won him so great a reputation that statues were erected to him and he was made priest of Aesculapius, an office of great dignity and prestige, during the holding of which he gave gladiatorial games to the people."[18] We do not know when he died.

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[15] The *Disputatio Platonis et Aristotelis de Anima Hominis* was sometimes included in medical treatises (see Beccaria, Nos. 6, 21, 34, 81, 133, 137), and Jerry Stannard ("Greco-Roman Materia Medica In Medieval Germany," *Bull Hist Med.*, 1972, *46:* 467) offers two instances where Plato seems to be cited as a medical authority In the Islamic tradition there is more clear-cut evidence that Plato was viewed as a legendary physician; see Franz Rosenthal, *The Classical Heritage in Islam*, trans. E. and J. Marmorstein (Berkeley: Univ of California Press, 1975), p. 33, and Moritz Steinschneider, *Die Arabischen Uebersetzungen aus dem Griechischen* (1886-96; rpt. Graz. Akademische Druck, 1960), p. 477 (371)

[16] Note, for example, the treatise "Apollo, Epistola de Emplastris contra Podagram," Vienna 93, f 159$^{rv}$.

[17] For information on Apuleius, see L. v. Schwabe "(9) Appuleius," *Paulys Real-Encyclopadie der classischen Altertumswissenschaft*, ed. Georg Wissowa et al , II (Stuttgart: Metzlerscher, 1896), col. 246-58; Elizabeth Hazelton Haight, *Apuleius and His Influence* Our Debt to Greece and Rome, Vol. 52 (New York· Longmans, 1927); and, for recent speculation on Apuleius' later life in Carthage, Patrick G. Walsh, *The Roman Novel* (Cambridge. Cambridge Univ. Press, 1970).

A number of works, spurious and authentic, have traditionally been considered the writing of the Madauran. The best known of his authentic writings are the *Apologia,* mentioned above, the *Florida,* a collection of excerpts, largely from his declamations at Carthage, and the *Metamorphoses,* often referred to as the *Asinus Aureus* (*The Golden Ass* or *The Splendid Ass*), a series of Milesian tales incorporated into the story of the transformation of a young man, Lucius, into an ass.[19] The *Metamorphoses,* justly famous for its Cupid and Psyche passage, has long been held to be the earliest of the extant works of Apuleius, but the possibility of its composition dating from the later career of the writer has been recently argued.[20] These three works are all contained in an eleventh-century manuscript of Beneventan provenance.[21] Additional authentic works, philosophical in nature, include *De Mundo; De Deo Socratis,* a treatise on demons as intermediaries between gods and men; and two works on Plato, *De Platone et Eius Dogmate.*[22] *Peri Hermeneias* is a work on logic probably ascribed falsely to Apuleius,[23] and it was widely held that he was responsible for the translation from Greek to Latin of *Asclepius,* a lengthy dialogue between Hermes Trismegistos, grandson of Hermes, and Asclepius, grandson of the god of medicine.[24] In the Middle Ages he was erroneously held to be the author of two works on grammar, of *The Sphere of Life and Death*, and, of course, of the *Herbarium* with which we are concerned.[25]

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[18] Haight, *Apuleius and His Influence,* p 35.

[19] For editions of these works, see the three volumes in the Teubner series edited by Rudolf Helm, *Apulei Platonici Madaurensis Opera Quae Supersunt:* Vol. I, *Metamorphosen Libri XI,* 3rd ed. (Leipzig, 1955); Vol. II, fasc. 1, *Pro se de Magia Liber (Apologia),* rev. ed. (Leipzig, 1969); Vol. II, fasc. 2, *Florida,* rev ed. (Leipzig, 1959). All citation will be from the Teubner series. See also William Abbott Oldfather, Howard Vernon Carter, and Ben Edwin Perry, *Index Apuleianus,* Am. Philological Assoc. Monographs, No 3 (Middletown, Conn.: Am. Philological Assoc., 1934)

[20] Walsh, *Roman Novel,* p. 186 et passim.

[21] This MS. is now held in Florence (Laurentian 62, 2). On the Apuleius MSS., see Schwabe, "Appuleius," col 252, Haight, *Apuleius and His Influence,* p. 91, Lynn Thorndike, *A History of Magic and Experimental Science,* I (New York: Macmillan, 1929), 241, and Thorndike and Kibre, *Catalogue of Incipits,* for some MSS.

[22] The philosophical works have been edited by Paul Thomas as Vol. III of the Teubner series *Apulei Platonici Madaurensis Opera Quae Supersunt* (Stuttgart, 1970).

[23] For a summary of argument on disputed works, see, in addition to Schwabe, Thorndike, *History* I, 222, 596.

[24] Although the translation was likely not by Apuleius, the work has been edited by Thomas in Vol. III of the Teubner series. See also Thorndike, *History,* I, 290 ff., on the Hermetic writings. A fifteenth-century addition to a fourteenth-century MS., British Library Harley 3969, attributes to Apuleius the Latin translation of a Greek text on physiognomy by Polemon. Valentin Rose considered the attribution a likely one, see *Anecdota Graeca et Graecolatina,* I (Berlin. Duemmlers, 1864), 73-86.

Just as this writer was accorded honors in Carthage in his lifetime, so his posthumous reputation continued to wax. From the latter years of Roman hegemony and the early centuries of Christianity, references to Apuleius of Madaura abound. A few of those who wrote of him are Lactantius, Ausonius, Martianus Capella, Fulgentius, Macrobius, Priscianus, Cassiodorus, and Jerome.[26] Augustine's lengthy discussions of the writings of Apuleius reflect the prestige his works enjoyed in the fourth and fifth centuries. The Bishop of Hippo, who had been educated in Madaura and Carthage himself, refers to Apuleius as a magician in *Epistles* 102, 136, and 138, but it is in *De Civitate Dei* where we find extensive references.[27] Augustine refers to *De Mundo* (Bk. IV, ch. 2), to the *Apologia* (Bk. VIII, ch. 19), to *Asclepius* (Bk. VIII, chs. 23-26), to the *Asinus Aureus* (Bk. XVIII, ch. 18), and develops (Bk. VIII, chs. 12, 14-22; Bk. IX, chs. 3, 7-8, 11, 16 et passim) an elaborate rebuttal to the Apuleian treatment of demons in *De Deo Socratis*. As one might expect, Apuleius was known to the early Middle Ages primarily as a Platonist philosopher and as a magician and was known in a vicarious manner from *De Civitate Dei* more than from first-hand knowledge of his writings; the authentic canon of Apuleius appears not to have been widely known before the twelfth century.[28]

Since it is clear that the second-century Apuleius could not have composed the *Herbarium*,[29] a number of suggestions have been offered to explain the unknown fourth-century herbalist's choice of Apuleius as a *nom de plume*. The most general explanation is the assertion that the prestige of Apuleius was so great by the fourth century that the use of his name would assure acceptance of the work and shut out criticism.[30] This explanation makes no attempt to draw a connection between the nature of the Madauran's reputation and the nature of the *Herbarium*, and it does not suggest that the name Apuleius, like that of Hippocrates or Galen, was a particularly congenial pseudonym.

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[25] See Thorndike, *History*, I, 596, and Henry E Sigerist, "The Sphere of Life and Death in Early Mediaeval Manuscripts," *Bull. Hist. Med* , 1942, *11*. 292-303.

[26] Haight, *Apuleius and His Influence*, pp 92-95.

[27] The works of Augustine are to be found in J.-P. Migne, *Patrologia Latina*, 32-47, and *De Civitate Dei* is to be found in *Corpus Christianorum*, Series Latina, 47-48 as well.

[28] Walsh, *Roman Novel*, p 230; Haight, *Apuleius and His Influence*, pp. 101-07

[29] See, among others, Howald and Sigerist, *op cit* (n. 2 above), p xix; however, as recently as 1928 we find the observation that a Latin Herbal was written in the fourth century by Lucius Apuleius (E. A. Wallis Budge, *The Divine Origin of the Craft of the Herbalist* [London. Society of Herbalists, 1928], p. 69).

[30] This explanation is to be found in Bethe, *Buch und Bild im Altertum*, p 33, and Sigerist, "The Medical Literature of the Early Middle Ages," pp. 33-34

An explanation which does attempt to deal with the suitability of the appropriation of the name is the one which links Apuleius to the herbal on the basis of his reputation as a magician.[31] We know, of course, from the *Apologia* that Apuleius was charged with sorcery during his life, and we know from the references Augustine made to him, discussed above, that much of his notoriety was linked to his fame as a magician. Further, a number of copies survive of a numerological device for divination called the *Sphere of Life and Death*; Apuleius is usually the name given for the author or translator of this work. Here the spurious attribution is clearly based on the notoriety of Apuleius as a magician.[32] It is difficult, however, to draw the same sort of connection between the name and the herbal. There is, to be sure, some reference to charms and amulets in the *Herbarium*,[33] but on the whole, the plant recipes are offered for their straightforward therapeutic value. Indeed, magic in the *Herbarium* is no more prominent than it is in Pliny, and the herbal with its limited use of divination, charms, and amulets is an altogether different sort of work from the *Sphere of Life and Death* or from the Old English *Lacnunga*.[34] The suggestion that magic is the link is not a convincing one.

Two other explanations for the Apuleius attribution have been set forth by Charles Singer and should be dealt with briefly. The first is that the *Herbarium* may have been given the name of Apuleius "because he was the author of a *Liber Floridarum*, which, however, is of a totally different character."[35] Problems are raised by this suggestion; surely, given the reputation of Apuleius in the fourth century as evinced by Augustine's discussions of his works, we could expect that a fellow countryman choosing the Madauran's name for the herbal would be aware that the *Florida* was a collection of excerpts of orations. Moreover, Singer offers no philological evidence for the likely linguistic confusion of the notions of florilegia on one hand and books dealing with plant remedies on the other. Singer's second, and equally speculative suggestion, is that the name Apuleius is "a Christian surrogate for Apollo." This suggestion seems to be based entirely on a comparison Singer draws between the author portrait in Cotton Vitellius C. iii (discussed above) and a Pompeian wall-painting depicting Apollo, Aesculapius, and Chiron.[36] There is, in actuality, little iconographic resemblance between the Roman composition and the Anglo-Saxon one,[37] but more important, Singer does not give us any linguistic or manuscript proof to justify a confusion of Apollo and Apuleius; his contention lacks the sort of evidence supplied by Swarzenski to establish the Apuleius/Plato conflation.[38] Lacking such necessary evidence, both of Singer's suggestions seem improbable.[39]

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[31] While I do not wish to put words in the mouth of Charles Talbot, he seems to imply that the importance of Apuleius to the *Herbarium* is related to magic; he says (*Medicine in Medieval England*, p. 15) that the herbal was "possibly drawn up . in North Africa, where Apuleius of Madaura enjoyed a great reputation for magic "

[32] Sigerist, "The Sphere of Life and Death in Early Mediaeval Manuscripts," 294-96.

[33] See, for example, the first use for *heraclea* (LXXIII.1) as an amulet, the fifth use for *sparagus agrestis* (LXXXV. 5) to break a spell, and the second use for *cardus silvaticus* (CX.2) as a charm. In the entire compilation of recipes in the *Herbarium Apulei*, these are exceptional

[34] On the *Lacnunga*, see Vol. III of Cockayne, *Leechdoms;* J. H. G. Grattan and Charles Singer, *Anglo-Saxon Magic and Medicine* (London· Oxford Univ. Press, 1952); and G. Storms, *Anglo-Saxon Magic* (The Hague· Nijhoff, 1948).

[35] Singer, "Greek Biology," p. 68.

Most germane of the proffered explanations for the pseudonymous attribution is the possibility that Apuleius Platonicus may have counted the practice of medicine among his many endeavors. Schwabe suggests (col. 249) that Apuleius was considered a physician because he was consulted by an epileptic (*Apologia,* 48). In regard to the attribution of the *Herbarium,* Lynn Thorndike observes only that using the names of famous authors was a common practice in the declining Roman Empire,[40] but he seems to be convinced that Apuleius studied medicine.[41] He draws this conclusion from a number of passages in the *Apologia,* a work wherein, he observes, "Apuleius shows that he really is a student, if not an authority, in medicine and natural science. The gift of the tooth-powder and the falling of the woman in a fit were incidents of his occasional practice of medicine, and he also sees no harm in his seeking certain remedies from fish. He repeats

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[36] In Grattan and Singer, *Anglo-Saxon Magic,* p. 8, fig. 19; see also p 78. Charles Talbot has recently posited an argument likewise based on the author portrait in Cotton Vitellius C. iii, it is to be found in the "Medico-Historical Introduction" to the facsimile of Vienna 93. He suggests (p. 29) that there may have been an antique herbal author portrait which depicted Chiron, Achilles, and the latter's father—Peleus, that in Roman usage Apuleius was substituted for Peleus, and that Aesculapius was substituted as well for Achilles. I would suggest that the reservations concerning Singer's Apollo-Apuleius conflation apply equally well to the Peleus-Apuleius/Achilles-Aesculapius hypothesis.

[37] For a discussion of the iconographic differences in the two compositions, see my thesis, "The Old English Herbal in Cotton MS Vitellius C. iii. Studies," Diss. Missouri 1973, pp. 89-90

[38] See above, n. 14

[39] Note too that at least one MS., Vienna 93, contains both the *Herbarium Apulei* and "Apollo, Epistola de Emplastris contra Podagram."

[40] *History,* I, 594.

[41] *The Place of Magic in the Intellectual History of Europe* Studies in History, Economics, and Public Law, Vol. 24 (New York: Columbia Univ Press, 1905), 72.

Plato's theory of disease from the *Timaeus* and cites Theophrastus' admirable work *On Epileptics*."[42]

Measuring Thorndike's inferences against the passages in the *Apologia* upon which they are based can lead, however, to conclusions different from his in regard to the Madauran's practice of medicine. A direct consideration of these passages confirms that Apuleius considered himself a philosopher interested in the natural sciences, one who follows in the tradition of Aristotle and Theophrastus (ch. 36). The poem commending the tooth-powder (ch. 6) is, to be sure, not philosophical, but it is not medical either; it commends the dentifrice more for its cosmetic worth than for its medicinal value. In his defense against the charge of casting a spell on an epileptic woman (ch. 48), he asserts that the woman was brought to him by his slave Themison, a physician, so that he might interrogate her to test the physiological implications of Plato's theory in the *Timaeus*, and he goes on to say it should be no cause for surprise that philosophers learn much of diseases and remedies in the course of their learned pursuits. Regarding his dissection of fish, again brought to him by his servant Themison, he says (ch. 36) he studied them as an ichthyologist in the philosophic tradition, and he later observes (ch. 40) that he dissected the fish in the course of writing his anatomical treatises on many kinds of animals. Not content to settle for one defense though, he acknowledges (ch. 40) that he might have looked for certain remedies in the fish but his interest was that of a philosopher wishing to benefit mankind, not that of a doctor seeking money for his purse.

Perhaps even more important to a consideration of a probable practice of medicine is the negative evidence of the *Apologia* and *Florida*. Never one for modesty, Apuleius claimed for himself expertise in many areas, but he does not call himself a physician, and he does in the *Apologia* (ch. 33, 48) refer to his servant as a physician. Furthermore, in the *Florida* (ch. 9) he itemizes the kinds of writing he has undertaken, and he does not mention medical treatises. Consequently, we can agree that Apuleius Platonicus was a student of medicine insofar as that study was incidental to his pursuits in philosophy and/or natural history. We should, however, have strong reservations about imputing to him any actual practice of medicine or any medical treatises.

That Thorndike focused on Apuleius' interest in medicine, albeit general, is important. The possibility must remain open that the fourth-century herbalist associated the Madauran's name with medicine and found it an apt pseudonym. Nevertheless, there exists another possible reason why the name Apuleius Platonicus was affixed to the *Herbarium*, and that reason seems to have been overlooked. The reason is that Apuleius was a priest of Aesculapius, the deity whose importance in late classical culture can scarcely be over-emphasized and who is significant in a number of different ways to the *Herbarium*.

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[42] *History*, I, 237

Burgeoning Christianity seems to have forced second-century Africans like Apuleius into outspoken reassertions of their personal pagan commitments,[43] and the evidence surviving in the works of Apuleius indicates that he was, as a priest, particularly devoted to Aesculapius. In considering his works, it is prudent to discount as spurious the identification of Apuleius as translator of the Hermetic dialogue *Asclepius*,[44] but it is also necessary to remember both that the fourth-century compiler of the *Herbarium* may have considered Apuleius the translator of *Asclepius* and that the association of the Madauran with this work may be an indication of a popular assumption of a connection between Apuleius and Aesculapius.

However, even if one disregards *Asclepius*, the Apuleian canon contains a number of references to Aesculapius, and some of them are significant.[45] In the *Metamorphoses*, which contains no references to the god by name and which does deal at length with the cults of Isis and Osiris, we find a reference (I. 4. 12) to the knotted wooden serpent-staff of the god of medicine.[46] In *De Deo Socratis* (XV, lines 31-36) Apuleius, in speaking of a number of localized cults of gods, asserts that Aesculapius is worshipped everywhere. Of particular importance is the passage in the *Apologia* (ch. 55) where Apuleius tells of the discourse he had delivered three years earlier on the greatness of Aesculapius and of the wide circulation in Oea the discourse had received. He calls on the audience to repeat the opening passage and avers that many voices supply the words.[47] However, the most significant statement is to be found in the *Florida* (ch. 18); in the course of praising Carthage, the Madauran speaks of the special protection afforded the city by Aesculapius and of his longstanding personal service to the god:

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[43] See Walsh, *Roman Novel*, p. 186; Peter Brown, *The World of Late Antiquity* (New York. Harcourt Brace Jovanovich, 1971); and E. R. Dodds, *Pagan and Christian in an Age of Anxiety* (Cambridge: Cambridge Univ. Press, 1965).

[44] See above, notes 23 and 24.

[45] For a complete list of references to the god in the writings of Apuleius, see Oldfather, s.v "Asclepius" and "Aesculapius." See also, for the passages from the *Florida and Apologia*, Emma J and Ludwig Edelstein, *Asclepius: A Collection and Interpretation of the Testimonies* (Baltimore: Johns Hopkins Press, 1945), I, T608, T609, and T837

[46] This reference is not cited in Oldfather.

[47] This passage bears citing: "nec hoc ad tempus compono, sed abhinc ferme triennium est, cum primis diebus quibus Oeam ueneram p[1]ublice disserens de Aesculapii maiestate eadem ista prae me tuli et quot sacra nossem percensui. ea disputatio celebratissima est, uulgo legitur, in omnibus manibus uersatur, non tam facundia mea quam mentione Aesculapii religosis Oeensibus commendata. dicite aliquis, si qui forte meminit, huius loci principium.—audisne, Maxime, multos suggerentis? immo, ecce etiam liber offertur. recitari ipsa haec iubebo, quoniam ostendis humanissimo uultu auditionem te istam non grauari" (p 63).

> Now, therefore, I will begin by speaking of the god Aesculapius. With what more auspicious theme could I engage your ears? For he honours the citadel of our own Carthage with the protection of his undoubted presence. See, I will sing to you both in Greek and Latin a hymn which I have composed to his glory and long since dedicated to him. For I am well known as a frequenter of his rites, my worship of him is no new thing, my priesthood has received the smile of his favour, and ere now I have expressed my veneration for him both in prose and verse. Even so now I will chant a hymn to his glory both in Greek and Latin.[48]

Apuleius Platonicus clearly associated himself with the god of medicine, and it may well be this association with Aesculapius which links the Madauran to the *Herbarium*. Aesculapius is important to the *Herbarium*, and his authority was significant both in the culture which spawned the herbal and in the culture which transmitted it. The authority of the god of medicine may suggest the divine origins of the herbal remedies or it may simply be viewed as the wisdom of the physician *par excellence*. This is not the place to survey these various antique notions of Aesculapius; the Edelsteins' valuable compilation is the place for that.[49] Nevertheless, it is useful to note that while there was an antique view of Aesculapius as a chthonian god,[50] there was another view of him as the great, (once) mortal physician.[51]

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H. E. Butler (*The Apologia and Florida of Apuleius of Madaura* [1909; rpt. Westport, Conn.: Greenwood, 1970], pp. 96-97) translates this passage in the following manner: "This is no invention on the spur of the moment; nearly three years since, in a public discourse on the greatness of Aesculapius delivered by me during the first days of my residence at Oea, I made the same boast and recounted the number of the mysteries I knew. That discourse was thronged, has been read far and wide, is in all men's hands, and has won the affections of the pious inhabitants of Oea not so much through any eloquence of mine as because it treats of Aesculapius. Will anyone, who chances to remember it, repeat the beginning of that particular passage in my discourse? You hear, Maximus, how many voices supply the words. I will order this same passage to be read aloud, since by the courteous expression of your face you show that you will not be displeased to hear it."

[48] Butler's translation (pp. 205-06). The text itself is as follows: "nunc quoque igitur principium mihi apud uestras auris auspicatissimum ab Aesculapio deo capiam, qui arcem nostrae Karthaginis indubitabili numine propitius [s]t[r]egit. eius dei hymnum Graeco et Latino carmine uobis etiam canam[iam] illi a me dedicatum. sum enim non ignotus illi sacricola nec recens cultor nec ingratus antistes. ac iam et prorsa et uorsa facundia ueneratus sum, ita ut etiam nunc hymnum eius utraque lingua canam, cui dialogum similiter Graecum et Latinum praetexui. . ." (p. 38).

[49] Edelstein, *Asclepius. A Collection and Interpretation of the Testimonies*, 2 vols. (see n 45 above).

[50] For this interpretation, see Károly Kerényi, *Asklepios*, trans. Ralph Mannheim (London.Thames and Hudson, 1960), and J. Schouten, *The Rod and Serpent of Asklepios*, trans. M E. Hollander (Amsterdam: Elsevier, 1967).

[51] See E. T. Withington, "The Asclepiadae and the Priests of Asclepius," *Studies in the History and Method of Science*, II, ed Charles Singer (Oxford: Clarendon, 1921), 192-205.

The importance of this second, euhemeristic view, is that it seems to have been quite strong in the latter years of Rome when the herbal was compiled, and it is the view that the Fathers of the Church transmitted to the West.[52] We find, for instance, in *De Civitate Dei,* both general euhemeristic interpretations of the gods (VII, 18) and a specific discussion of Aesculapius (VIII, 26) as a (falsely) deified mortal physician. This euhemeristic interpretation was one of the ways the pagan gods survived in the Middle Ages.[53] Aesculapius did survive, in herbals as well as in other ways; for example, this god is paid tribute in an eleventh-century hymn to St. Pantaleon attributed to a cleric learned in medicine, Fulbert, Bishop of Chartres.[54]

Given the medical authority of this tutelary god of Apuleius in the eras which produced and transmitted the *Herbarium Apulei,* we should not be surprised at the frequency with which Aesculapius is cited or depicted in the herbal. A number of surviving manuscripts of the herbal contain depictions of the god; four examples are representative. In a manuscript held in Vienna, Nationalbibliothek 93, the thirteenth-century *Herbarium,* which may be a copy of a sixth-century original,[55] contains a miniature (f. 114$^{v}$) depicting a physician accepting a herb from a centaur. The accompanying text reads ". . . que scolapius aut Chiron centaurus magister medicine . . . invenit."[56] A second instance is one of five author-portraits accompanying the tenth-century manuscript of the *Herbarium* now held at Cassel. In this portrait, the figure is seated in an aureole of three circles, the uppermost of which contains the inscription "SCOLAPIUS MEDICUS MAG[ISTER]."[57] A colorful frontispiece to a codex executed in Germany ca. 1200 depicts three men digging herbs; two of them have been identified as Aesculapius and Apuleius.[58] Finally, the eleventh-century author-portrait in the Old English translation of the *Herbarium*[59] contains a portrait resembling most classical depictions of the god.[60] The classical portrayal of physicians in medical codices[61] prevailed for centuries in the herbal illustrations of Aesculapius.

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[52] On euhemerism and Christian apologetics, see ch. i of Jean Seznec, *The Survival of the Pagan Gods,* trans. Barbara F. Sessions (1953; rpt. in Bollingen Series; Princeton: Princeton Univ. Press, 1972).

[53] Seznec (*ibid.*, p. 13) bears quoting on this point: "The euhemeristic tradition remains a living influence throughout the Middle Ages, although it undergoes a total change of character. The human origin of the gods ceases to be a weapon to be used against them, a source of rejection and contempt. Instead, it gives them a certain protection even granting them a right to survive. In the end it forms, as it were, their patent of nobility."

It should be acknowledged, of course, that pagan gods survived in other ways, as for example in allegorical interpretations, but the euhemeristic view seems to be the most important consideration in dealing with the survival of Aesculapius in the early Middle Ages.

[54] Migne, *Patrologia Latina,* 141, col. 340-41. For a discussion of the "Hymnus seu Prosa de Sancto Pantaleone," see Loren C. MacKinney, *Early Medieval Medicine with Special Reference to France and Chartres* (Baltimore: Johns Hopkins Press, 1937), p. 134. Although the hymn was written before 1050, the attribution to Fulbert is not a certain one. Admittedly, there is at least one instance of an early medieval medical treatise with emphasis on the supernatural aspects of Aesculapius as one of the classical pantheon (see Rudolf Laux, "Ars Medicinae: Ein fruhmittelalterliches Kompendium der Medizin," *Kyklos*, 1930, *3:* 418-19, but the euhemeristic tradition of the hymn to St. Pantaleon seems to be the dominant one in the early medieval period. For a similar example, consider the prosaic treatment of Aesculapius as the founder of one of the schools of medicine in the OE *Peri Didaxeon* (Cockayne, III, 83), an account quite similar to that found in IV, 3-4 of the *Etymologiae* of Isidore of Seville (ed. W. M. Lindsay [Oxford. Oxford Classical Texts, 1910])

[55] Hermann, *Die fruhmittelalterlichen Handschriften*, p. 9.

[56] Another portrait of Aesculapius occurs in this MS. of the enlarged herbal in the Pseudo-Antonius Musa treatise on vettonica. On f. 13$^{v}$ a figure is portrayed gathering the plant; the text identifies him as Aesculapius.

Textual references to Aesculapius as well as to Chiron, Apollo, and other gods, are also to be found in the *Herbarium Apulei*. Of Herba Apollinaris (XXII) we are told "Apollo hanc herbam dicitur inuenisse et Asclepio dedisse, unde nomen ei inposuit apollinaris."[62] There are references to Aesculapius in three other recipes, but they may be interpolations.[63] In two of the works usually transmitted with the core herbal, Aesculapius is cited as the warrantor of the efficacy of the remedies: the praecatio in *De Herba Vettonica* and the initial epistle in the *De Taxone* treatise. Most conspicuous and most important of the written references to the god of medicine in this book of remedies are the titles and/or incipits found in the manuscripts. Howald and Sigerist give (p. 15), with variants, the title "HERBARIVM APVLEI PLATONICI quem accepit a Cirone centauro, magistro Achillis, et ab Aesculapio." Although the first leaves are missing in the Monte Cassino manuscript of the herbal (ninth or tenth century), the extant explicit is similar: "EXPLICIT HERBARUM APULEI PLATONIS, QUEM ACCEPIT AB SCOLAPIUM ET CHIRONE CENTAURO, MAGISTRO ACCHILLI."[64] Another instance is to be found in Cotton Vitellius C. iii. Only the title in this manuscript was not translated into Old English; painted in bright colors in a circular classical ornament on f. $19^{v}$, it reads "HERBARIVM APVLEI PLATONICI QVOD ACCEPIT AB ESCOLAPIO ET ALCHIRONE CENTAVRO MAGIS[T]RO ACHILLIS." On the recto of this same leaf is the author-portrait, the visual equivalent of the title, depicting Plato/ Apuleius receiving the book from Aesculapius and Chiron who flank him.[65]

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[57] The MS is a Cassel, Landesbibliothek Phys. et Hist. Nat., fol. 10. See Adolph Goldschmidt, *The Carolingian Period* (1928; rpt. with *The Ottonian Period as German Illumination;* New York: Hacker, 1970), Pl. 19.

[58] The MS. is Eton College 204. Loren MacKinney, *Medical Illustrations in Medieval Manuscripts* (London: Wellcome Library, 1965), p. 219 and fig. 20, identifies the two older men as Aesculapius and Apuleius.

[59] Regarding this illumination (Cotton Vitellius C. iii, f. 19), see above, ns. 12 and 13.

[60] For a detailed discussion of the iconography of this illumination see Voigts, "One Anglo-Saxon View of the Classical Gods."

[61] This antique tradition is dealt with by both Bethe and Weitzmann; the best known examples are the physicians' portraits in the famous Vienna Dioscorides (Nationalbibliothek Cod. Med. Gr. I, f. $2^{r}$).

[62] This reference to Aesculapius is not mentioned in Howald and Sigerist's "Index Nominum Propriorum" (p. 299) although they do have entries for the three references which may be interpolated.

[63] These references, to be found in Howald and Sigerist, *op. cit.* (n. 2 above), in the appendix, are in plant chapters CXIX, CXX, and CXXVI.

[64] Hunger, *Herbal of Pseudo-Apuleius*, pp. 118, 161.

It is plausible then that a factor or perhaps *the* factor in the decision of the fourth-century herbalist to appropriate the name of Apuleius of Madaura was the wish to declare that the ultimate source of the *Herbarium* was the god of medicine. It seems appropriate to conclude this assessment of the Apuleius attribution with the titles, incipits, and author-portraits of the work *Herbarium Apulei Platonici quem accepit . . . ab Aesculapio*, for they make the point most tellingly that if the authority for this herbal is Aesculapius (and Chiron, mentor of Aesculapius), then the most likely recipient of the wisdom of the god would be the one who had shown special devotion to him, his priest Apuleius. To see Aesculapius as the link between the name Apuleius and the *Herbarium* is not to discount altogether other factors in the herbalist's choice of pseudonym, particularly the possibility that Apuleius was considered a physician. It is to suggest, however, that in the god of medicine we find a credible explanation for the attribution of the *Herbarium* to Apuleius Platonicus.[66]

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[65] The question of donor(s) and recipient(s) in this book exchange has been muddled. Suffice it to say, the conventions of the depiction of a book exchange and the size and position of the central figure make it clear that Apuleius Platonicus is portrayed receiving the book from the two smaller figures. The description in Saxl and Meier (pp. 134-35) is accurate: "In der Mitte Plato, einen grossen Band haltend, der ihm von Escolapius l. und (Chiron) Centaurus r. überreicht wird."

[66] Since this article was written, a study of the illustration tradition of the *Herbarium Apulei* has appeared. Heide Grape-Albers, *Spatantike Bilder aus der Welt des Arztes* (Wiesbaden. Pressler, 1977). While Grape-Albers does not address the question of the attribution of the herbal, the volume should be consulted by anyone interested in the *Herbarium Apulei*.

## *THE OSLER MEDAL ESSAY*

# THE LIFE AND TIMES OF SIR FIELDING OULD: MAN-MIDWIFE AND MASTER PHYSICIAN

STEVEN A. BRODY

The history of modern obstetrics and gynecology, which had its origins in the rural folk practices of Europe during the Middle Ages, is first marked by the evolution and development of an unstructured and poorly regulated system of practical midwifery. By the end of the Renaissance the process of birth itself, shrouded in religious and ethnic superstition, was still being neglected by the foremost medical thinkers of the age. Similarly, the practice of midwifery was held in contempt by the established medical community even as late as the mid-eighteenth century. With advances in medical knowledge, however, it was inevitable that the care of the lying-in patient would ultimately progress from the hands of the unskilled sixteenth-century midwife to those of the trained accoucheur, or man-midwife, and finally to those of the physician skilled in the art of healing.[1]

Although obstetrics finally became an accepted specialty branch of the medical profession, the original practitioners of midwifery were never elevated to the ranks of the physician nor incorporated into their academic community. On the contrary, these midwives were slowly excluded from their usual work as physicians began to assume the various obstetric duties, albeit sometimes reluctantly; and because of the obvious benefit to the patient, the practice soon became institutionalized.

One notable exception to this pattern of gradual incorporation of obstetrics into the medical profession was the work of Fielding Ould, an Irish man-midwife who was a central figure in the historical transition from medieval midwifery to modern obstetrics. His careful observations and recondite insights, as recorded in his *Treatise on Midwifery* (1742), helped to revolutionize the field of obstetrics by establishing it as an area subject to scientific investigation not unlike any other medical specialty. He was the first to recognize and describe in detail the very existence of a "mechanism of labor," i.e. "the series of adjustments in position, lie and attitude of the fetus that permits it to

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[1] The sixteenth-century midwife was "a person of inferior education who had borne children herself and watched other women's labors; relics, charms and incantations comprised her armamentarium, in the main." Theodore Cianfrani, *A Short History of Obstetrics and Gynecology* (Springfield, Ill.: Charles C Thomas, 1960), p. 131.

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progress through the irregularly shaped pelvic cavity."[2] Furthermore, Ould's modern description of the proper handling of the third stage of labor, his advocacy of the surgical episiotomy, and the use of opiates for sedation all serve to ensure his importance in medical history. Another facet of Ould's life assumes importance in retrospect. Based on his own determined efforts and remarkable perspicacity, Ould had assembled a knowledge of reproductive physiology and anatomy unrivaled for his time. He proceeded summarily to fulfill the requirements for a medical degree at Trinity College; but his final petition for the medical degree, as a midwife-turned-physician, was a direct affirmation of the idea that obstetrics and gynecology were to have a place within established medical practice. In other words, Ould served as a prototype or test case for a novel idea which was immediately opposed by the contemporary medical community. It is this dramatic confrontation, along with his scientific contribution, which places Sir Fielding Ould, and the early history of the Dublin School of Obstetrics which he inadvertantly founded, in a pivotal position in the birth and growth of modern obstetrics.

It is the purpose of this paper to examine the role of Sir Fielding Ould's life and work in the intellectual, medical and social development of obstetrics and gynecology. This will elucidate the historical role of the physician-midwife as the forerunner of the modern obstetrician, and the reluctance of the established medical community to accept this latter development.

## *Early Eighteenth-Century Irish Midwifery and the Creation of the Rotunda*

> The misery of the poor women of the city of Dublin, at the time of their lying-in, would scarcely be conceived by any one who had not been an eyewitness of their wretched circumstances; their lodgings are generally in cold garrets open to every wind, or in damp cellars, subject to floods from excessive rains; themselves destitute of attendance, medicines, and often of proper food, by which hundreds perish with their little infants, and the community is at once robbed of mother and child.[3]

These sentiments of Bartholomew Mosse (1712-1759), reveal much of the motivation behind one of the grandest undertakings in the history of that branch of medicine dealing with "the diseases and conditions which are peculiar to women." For it was Bartholomew Mosse in 1745 who established the first lying-in hospital in the British Isles: the famous Dublin Lying-In Hospital, or simply, The Rotunda.[4]

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[2] J. R. Willson et al., Obstetrics and Gynecology (St. Louis: C. V. Mosby, 1971), p. 347.

[3] O'Donel T. D. Browne, *The Rotunda Hospital, 1745-1945* (Baltimore: Williams & Wilkins, 1947), p. 3. Wilde, "Bartholomew Mosse," *Dublin Quart. J. Med. Sci.*, 1846, *ii*: 565-596.

[4] For a refutation of the priority claim of Queen Charlotte's Hospital, see J. E. Donnison, "Note of the founda-

In the early eighteenth century, there were three classes of practitioners engaged in regular medical practice in Dublin. The apothecaries and barber surgeons learned their trade as apprentices to existing practitioners. Only the physicians professed a university education, encompassing a scientific or academic regimen, and their College of Physicians, founded in the mid-seventeenth century, was a professional organization without equal. The problem was, however, that most physicians had to seek their training at foreign universities. Even the graduates in medicine from the University of Dublin had, for the most part, first obtained degrees abroad. By 1711, the date of the founding of the Medical School of Trinity College, there was no civil hospital in Dublin where the scientific study or treatment of disease could be initiated. The founding of Jervis Street Hospital, Steeven's Hospital and Mercer's Hospital over the next twenty-five years afforded altogether a capacity of about one hundred beds. However, none of the hospital beds was designed for the purpose of teaching clinical medicine; and at the same time Trinity College Medical School offered no clinical teaching, only theoretical courses in anatomy, chemistry and botany.[5]

The profession of surgery was nominally regulated by the Guild of St. Mary Magdalene, which was first established as the Guild of Barbers by the Charter of Henry VI in 1446 and was subsequently joined by the surgeons, apothecaries and periwigmakers of the dominion. In actual practice, management of the Guild fell into the hands of the barbers and little real control was exercised over the surgeons.

The practice of midwifery intrinsically was considered a disreputable

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tion of Queen Charlotte's Hospital," *Med. Hist.*, 1971, *15* 398-400 Sir Richard Manningham had equipped a house as a lying-in nursing home in 1739 (in England) but there is no record of any established medical staff or lay committee affiliated with the institution, and it had ceased to exist at least by 1749. Queen Charlotte's Hospital, although founded 7 years after the Rotunda, was the first English institution to adopt the (Rotunda's) philosophy of extending charity to all lying-in women, even those who were unmarried or of a foreign nationality The original English lying-in hospitals, Brownlow Street Hospital (1749), City of London Hospital (1750) and Queen Charlotte's all had the Rotunda to look to as an example. The Rotunda was also the site of the first gynecological unit for the special care of women only; this was established by Evory Kennedy, Master of the Rotunda in 1835.

[5] Thomas P. C. Kirkpatrick and Henry Jellet, *The Book of the Rotunda Hospital* (London: Adlard & Son, 1913), chap I Nathaniel Barry was appointed King's Professor of Chirurgery and Midwifery in 1749. However it is doubtful that he ever lectured at all, certainly not on midwifery The King's Professors withdrew entirely from Trinity College Medical School after the controversy with the College of Physicians. The first native professor of midwifery, W. F. Montgomery, was not appointed until 1827. Although it is probable that students attended the Rotunda Lying-In Hospital as far back as Ould's time, there is no record of their presence or of any organized lecture series T P. C. Kirkpatrick, *History of the Medical Teaching in Trinity College, Dublin, and of the School of Physic in Ireland* (Dublin: Hanna and Neale, 1912), pp. 101-125, 313; also J B Fleming, "The teaching of midwifery in the University of Dublin," *Hermathena*, 1966, no ciii: 69.

field of endeavor to the skilled physician; consequently, it was left almost entirely to midwives, surgeons and apothecaries. Most physicians had no desire to acquire any proficiency in midwifery, and if a physician were called in for a difficult obstetric case "his presence must have been more useful for its moral support than for any benefit which his knowledge or experience could afford."[6]

Although the responsibility of examining and licensing midwives was delegated to the College of Physicians by the Charter of 1692, there was hardly any true regulation or instruction of midwives. Consequently, the Irish midwives were wholly untrained and often quite ignorant of their art, and were left to learn their practice from the labors of their unfortunate patrons. Furthermore, they were not as significant a social class as in England where the female midwives rebelled against the growing regulation of their practice by male practitioners.

The King and Queen's College of Physicians in Ireland rarely exercised its privilege in licensing midwives. By 1740 they had only examined four persons in midwifery; these included Matthew Carter and Fielding Ould in 1738 and Bartholomew Mosse in 1742, along with one female, a Mrs. Cormack in 1696.[7]

The hazards of obstetric practice were considerable in this period before the principles of antisepsis and anesthesia were understood. Even the ordinary social hand washings were not considered necessary before pelvic examinations or internal manipulations; and in the hospital, communal marine sponges were used for washing the patients until as late as 1881. Although the Chamberlen family secret of the forceps had been published by Chapman in 1733, it is improbable that Mosse knew how to use them or even owned a pair: "the vectis, the crotchet, the scissors and the fillet were all that were considered necessary." And of course, most men-midwives were restricted to manipulations under the bed-clothes of their modest patients since it was considered shameful for any man to view the process of delivery.[8]

Within this social and historical context, it is remarkable that at the age of thirty-three, Bartholomew Mosse, without assistance, wealth, precedent, or experience, set out to establish the first refuge for the impoverished lying-in patients of Dublin. No doubt Mosse's early childhood as the son of the Rector of Maryborough in rural Queen's County represented a marked departure from the urban squalor of Dublin, and in a sense contributed to his future philanthropy. In any case, Mosse was the product of a "genteel education" and was bound as a youthful apprentice to a Dublin surgeon, John Stone, for a period of five to seven years. He was finally certified to practice the art of surgery by the Surgeon-General in 1733, after which he was appointed the medical officer for a regiment of Irish troops in Minorca.[9] Before returning to Ireland, he travelled through several parts of Europe including England, France and Holland, with the intention of improving his mastery of surgery and midwifery. He soon became convinced of the necessity of a lying-in hospital, and after obtaining his midwifery license from the College of Physicians in 1742, he quit the practice of surgery to devote himself entirely to obstetrics. Mosse formed a group of contributors and acquired a large three-story house in George's Lane, now South Great George's Street, which had been used as a theatre by an Italian impresario, Madam Violante. It contained fifteen rooms and ten (later twenty-eight) beds and on March 15, 1745, the Hospital for Poor Lying-In Women admitted Judith Rochford, the first of 209 patients treated that year.[10]

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[6] Kirkpatrick and Jellet, *Book of the Rotunda*, p 5.

[7] *Ibid.*, p. 2

[8] Cianfrani, *op cit.* (n 1 above), pp. 245-247; and Browne, *op. cit* (n 3 above), pp 159-160.

Mosse supported his hospital through a variety of financial schemes including plays, exhibitions and even lotteries (where the winners were often inclined to return their booty). These lotteries involved Mosse in endless worry and on several occasions, almost in complete financial ruin.[11]

By 1748, Mosse was acutely aware of the inadequacy of his small hospital, and he proceeded to purchase a large tract of land on North Great Britain Street in order to build a lying-in hospital of one hundred and fifty beds. Even before the hospital was open, Mosse constructed a set of public gardens out of the adjacent acreage for the purpose of "general entertainments" which would support the hospital financially. These gardens, which contained bowling greens, walkways, "coffee stalls," and "concert rooms" soon became one of the focal points of high society in Dublin. Further financial difficulties forced Mosse to appeal to the Irish Parliament and the City Corporation for assistance, and on two occasions he was granted large sums of money "as a reward for his great care and diligence in attending the lying-in hospital" and because he had "greatly injured himself in his profession, and hurt his family in their circumstances, having never received any reward"; a Royal Charter was also granted by George II on December 2, 1756, making Mosse the first and only Master for life (all other Masters holding office for seven years).[12]

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[9] Wilde, *op. cit.* (n. 4 above), p. 567.

[10] Browne, *op cit* (n. 3 above), pp. 3-6.

[11] Kirkpatrick and Jellet, *Book of the Rotunda*, p. 27.

[12] *Ibid.*, p. 38.

The new hospital on Great Britain Street, although not entirely completed, was opened on December 8, 1757, as the Dublin Lying-In Hospital. The inaugural was celebrated in a typically grand fashion, with a public breakfast, after which "fifty-two poor women, great with child, . . . were all decently clothed in uniform at the expense of the Hospital, each in a blue calimanco gown and petticoat, shift, handkerchief, cap, and apron; and thus they appeared before his Grace as President of the Hospital, the Duchess, and the rest of the Governors and Guardians, with many of the nobility and gentry, who all expressed the highest satisfaction." On the same day "Mary Rea and Elizabeth Knight were safely delivered of fine babies." The original hospital was closed down, after twelve years of service to 3,975 lying-in patients.[13]

Unfortunately, Bartholomew Mosse died of unknown causes on February 16, 1759, in the prime of his life at the age of forty-seven. His only legacy was the lying-in hospital; dedicated to the relief of the poor and the teaching of midwifery, this institution had totally consumed the last fifteen years of his life.

### *The Life of Sir Fielding Ould*

Although Mosse is justly remembered as an inspired philanthropist and social iconoclast, it was his successor, Fielding Ould, who established the Irish School of Midwifery with his significant writings on the practice of obstetrics and the fundamental process of labor.

Fielding Ould was a descendant of "an ancient English family" that had settled in Sherborne early in the 17th century. Ould's grandfather, Colonel Robert Ould (b. 1667), came to Ireland with the invading army of King William III, and in 1690 he commanded the Royal Regiment of Welsh Fusileers at the Battle of the Boyne, where James II was defeated. After the surrender of Dublin, Colonel Ould and his son, Captain Ould (b. 1689), were registered on the Freeman's Roll of the city, since this was an expedient courtesy extended to all the senior officers of the conquering army.[14] While stationed in Galway after the battle of Aughrim, Captain Ould married a Miss Lettice Shawe, the daughter of Rev. Fielding Shawe, D. D., who was Canon of Tagh Saxon and a distinguished and wealthy scholar from Trinity College. Mrs. Ould lived in England with her two children, Fielding (b. 1710) and Abraham, until Captain Ould was "assassinated at night in the streets of London" (c. 1715).[15] Both children were subsequently educated in Galway, under the supervision of Rev. Shawe. Abraham became an apprentice to an apothecary and later in his life achieved some success as a barrister.

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[13] Browne, *op cit* (n. 3 above), p. 14. Also see Alfred H. NcClintock, "On the rise of the Dublin School of Midwifery with the memoirs of Sir Fielding Ould and Dr. J. C. Fleury," *Dublin Quart J. Med Sci.*, 1858, *25*. 1-20

[14] Kirkpatrick and Jellet, *Book of the Rotunda*, p. 53.

[15] McClintock, *op. cit.* (n. 13 above), p. 6.

Around the age of 19, Fielding Ould began a 5-year period of employment as a prosector in the anatomy department of Trinity College Medical School, although he was not a matriculant. In his *Midwifery* (1742), he states that he "spent that time which others employ in their improvement in polite Literature, in a more laborious Manner; namely, in the Dissection of human Bodies."[16] During this entire period Ould also managed to attend the lectures on natural philosophy, chemistry and botany. His endeavors were well-founded since these three disciplines, together with anatomy, constituted the entire medical curriculum of Trinity College.

For reasons which are not at all clear, Ould decided not to pursue a college course, but instead left Ireland to study midwifery in Paris. This career decision is particularly unusual in view of the disparaging light in which midwives were held at that time. Indeed, before Ould there was no precedent for men-midwives in Ireland, and formal instruction in the field was not even offered in the country until Fleury challenged the Medical Board of Trinity College twenty-five years later, in 1761.

During the early 1700s Paris was considered the finest center for obstetric training and Ould never failed to record his ultimate indebtedness to France:

> . . . the Opportunities that are there met with, are no where else to be found, without which, it is hardly possible to be an adept; namely, those of occular Demonstration of Women being delivered, both in natural and preternatural Labours; where, as well the external Parts of the Patient, as every Action of the Operator, are the whole Time in View.

While in Paris, Ould was apparently a diligent student of the celebrated Grégoire, who offered private lessions in midwifery; and in recalling this period he states, "I made the strictest Examination of every Woman, which I either delivered, or saw delivered, during my Continuance in Paris."[17]

As a consequence of his perseverance and perspicacity, Ould made several original observations which at once seemed to conflict with the standard teachings of the day. While watching a case of prolonged labor due to an entangled umbilical cord (funis), Ould noticed that the head of the fetus, descending and receding in the pelvis several times, was always directed with the face towards the shoulder. The attending practitioners assumed this to be an abnormal posture of labor and would have dramatically intervened "but fortunately, a few pains completed the delivery."[18] This experience did not escape Ould's attention, and he made the process of expulsion of the presenting head a matter of meticulous observation. Prior to this time little thought had been given to the mechanical process or "mechanism" of labor. It was believed that the head of the fetus entered the superior inlet of the pelvis with its sagittal suture parallel to the conjugate diameter and with the occiput of the fetus facing the symphysis pubis, or sometimes the sacrum. Ould was the first author to explain that the face of the fetus must be directed to the lateral side of the pelvis so that the sagittal suture lies relatively parallel to the transverse diameter, thereby reconciling the widest diameter of the fetal head with that of the pelvis. Dr. Rigby, in his translation of Naegele's work, rightfully acknowledges this singular contribution as the "punctum saliens," or first great step, towards all knowledge and understanding of the mechanism of parturition.[19] Ould's subsequent description of this principle is as follows:

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[16] Fielding Ould, *A Treatise of Midwifery*. In three parts (Dublin: O. Nelson & C. Connor, 1742), p. 4

[17] *Ibid.*, p. 71.

> When a Child presents itself naturally, it comes with the head foremost, and (according to all Authors I have seen) with its Face towards the Sacrum of the Mother, so that when she lies on her Back, it seems to creep into the World on its Hands and Feet. But here I must differ from this Description in one Point, which at first Sight may probably seem very trivial: The Breast of the Child does certainly lie on the Sacrum of the Mother, but the Face does not; for it always (when naturally presented) is turned either to one Side or the other, so as to have the Chin directly on one of the Shoulders.
>
> This every Practitioner in Midwifry has in his Power to be certain of; yet it may not be amiss to prove it to the Reader, who has not yet practised, by plain Reasoning: First, it is evident that the Head from the Os Frontis to the Occipitis, is of an oblong Figure, being very flat on each side: Secondly, that the Body, taking in the Shoulders makes still a more oblong Figure, crossing that of the Head; so that supposing the Woman on her Back, the Head coming into the world, is a kind of Elipsis in a vertical Position; and the Shoulders of the same Form in an Horizontal Position: Thirdly, that the Pelvis is of an Eliptical Form, from one to the other hip. Now, if the Child presented with the Face to the Sacrum, the oblong Figure of the Head must cross that of the pelvis; and if it were possible that the Head and Pelvis could be formed to each other, so as to admit of its Exit, it must of Necessity, from what has been said above, acquire another Form for the Admission of the Shoulders; which is very different from the constant Uniformity in all the Works of Providence.
>
> From what has been said, it is evident that when the Child is turned, so as to have the Chin on one Shoulder, all the above Objections are removed; for the Head and Shoulders are on a parallel Line, in respect of their Shape, and at the same Time, both answer the Form of the Passage from the Pelvis.
>
> For want of this Knowledge, many Labours prove dangerous and tedious, that might

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[18] Edward W Murphy, "Introductory lecture on the history of midwifery," *Brit. Med J.*, May 14, 1864, 523-28.

[19] C F. Naegele, *An Essay on the Mechanism of Parturition* (London: Callow & Wilson, 1829), pp. vi-ix.

> have been very successful had they been committed to Nature. For it is too common for Midwives, immediately on the Eruption of Waters, to move the Child's Head to and fro, in order to facilitate its Exit; and this jogging may very easily alter the Position of the Head, so as to make it what is generally esteemed natural; hence the Crown of the Head, near the joining of the coronal and sagital Sutures, are by the Efforts of the Mother, forced against the Os Pubis: When this happens, the Women tell you, the Head is fixed on the Share Bone, which in Reality, is the intersection of two Elipses, namely, the Head and the Passage from the Pelvis; but the Misery does not end here; for the repeated Throws of the Mother, forcing the Head against the Pubis, at the Point above-mentioned, pushes it so as to make the Lambdoidal Bone lye on the Back, whereupon the Face presents itself; the Consequence whereof shall be mentioned in its proper Place, when we treat of preternatural Deliveries.
>
> It is to be hoped that this Opinion, being founded on Theory, and confirmed by Experience will meet with few Opponents; and without doubt, The due Application of it will be of infinite Use.[20]

After completing his two years of study in France, Ould returned to Dublin and began to practice midwifery early in 1738 and was licensed (in midwifery) by the College of Physicians in August of that year, at the age of 28. Three and a half years later, Ould completed his only published work, *A Treatise of Midwifery* (1742), which embodied the observations which placed obstetrics on a scientific foundation for the first time. The book was dedicated and submitted for approval to the President, Censors, and Fellows of the College of Physicians in Dublin, by "their most obedient, humble Servant, Fielding Ould"; and in front of the title page the appropriate officers of the College expressed their high regard for the substance of the manuscript (dated Dec. 5, 1741).

In 1745, three years after the publication of his *Midwifery*, Ould was appointed Assistant Master of the newly founded Dublin Lying-In Hospital. For the most part the Assistants and the handywomen were responsible for the actual deliveries and examinations at the hospital, while Master Mosse provided general supervision over the whole maternity practice.

Like many other practitioners of his time, Ould did not confine himself strictly to the realm of his specialty (obstetrics & gynecology) in the care of his patients.[21] In addition, the few men-midwives now working in Dublin, including Ould, had managed to build financially successful practices from their large pool of patients. These developments evoked the resentment and jealousy of the College of Physicians. And so, on May 6, 1745, only two months after the founding of the Rotunda—a milestone in the history of midwifery and gynecology—the College of Physicians (which had for years neglected its duty to regulate the practice of midwifery) decided to reassert its control over medical practice:

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[20] Ould, *op. cit.* (n 16 above), pp 28-31

[21] Kirkpatrick and Jellet, *Book of the Rotunda*, p. 55.

> Whereas it has been found that several persons licensed to practise midwifery only have notwithstanding presumed to practise physick in general, . . . we will not for the future consult with any of them as physicians, nor with any other person, who is not a graduate or licensed physician of this College.[22]

In the early 1750s Ould strove to overcome the limitations of his practice by obtaining a medical degree, thereby improving his professional prospects and raising himself to a more dignified station in life than that of a lowly midwife. This plan dates the onset of a controversy which was soon to embroil Ould's future career, and ultimately sever all relations between the two institutions responsible for the conduct of Irish medicine.

The rules for medical licensure at this time were governed by the Charter of William and Mary (1692). This document granted the College of Physicians the power to license physicians within a 7-mile radius of Dublin, on the condition that all successful graduates of the University of Dublin (Trinity) would automatically be awarded a license from the College, upon payment of a fee. The purpose of the latter stipulation was to ensure that preference would be given to graduates of Ireland's only medical school. In order to exercise some regulatory control over the physicians they were licensing, the College of Physicians made an agreement with the Board of Trinity College on July 25, 1701, which allowed them (the College of Physicians) to conduct the final examinations for the University's medical degree. In return, the College agreed to license, and accept as Fellows, only the medical graduates of Trinity.[23] This arrangement worked well until Ould applied to the Board of Trinity College and was granted a Bachelor of Arts by "Special Grace" (and without examination), in order to fulfill this requirement in preparation for obtaining a medical degree. Only four days later on Feb. 19, 1753, the College of Physicians, fearing that the University might grant Ould his medical degree by "Special Grace," passed a resolution stating that recipients of such medical degrees would not be licensed by the College of Physicians. Although their fear was unfounded, since Ould presumably was capable of passing their professional licensing examination, the resolution itself was not unreasonable. However, these physicians were not actuated merely on behalf of academic integrity since they adopted this additional resolution on the same day: "Ordered yt the College of Physicians shall not for the future examine any person who hath or does practise midwifery, for any degree in Physick, or a License in Physick."[24] By this action the College expressed some of their animosity toward Ould and their bias toward men-midwives in general, who practiced a purely mechanical, laborious art that was derogatory to the dignity of academic medicine. The hypocrisy of this decision is underscored by the fact that some of the Fellows of the College were noted for their expertise in the art of obstetrics. Furthermore, one of the signatories of the resolution was a Dr. Nesbitt, Consulting Physician to the Rotunda and later one of the Governors who would elect Ould Master of the Hospital. In any case, there is no way to justify retrospectively the decision to refuse to examine a qualified medical candidate, simply because he had practiced, and would continue to practice, midwifery.

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[22] Kirkpatrick, *History of the Medical Teaching in Trinity College*, p 117.

[23] *Ibid.*, pp. 117-125.

Ould felt himself unable to overcome this situation, and in November of 1756 he wrote an "abusive" letter to the President of the College of Physicians, and a copy of the by-laws was sent to Ould in reply. At this point, Ould appealed to Trinity for permission to be examined for an M.B., but his request was denied because of the fear of provoking "much uneasiness in the College of Physicians." By January of 1757 the College of Physicians demanded that all licentiates sign the resolution of May 6, 1745, forbidding consultation with midwives and other non-medical personnel.

The controversy over Ould still remained unsettled when Mosse died on Feb. 16, 1759. Subsequently, Dr. Carter and Mr. Ould voluntarily began to supervise the operation of the hospital on alternate months until Ould was unanimously elected the new Master on November 2, 1759. In the meantime, Provost Richard Baldwin of Trinity College had died in 1758, and his position was filled by Francis Andrews, a man who was favorably influenced by Ould. On June 2, 1759, Ould was granted a *liceat* for his degree of Bachelor of Medicine from Trinity College and with the support of the Provost he attempted to force the issue with the College of Physicians.[25] The latter organization still refused to examine Ould as a candidate for the M. D. degree. At this point, Trinity College simply bypassed the College of Physicians by conducting their own qualifying examination within the University. Subsequently, Trinity granted Ould his Bachelor's Degree in Physick in 1760, and his Doctor's Degree in Medicine on June 29, 1761. The Physicians refused to tolerate Trinity's transgression and "contempt" of their authority in granting a degree to someone "who had no Academic Education," and who was "disqualified by his occupation." At a meeting held on February 5, 1761, they resolved to sever the 60-year long association which had existed between the two institutions.[26] Trinity College subsequently formed a new committee of examiners among its own faculty, and when Robert Robinson, Professor of Anatomy (and the only professor who was also a Fellow of the College of Physicians), refused to concur, he was dismissed.

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[24] *Ibid.*, p. 118.

[25] Kirkpatrick and Jellet, *Book of the Rotunda*, p 57

The absurd injustice committed against practitioners of the obstetric art by the College of Physicians was memorialized in 1775 by Gilborne who wrote of the official disapproval of physician-midwives:

> The College him a Fellow would announce,
> Condition this, to Midwifery renounce;
> Renounce but sooner he would his Right Hand
> Then from the Service of the Fair disband
> Why may not any Doctor that would chuse
> For Man's Relief his total knowledge use,
> Or does one Portion of Apollo's Trade
> More than the rest his votaries degrade?[27]

Gradually, the disabilities placed upon men-midwives in Ireland were to disappear, but it was not until 1785, at the age of 75, that Ould was admitted as a licentiate (but not a Fellow) in medicine by the College of Physicians. Of course, it was this same year that the College of Physicians elected Francis Hopkins, a physician-midwife and future Master of the Rotunda (1807-14), president of their organization. Hopkins published his own midwifery, *Vade Mecum*, in 1811.

In the midst of his dispute with the College of Physicians, Ould reached the zenith of his fame when the Lord Lieutenant, Duke of Bedford and Viceroy of Ireland, rewarded him with knighthood on May 17, 1760 (the only Master honored in such a way). This event generated several epigrams, including:

> Sir Fielding Ould the sword of knighthood gained
> For saving ladies' lives in child-bed pained [28]

and,

> Sir Fielding Ould is made a knight,
> He should have been a lord by right;

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[26] Kirkpatrick, *History of the Medical Teaching in Trinity College*, p 121· ". we do hereby declare that for the future we will not examine your candidates, nor officiate at the performance of their public acts; and that we will receive into our College the graduates of other Universities, if sufficiently recommended by their learning and morals, tho' not admitted *ad eundem* in yours." Minutes of the College of Physicians, Feb. 5, 1761

[27] Charles Cameron, *History of the Royal College of Surgeons in Ireland and of the Irish Schools of Medicine* (Dublin: Fannin, 1886), pp 26-28

[28] Browne, *op. cit.* (n. 3 above), p. 21. The exact reason for Ould's knighthood is not known At the time of the ceremony the Duke was the President of the Board of Governors of the Rotunda, but Kirkpatrick believes that Provost Andrews and his intimate friend the Hon. Richard Rigby, the Irish Secretary, may have been the most influential parties promoting this honor for Ould. Kirkpatrick, *Book of the Rotunda*, p 58.

For then each lady's prayer would be,
O Lord, good Lord, deliver me![29]

During his tenure as Master of the Rotunda (1759-1766), Ould took steps to completely renovate the hospital grounds and gardens. He supervised the construction of the large Round Rooms, in order to complete Mosse's original plan for "a large, impressive and suitable place of entertainment." This led to the adoption of the "Rotunda" as the name of the hospital—a name which Mosse himself had never heard. Ould was elected a Governor of the hospital a year after assuming office. During his 7-year Mastership he had made significant efforts to improve the welfare of his patients.[30] Out of 3,800 women delivered there were 49 deaths (a maternal mortality of 1.29%).

During Sir Fielding Ould's professional career, which spanned half a century, he enjoyed an extensive and lucrative private practice. Indeed, he had many patients from the nobility, including the Countess of Mornington, who gave birth in April of 1769 to her son Arthur, the future Duke of Wellington.

ROTUNDA MATERNITY RECORDS

1745-1943

| Period Covered | Maternal Deaths | Deliveries | Perinatal Deaths | Births | Maternal Mortality | Perinatal Mortality |
|---|---|---|---|---|---|---|
| First year at George's Lane, 1745 | 1 | 208 | 18 | 208 | .48% | 8.7% |
| Total of 12 years at George's Lane, 1745-57 | 44 | 3,975 | 407 | 4,049 | 1.11% | 10.1% |
| First 14 years Mosse's tenure 1745-59 | 58 | 4,890 | 603 | 4,986 | 1.19% | 12.1% |
| First Year at Great Britain Street, 1758 | 8 | 454 | 75 | 462 | 1.76% | 16.2% |
| Ould's 7-year tenure, 1760-66 | 49 | 3,800 | 905 | 3,854 | 1.29% | 23.5% |
| Collum's tenure 7 years, 1767-73 | 65 | 4,724 | 1,150 | 4,800 | 1.38% | 24% |
| One-year—1943 (five-fold reduction in maternal deaths) | 10 | 3,979 | | 3,822 | .25% | |

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[29] Cameron, *op. cit.* (n. 27 above), p. 27

[30] Browne, *op. cit.* (n. 3 above), p. 20

Ould continued his work and his affiliation with the Rotunda until November 29, 1789, at which time he died suddenly from a stroke, shortly after delivering a patient. He was buried in St. Anne's Church in a grave which has never been identified.[31]

## *The Work of Sir Fielding Ould*

Sir Fielding Ould's *Treatise on Midwifery*, the first such text published in Ireland, established him as a pioneer and authority in scientific obstetrics. For almost two centuries prior to this time, British midwives were almost entirely dependent on a little book called *Rosengarten* (1513) which was translated into English as *Byrth of Mankynd* in 1540. Harvey published the first original English book on obstetrics in 1651, *De generatione animalium*. But this book devoted only a single chapter to the process of labor, addressing its physiological aspects; the rest of the book dealt with embryology.

François Mauriceau (1637-1709) of Paris and Henrik Van Deventer (1651-1724), who are often referred to as the founders of modern obstetrics and gynecology, were Ould's immediate forerunners historically. Ould directed much of his criticism towards these two authorities as well as several others: "many of their Schemes are like those of some Navigators and Geographers, who never made use of a Compass, but in their Closet."[32]

In the preface to his *Midwifery*, Ould unassumingly repudiated any claim that his method of practice was automatically superior to the unrecorded techniques of his contemporaries. Rather, he decried the fact that each practitioner must painfully develop his own method of practice without the benefit of the collective written experiences of his predecessors: ". . . for the particular Acquisition of every Person added together, would amount to a great Sum whereby our Art would soon flourish."[33]

The *Treatise* is divided into three sections which function as a practical guide to the art of midwifery; the first deals with descriptive anatomy (including the fetal circulation) and the conduct of a normal labor, the second deals with abnormal cases not requiring instruments, and the third discusses instrumental delivery.

The most remarkable aspect of Ould's book is that it contains a variety of innovative and controversial practices which are today routinely accepted as part of the standard armamentarium of the modern obstetrician.

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[31] Herbert R. Spencer, *The History of British Midwifery from 1650 to 1800* (London: John Bale Sons & Danielson, 1927), pp. 30-37, esp p 33

[32] Ould, *op. cit.* (n 16 above), p. ix Spencer, *op cit.*, p. 33, calls Ould's work "a distinct advance on the original obstetrical books which had previously appeared in English." See William LeFanu, "The lost half-century in English medicine," *Bull. Hist. Med*, 1972, *46*: 319-348, where reference is made to Ould's work as the first adequate treatise of midwifery

[33] Ould, *op cit* (n. 16 above), p x

Ould admonished Mauriceau and other authors for adhering to the absurd view that the uterus expands by thinning its walls, in the same manner as the bladder. In Ould's words: "It is wronging Providence to the last Degree, to let slip, not to say conceal, a Phenomenon, so surprisingly beautiful, because it surpasseth our narrow Comprehension."[34]

He openly ridiculed Deventer's widely accepted view that difficulties of labor were mostly caused by obliquities in the position of the uterus: "This Matter surprised me more than any Thing I ever met with; That a Man should impose on the learned World in a Matter so demonstrably false."[35]

In reference to the conduct of practical midwifery, Ould favored the left lateral position as "the most advantageous Posture for natural Labours," and he performed vaginal examinations in this manner as well. At this time most women on the continent were delivered in the supine lithotomy position, in contrast to the British Isles where a variety of postures were in existence ". . . namely, on their Back, Side, Knees, Standing, and Sitting on a perforated Stool." Ould felt that by placing women on their sides the flat surface of the bed would apply less pressure to the coccyx, thereby permitting the structure to hyperextend naturally as the child presses posteriorly. The side position also offered the least resistance to manipulation, kept the patients "less subject to Cold . . . and as the Operator and Standers by, are by this Means behind her Back, she is less subject to be disturbed by their Remarks and Whispers."[36] It is of note that the left lateral position is still a position used today throughout Ireland and the Rotunda. Its further advantages include decreased weight bearing by the patient, reduced risk of perineal lacerations, and decreased pressure on the great abdominal (retroperitoneal) vessels, thereby resulting in improved uterine perfusion.

Another innovative practice described in Ould's *Treatise* is in regard to the third stage of labor. Most authors before Ould, including Deventer, had recommended immediate manual separation of the placenta from the uterine wall, for fear of placental retention as the uterine wall promptly contracts and the cervical orifice closes. Ould rightfully explained how this unfounded fear resulted in a dangerous practice because of the possibility of hemorrhage, ripped placental fragments and severe pain. Consequently, he believed that manual separation should only be used in cases of delayed placental delivery and other emergencies. Ould's technique was to allow the uterus to expel the placenta naturally with the aid of the voluntary bearing-down efforts of the mother, while maintaining moderate traction on the cord.

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[34] *Ibid.*, pp xii-xiii. Ould's objection is appropriate, but his description falls short of the modern viewpoint. While it is true that the first twenty weeks of uterine enlargement is accomplished by hypertrophy of muscular and connective tissue elements with rapid weight gain, the next period of twenty weeks is characterized by longitudinal thinning and stretching of the wall with a slower increase in weight gain so that the uterus becomes elongated rather than spherical L. M Hellman and J. A. Pritchard (eds.), *Williams Obstetrics*, 14th ed. (New York: Appleton-Century-Crofts, 1971)

[35] Ould, *op cit.* (n 16 above), p xxi and pp. 96-100

[36] *Ibid* , pp 31-34.

> The child . . . must be laid on the Operator's Lap, or on the Bed, as far from the Mother as the Length of the Funis will admit; which he must take in the right Hand, about six Finger's Breadth from the Pudendum, and roul it twice or thrice about his Finger; then the first and second Fingers of the left Hand must be thrust into the Vagina, by its Direction; and the Patient stopping her Breath and forcing as if she were at Stool, the Navel-string must be gently pulled forward as she forces, the Operator rather waiting for her Expulsion of it, than being too desirous to extract it; for pulling the Funis so as to extract the Placenta forcibly, may probably cause a Flooding; or perhaps break the Navel-string whereby the Placenta would be very difficultly brought forth; therefore let him just pull in sufficiently to make it incline forward still insisting on the Patient's forcing down, which if she be not able to do of herself she must be compelled to it, by putting a Finger into her Throat, which will cause a Pressure of the Diaphragm, and the Muscles of the Belly, by her Efforts to vomit; by these Means it is commonly brought forth in about five Minutes. When it comes away by Expulsion, it always is whole, but it is subject to be broke, and Part of it is left in the Womb, if any Violence be used for its Extraction.
>
> Most Authors give a strict Charge to lose no Time in the Extraction of this extraneous Body lest the Orifice of the Womb should contract and obstruct its Passage; and for this Reason they advise the Introduction of the Operator's Hand into the Matrix; and by insinuating the Fingers between it and the Placenta, to cause their Separation. . . . This Fear of the Womb closing, makes many Operators too hasty, which often produceth fatal Accidents.[37]

Ould's conservative approach represented an important advance over previous authors. Modern obstetrical practice is predicated on differentiation of placental separation, support of the uterus during controlled traction delivery from the lower segment, and the use of uterotonic agents to prevent postpartum hemorrhage.

In cases of breech extraction or podalic version, Ould chided Deventer for the "greatest Piece of Cruelty that can be imagined; namely, to leave the Hands to come out together with the Head." Ould advocated the modern method of bringing down the arms before delivering the after-coming head, thereby reducing the possibility of cephalic obstruction.[38] He performed complete breech extractions with his hand introduced into the uterus to bring down the feet. The head was delivered with one hand over the shoulders and the fingers of the other hand in the child's mouth. In the case of twins, Ould delivered the second infant (by podalic version) immediately after the first, a method which was "never practiced by the Female Adventurers in the Art of Midwifery," but which is currently the accepted procedure.[39]

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[37] *Ibid* , pp. 57-58. In 1786 Clarke described a modification of Ould's technique for the delivery of the placenta which involved the application of external manual compression to the fundus This procedure, which forms the basis of the old "Rotunda Method" of the third stage of labor, is described in detail by O'Donel Browne, *A Manual of Practical Obstetrics* (Bristol. John Wright and Son, 1936), pp. 68-70

Ould was also the first to recommend internal podalic version in cases of moderate cephalopelvic disproportion due to a contracted pelvis. He would introduce his hand into the womb, grab both feet, turn the fetus and achieve delivery by breech extraction: "it will appear, that by drawing from a small end, which is the Feet, in order to bring forth the larger, . . . there is far greater Probability of bringing it forth; than when the large End comes first."[40] He further relied on this technique for hand presentations, compound presentation involving the extremities, prolapse of the cord, delivery of the dead fetus and transverse lie. The knee-chest position was used for all these preternatural deliveries.

Ould made use of digital manipulation per rectum to disengage an obstructing coccyx or to pull the child forward with the finger fixed under the child's mandible, in cases of prolonged labor.

> The Manner of assisting a weak Patient in her last Labour-Pains, by introducing a Finger into the Anus, has never before been taken Notice of by any Author which I have seen; which I wonder at, for the good Effects of it are obvious to the meanest Capacity.[41]

For further assistance Ould advised that "an Opiate is of surprising Service; for while the Medicine operates, The Patient is lulled, and the Pain quite removed; but when the Narcotic Quality is gone off, she revives with new Vigour."[42] If the lip of the cervix were caught between the pubis and the ver-tex, Ould routinely pushed back the edge of the cervix over the head of the fetus.

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[38] Ould, *op. cit* (n. 16 above), pp. 122-130 Ould believed the breech was caused by the "Body being in more than ordinary erect postures in the womb" (*ibid* , p. xv). In Ould's words. "It is obvious to everyone who has practised Midwifery, how much Pain and Labour a poor Woman must undergo, to bring forth the Head of a Child by itself; (for in that consists the whole Difficulty) how great then must be the Torture, if it was possible to drag forth by main Force, with an Arm at each side; I must own I think it is the greatest Act of Inhumanity to attempt it" (*ibid.*, p. 128)

[39] *Ibid* , pp. 55, 130-133, and preface

[40] *Ibid.*, part II. Also see J. L Breen and E. Wiesmeier, "Compound presentation. a survey of 131 patients," *J Amer College Obst & Gynec* , 1968, *32*: 419-422. Ambroise Paré (1510-1590) is responsible for the reintroduction of podalic version which had been practiced centuries before by Soranus (A. D. 98-177) For compound presentation, modern textbooks recommend either version or disengagement of the head with the replacement of the prolapsed extremity into the uterine cavity

[41] Ould, *op cit* (n. 16 above), preface and part II

[42] *Ibid.*, p. 38.

Ould was the first author to advocate and describe the method of episiotomy (i.e. incising a rigid perineum) and its repair, a procedure which is now extremely common because it prevents serious lacerations and avoids perineal obstruction:

> It sometimes happens that the Head of the Child . . . cannot however come forward, by Reason of the extraordinary Constriction of the external Orifice of the Vagina . . .; wherefore it must be dilated if possible by the Fingers, and forced over the Child's Head; if this cannot be accomplished, there must be an Incision made towards the Anus with a Pair of crooked Probe-Sizars; introducing one Blade between the Head and Vagina, as far as shall be thought necessary for the present Purpose, and the Business is done at one Pinch, by which the whole Body will easily come forth.[43]

Ould was relatively conservative in his consideration of certain obstetric manipulations, preferring not to meddle in what were otherwise natural processes. Ould counseled his readers to use forceps and other such dangerous "foreign materials" only when it is "absolutely unavoidable" since such manipulations "are very useful and necessary, when undertaken with Caution, Skill and Prudence." He set forth guidelines for the application of "the large Forceps, which is in general Use all over Europe."[44] This included gross cephalopelvic disproportion (with a living fetus only), leading to the patient's exhaustion and excessive prolongation of labor.

For delivery of the dead fetus, Ould developed his own instrument, an advancement over Mauriceau's "Tire-Tête," which avoided bruising the mother from the large forceps applied to the extracted fetal head. Ould contrived a sheathed cutting instrument, The Terebra Occulta, for perforating the skull, and reducing the cephalic mass to a reasonable size for delivery. The latter was accomplished by evacuating the substance of the brain with manual pressure.[45]

As for the cesarean section, Ould considered it to be a "detestable, barbarous, illegal Piece of Inhumanity," which is "most certainly mortal." Its only possible indication was to rescue the mother and child when the pelvis is so contracted that the hand is completely obstructed from access to the fetus.[46]

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[43] *Ibid.*, p 145. See also L. E Laufe and D C Leslie, "The timing of episiotomy," *Am J Obst & Gynec* , 1972, *114*: 773-774, for a brief look at the history of the episiotomies. Previous authors had suggested salves for softening the perineum. Also see Constance Beynon, "Midline episiotomy as a routine procedure," *J Obst & Gynec. Brit Commonwealth*, 1974, *81*. 126-130

[44] Ould, *op cit.* (n 16 above), p 153

[45] *Ibid.*, pp 177-189

[46] *Ibid* , pp xxiii-xxiv and 195-203 The cesarean section was first performed in modern times by a sow gelder named Jacob Nufer on his own wife in 1500 In the United Kingdom the first reported cesarean section was

With the foregoing in mind, it is worth emphasizing that Ould's pivotal significance in the history of obstetrics and gynecology relates to his seminal discovery which forms the basis for all knowledge of the mechanism of labor. He started out by examining how the body of the fetus accommodates to the geometry of the pelvis. And in doing so, he realized that the "ellipses" formed by the head and the shoulders must both enter the pelvic cavity in a transverse (or oblique) diameter to effect fetal engagement and descent, and subsequently account for restitution of the head. He clearly recognized for the first time that the head rotates during its passage in a predictable, understandable manner.

### *Recognition of Ould's Work*

There is strong evidence to suggest that medical historians have unduly overlooked Ould's general significance, the priority of his discoveries and his influence on subsequent practitioners. The next great writer on the mechanism of labor was William Smellie (1697-1763), the so-called Master of British Midwifery, an undisputed genius who remained a productive teacher of over 900 pupils during his lifetime. In his *Treatise on the Theory and Practice of Midwifery* (1752) Smellie greatly advanced the understanding of the normal mechanism of labor, and the causes of dystocia. Although he was definitely aware of Ould's work, he gave him little credit. This is all the more surprising since Smellie, who had also trained under Grégoire, was Ould's elder by thirteen years; and yet his exposition on the mechanism of labor did not appear until 1752, ten years after Ould's *Midwifery* was published.[47] Indeed, Smellie's *Treatise* states that he "endeavored to reduce the art of midwifery to the principles of mechanism, ascertained the make, shape and situation of the pelvis, together with the form and dimensions of the child's head, and explained the method of extracting from the rules of moving bodies in different directions."[48] This theme, which is quite reminiscent of Ould's own

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accomplished by an illiterate Irish midwife, Mary Donally, in County Tyrone in 1738, four years before the publication of Ould's work. The 33-year-old patient survived the extraction of her dead child and "in about twenty seven Days the Patient was able to walk a Mile on Foot," T. E. Cone, Jr., "Cesarean section performed with success by an illiterate Irish midwife in 1738," *Pediatrics*, 1974, *54*. 460. Ould's warning about the danger of the procedure stems from the general morbidity of abdominal puncture wounds in his time, and from the probability of excess hemorrhage because of the increased vascularity of the womb. See Fleetwood Churchill, *Researches on Operative Midwifery* (Dublin: Martin Keene, 1841), esp p 191

[47] Cianfrani, *op cit* (n 1 above), pp. 244-248 *A Course of Lectures upon Midwifery* was published by Smellie in 1742

[48] Spencer, *op cit.* (n 31 above), p 55 Smellie actually reiterated Ould's theories in stating: "when the head first presents itself at the brim of the pelvis the fore-head is to one side, and the hind-head to the other . . thus the widest part of the head is turned to the widest part of the pelvis." Subsequently, Smellie was able to describe perfectly the mechanism of internal rotation whereby the head passes from the transverse to the conjugate diameter. William Smellie, *Treatise on the Theory and Practice of Midwifery* (London, 1752), pp 90-96

pioneering efforts, no doubt contributed to Smellie's two great achievements: the modern refinement of the forceps and the development of pelvimetry. Furthermore, Smellie referred to the left lateral posture of labor, first advocated directly by Ould, as "the London Method." Regarding the third stage of labor, Smellie was not as modern as Ould, since he tended to pull the funis and pass his hand into the womb relatively early after the child's delivery and "on slight occasions," according to McClintock.[49]

John Burton of York (1710-1771), a famous critic of Smellie who was caricatured as Dr. Slop in *Tristram Shandy*, was another contemporary author in the field of midwifery who copied Ould's methods "well-nigh verbatim" and then ascribed them to "common practise."[50]

The sensation caused by Ould's work is revealed by the publication later in 1742 of a pamphlet which was produced for the sole purpose of criticizing Ould's purportedly dangerous ideas. This work, entitled *Remarks on some of the Errors, both in Anatomy and Practice, Contained in a late Treatise of Midwifery, published by Fielding Ould, Man-Midwife,* was written by a neighboring Irish physician and man-midwife, Thomas Southwell. Apparently, Southwell considered this pamphlet to be a preliminary edition of his ideas. A more comprehensive critique, which he published in 1744, was of similar magnitude to Ould's original text. This book was entitled *A Continuation of Remarks on Mr. Ould's Midwifery; shewing His Errors in Anatomy, the Danger and bad Consequences attending his Practice and Manner of Deliveries*, by Thomas Southwell, M.D. and Accoucheur. Southwell criticized the anatomical descriptions in Ould's *Treatise* but failed to notice Ould's greatest error: thinking that there was a direct vascular connection between the mother and fetus. Nor did he correct Ould's belief that retained placental fragments could cause, among other things, cancer and gangrene. Southwell did subscribe to Deventer's antiquated theory of the obliquity of the uterus. He mocked Ould as "the youngest surgeon practising midwifery in this city," but Spencer refers to Southwell's work as the cavillings of a rival practitioner.[51] Ould received much criticism from Southwell and others over his novel view that there should be consultation between doctors in difficult cases since this is "too great an Undertaking, for any one Man singly to resolve on."[52]

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[49] Alfred H. McClintock (ed.), *Smellie's Treatise on the Theory and Practice of Midwifery*, vols I & II (London: New Sydenham Society, 1876), p. 238.

[50] Irving S. Cutter and Henry R. Viets, *A Short History of Midwifery* (Philadelphia: W. B. Saunders, 1964), 29.

[51] Spencer, *op. cit.* (n. 31 above), p. 38.

[52] Ould, *op. cit.* (n. 16 above), p. 38. When his (the obstetrician's) "Thoughts are confused, perhaps by the Loss of his Rest, hard Labour, being four and twenty Hours or more in the same Chamber, hearing the Cries and Moaning of the Patient, and being obliged to answer the many absurd Questions of the By-standers, and in short,

In accord with contemporary critics of the 1700s, modern historians have been quite harsh toward Ould, often neglecting even to mention his name, or else disclaiming his importance. The two major historians of the Rotunda addressed Ould's significance directly but reached different conclusions. Kirkpatrick wrote in 1913 that "it is difficult to think that, were he the brilliant investigator some would have us believe, he could have lived so long without producing any other works."[53] On the other hand, in 1947, Browne praised Ould as a pioneer and as one of the great Masters of the Rotunda.

From the present perspective, Ould's life and work represent a major force in the progress from haphazard midwifery to modern obstetrics. The events of his life parallel and exemplify the transitional role of the physician-midwife in the gradual refinement of the present-day obstetrician-gynecologist.[54] In a sense he established the original legitimacy of such a specialist. For these reasons, Sir Fielding Ould must be esteemed in the vanguard of this new class of practitioners dedicated to the establishment of a scientific field of medicine subject to rational empiricism and deductive logic.

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by the great Distraction that must be the Result of the whole Affair; in this Case I say, it is hardly possible that a Man can have the distinct Exercise of his reasoning Faculties, at this Time so particularly requisite; if a second Person free from these Inconveniencies agrees in his Opinion, his Mind and his Conscience are at ease. and his Reputation remains free from Censure," *ibid.*, p. 143.

[53] Kirkpatrick and Jellet, *Book of the Rotunda*, p 60.

[54] The term midwife or accoucheur was directly replaced by Michael Ryan in 1828 with "obstetrician," a term originally designating the male practitioners of midwifery See T R. Forbes, "The regulation of English midwives in the eighteenth and nineteenth centuries," *Med Hist.*, 1971, *15*: 352-362. The controversy between male and female midwives developed following the general introduction of the forceps and other operative tools which were monopolized by male practitioners and repudiated by female midwives who also resented the impropriety of men in the delivery room. The success of operative obstetrics led to the prominence of the male practitioner

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# "TO ONE COMMON END": THE UNITED STATES PUBLIC HEALTH SERVICE AND THE MISSOURI CHILD HYGIENE MOVEMENT, 1919-1921

PETER ROMANOFSKY

Events related to World War I made Americans increasingly concerned about the physical and medical conditions of the children of the nation. Draft boards rejected large numbers of young men because of defective physical conditions; and the tragedies of war, including starvation, malnutrition and disease, were demonstrated in the widely read accounts about the children of war-torn Europe.[1]

Even before the American public became sensitized to child health conditions, some doctors, nurses and rural welfare workers observed the disturbing trend toward malnutrition, physical defects and disease among the children of rural America. True enough, the urban slum and tenement conditions, which turn-of-the-century reformers studied and depicted in books like Jacob Riis's *How the Other Half Lives* and John Spargo's *The Bitter Cry of the Children*, had not been mitigated completely. But city boards of health, a growing number of special divisions of child hygiene and well-organized urban voluntary agencies became increasingly active—and successful—in promoting public health. Indeed, as the noted authority Dr. Thomas Wood of Columbia University pointed out in 1916, the rural superiority over cities in child health had been reversed dramatically in the last ten years. "The rural community, with all its fresh air and sunshine," another student of rural conditions stressed in 1916, "is the *most unhealthful* place in which to live."[2] And while organized sanitary movements, particularly the activities of the American School Hygiene Association, focused on urban schools, country schoolhouses remained particularly unhealthy.

In both Kansas City and St. Louis, Missouri, enterprising private and

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[1] See e.g. Dr. Charles Dalton, "Relation of state health departments to federal and volunteer agencies," *Pub. Health Rep.*, August 5, 1921, *36* 1795; Dr. Anna Rude, "Status of state bureaus of child hygiene," *Am. J. Public Health*, October 1920, *10* 779; Missouri Superintendent of Schools, *71st Annual Report* (1920), pp. 13-15; Albert Deutsch, "Some wartime influences on health and welfare institutions in the United States," *J Hist Med & All Sci.*, April 1946, *1:* 326.

[2] William J. Bray, "A practical application of school surveys," American School Hygiene Association, *Proceedings of 9th Congress* (1916), pp. 173, 168. My emphasis.

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public health and welfare agencies safeguarded the health of urban children. Throughout much of the second decade of the twentieth century, St. Louis, the fifth largest city, led all other major cities with the lowest infant mortality rate, an accomplishment which even the pioneering Division of Child Hygiene in New York City envied. And because of the diverse and constructive health activities in Kansas City, many national authorities recognized it as one of the healthiest cities in America.[3]

Child health conditions in rural Missouri, however, were strikingly poor. In Branson, where animals roamed the streets, "an utter lack of any attempt at sanitation" threatened the health of children. Few rural counties had health boards, and even the existing ones were very ineffective.[4] Studies showed the infant mortality rate of rural Missouri to be "very much higher" than in cities. "Mothers in the country districts," one report found, "have no opportunity whatever to learn even the simplest things about the care of the baby." Conditions in rural Missouri schools were "notoriously bad," with, for example, open, infected toilets close to the drinking water supply.[5] One study of rural schools in northeast Missouri showed that only 1.6 percent of the school toilets used disinfectants; even the state School Board argued in 1918, "A large per cent of the [rural] schools are culture spots for disease." Another, intensive study of child health in six rural Missouri counties concluded that "country children are distinctly worse off than city children."[6] Little wonder, then, that a group of experts appointed to study all aspects of Missouri childhood—including education, child labor and maternal hygiene—stressed in 1916, "No one feature of child-life [*sic*] in Missouri today needs regulation and supervision more than the health of infants and children in rural areas."[7]

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[3] Dr. Taliaferro Clark (hereafter Dr. Clark) to Surgeon General, Washington, D.C (hereafter D C.), November 20, 1920, Box 337, Division of Child Hygiene, file 3610, General Files, 1897-1923. Records of the United States Public Health Service, Record Group 90, National Archives (D C ) Since all citations from this record group are from the General Files, 1879-1923, hereafter other documents from the files of the Division of Child Hygiene will be cited as file 3610, RG 90, with the appropriate box number, American Child Health Association, *The Little Children of Our Great Cities* (New York, 1925), pp 41-45, St Louis Health Commissioner, *39th Annual Report* (1916), p. 13, New York City, Department of Health, *Annual Report, 1919* p 134

[4] Dr Clark to Dr Schereschewsky, D C , February 3, 1920, Box 336, file 3610, RG 90; Louise R Cahill to Julia Lathrop, Branson, February 23, 1915, Box 37, 4-15-3-5-1, Central Files, 1914-1920, Records of the United States Children's Bureau, Record Group 102, National Archives (hereafter the Records of the Children's Bureau will be cited as RG 102); one Missouri doctor felt in early 1919 that county boards of health were "more interested in sanitation among livestock than human subjects." Dr G. W. Vinyard in "State Summaries," 1919, Box 220, Division of Rural Sanitation, file 2240, General Files, 1897-1923, RG 90 (since all other materials from the Division of Rural Sanitation are also from the General Files, 1897-1923, this source will be cited as file 2240, RG 90, with the appropriate box number )

[5] T R Luckett to Surgeon General Blue, Sedalia, Missouri, August 15, 1919, Box 221, file 2240, RG 90, Arthur C Moreland to Surgeon General Blue, Butler, Missouri, August 14, 1919, *ibid.*; Missouri Children's Code Commission, *Report A Complete Revision of the Laws for the Welfare of Missouri Children* (Jefferson City, 1918), p 71 (hereafter cited as *Report, 1918*)

[6] Elizabeth Moore, *The Rural School Health Survey in Missouri* (St Louis, 1922), p 29; Missouri Superintendent of Schools, *69th Annual Report* (1918), p 60

Missouri health officials, social workers and other concerned people were aware of such conditions and worked to improve them. In 1915, a coalition of urban reformers, health and welfare workers and faculty members of the University of Missouri pressured the Governor to appoint a Children's Code Commission to study the condition of children and to propose legislative reforms. Composed of prominent child welfare activists like Roger Baldwin, the St. Louis Juvenile Court officer, for three years the Commission conducted investigations, solicited information about the children and held study seminars, seeking to understand the quality of children's lives and therefore to recommend proper legislation. The Commission concluded that even though 65 percent of its population was rural, Missouri had shamefully neglected its rural children. Well-organized agencies protected the health of city children, but, the Commission stressed in 1918, "Little has been done to improve the sanitary conditions in the rural communities and practically nothing [has been done] in educating mothers in the proper care of infants."[8]

The Children's Code Commission urged the legislature to establish a Division of Child Hygiene. The Commission hoped that this new agency would, for example, conduct educational and demonstration programs, reducing the death rate of rural infants, promoting the hygiene of pregnant women and heightening an awareness of basic nutrition principles. The 1917 session of the General Assembly rejected the proposal, and in 1919, seemingly more concerned with protecting the health of pigs and cows, it moved slowly on the bill. Only after last-minute pressure from Missouri women's groups and from health officials attending the sessions in Jefferson City did the General Assembly pass the bill establishing the Division of Child Hygiene. The legislature, however, failed to appropriate funds for the operation of the Division, frustrating temporarily the plans of Missouri health officials, who moved quickly to activate the newly established agency. Led by the Executive Secretary of the Missouri Tuberculosis Association (MTbA), Dr. Walter McNab Miller, and officials from the state Board of Health, they urged the Acting Governor to request the assistance of the United States Public Health Service (PHS) in beginning the work of the new state agency.[9]

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[7] Missouri Children's Code Commission, *A Complete Revision of the Laws for the Welfare of Missouri Children* (N. p., 1916), p. 54 (hereafter cited as *Report, 1916*). For further evidence of urban superiority, e.g. Ida Curry, "Illegitimacy in Eighteen Counties of New York State," U. S. Children's Bureau, *Illegitimacy as a Child Welfare Problem*, part 3, Publication #128 (D. C., 1924), pp. 164-165 and Helen Bary, "The trend of child welfare work," *North American Review*, April 1921, *216* 497

[8] Missouri Children's Code Commission, *Report, 1918*, p. 15; Dr. George Jones, "Recommendations," in Missouri State Board of Health, *35th Annual Report* (1917), p. 8.

The invitation came at an opportune time for the PHS, which was competing with other private and federal groups to coordinate the national child hygiene movement. During the progressive era, the development of both the United States Children's Bureau and the American Child Health Association, and the plans to organize other national health agencies, like the National Child Health Council, diminished the PHS influence over child hygiene work. PHS physicians argued publicly and privately that the Service should seize any opportunity to lead the child hygiene movement. Trying to elevate the stature of the PHS in 1919, the head of its Division of Child Hygiene, Dr. Taliaferro Clark, urged publicly all other child health organizations to work through his office in the campaign. Denigrating the private agencies, another PHS doctor argued, "[Voluntary organizations are] essentially temporary in character and . . . their activity is likely to be sporadic. If we permit the public to rely upon private efforts . . . in public health work," he predicted, "we may count on it that progress will be slow and difficult in the future." And in May 1920, PHS physician Dr. Lydia Allen deVilbiss argued that child hygiene work constituted "the keenest entering wedge we have for the entire public health program."[10]

As expected, the PHS responded positively to the Missouri request, sending Assistant Surgeon General Dr. Carlisle P. Knight and a team of experts, including physicians, field investigators, nurses and an administrative staff, to help develop the state Division of Child Hygiene. In October of 1919, Dr. Knight and his staff arrived in the Jefferson City offices of the state Department of Health, characteristically assigned to the basement of the State House while the Department of Agriculture maintained plush offices on the main floor. Nevertheless, the PHS was "definitely committed to the [Missouri] work and [felt] it to be of the greatest importance."[11] The PHS hoped fundamentally to improve child hygiene and health in the state. Through its educational and demonstration campaigns, for instance, it planned to register more births, to teach mothers the essentials of maternal and infant hygiene and to convey to school children and their parents basic nutrition principles. Central to the entire program, the PHS sought to initiate permanent local health projects which local citizens would maintain and develop further, especially baby, children's and community health centers. "The more [community] people . . . who become actively involved with the health center work," argued Dr. Knight at the beginning of the campaign, "the more likely it is to be [successful]."[12] Indeed, the Missouri movement became a test and a model for the rest of the country.[13]

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[9] Dr. George Jones to Surgeon General Blue, Jefferson City, August 18, 1919; Dr. Walter McNab Miller to Blue, St Louis, August 18, 1919, both in Box 336, file 3610, RG 90; Arthur Straus to Mr Hopkins, Inter-office Letter, December 27, 1919, American Lung Association Archives (New York City). See also, Peter Romanofsky, " 'The public is aroused': The Missouri Children's Code Commission, 1915-1919," *Missouri Historical Review*, January 1974, *68*: 204-222

[10] Dr deVilbiss, "[Discussion-Child Hygiene]," Annual Conference of State and Territorial Health Officers with the United States Public Health Service, *18th Transactions* (D. C , 1921), p 82 (hereafter cited as ACSTH-PHS), Dr Clark, "Child Hygiene," *ibid* , *17th Transactions* (D C., 1920), p 59; Dr. Drake, "Nation-Wide State Welfare Program," *ibid* , *passim*, esp p 62, Dr Schereschewsky was annoyed that the PHS was assigned a minor role in the implementation of the Sheppard-Towner Act, which provided federal aid to states for promoting maternal and infant health and welfare *Ibid., 18th Transactions* (D C., 1921), p. 58

Early in the campaign, PHS officials decided that its activities would take place in localities demonstrating the best promise of local cooperation and of establishing their own facilities once the Service initiated the campaign. For example, in Springfield, a group of public-spirited citizens organized the Greene County Health Association in the fall of 1919, displaying the kind of local enthusiasm which led the PHS to select Greene County as one of five demonstration areas. In such areas, the PHS planned to conduct intensive investigations of the children to determine the precise health needs of each locality; to conduct remedial nutrition clinics; to promote the development of general health and specific baby clinics; and to introduce other kinds of child and maternal health activities.

State-wide and local organizations contributed importantly to the child hygiene campaign. The MTbA, under the leadership of Dr. Walter Miller and his wife, detailed its field agents to help, particularly in the publicity efforts. The Association also prepared and distributed Knighthood Health Charts, which school children used in child hygiene instruction. The Agricultural Extension Service of the University of Missouri assigned its home demonstration agents, home economists and nutrition specialists to conduct programs in rural towns and villages. "Looking forward to newer activities," the state Women's Christian Temperance Union donated the use of its multigraph and developed the "Big Sister to Expectant Mother" program, giving women maternity outfits and health instructional kits.[14] In the school health campaign, local circles of the Missouri Congress of Parents and Teachers volunteered their services, helping local doctors and nurses weigh, measure and examine the children. Branches of the American Red Cross provided a corps of well-trained public health nurses to communities. Many of them remained to head and to conduct Red Cross-sponsored health centers. As early as September 1919, an official noted correctly "the ardor of these [Missouri voluntary agencies]."[15] Community health committees, particularly the Greene County Health Association, like scores of individual doctors, dentists and nurses throughout the state, assisted energetically. And the state Board of Health, along with the MTbA and the Agricultural Extension Service of the University, distributed thousands of pamphlets, including such publications as "Nutrition for Children" and the "Missouri Baby Book." Indeed, as Dr. Clark argued in 1920, the Missouri campaign was "distinctive" because it not only led to permanent local health work but because it also pioneered in producing smooth cooperation and coordination between private and public groups.[16]

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[11] F. Munroe to Dr. [Livingston] Farrand, February 14, 1920, in Red Cross Archives, D C (hereafter cited as RC Archives, D C ).

[12] Dr. Knight, "Child hygiene in Missouri," *J. Missouri State Med Assn.*, January 1921, *18* 19

[13] "The establishment and conduct of child health centers," *Pub. Health Rep* , July 30, 1920, *35*· 1795-1798, *Kansas City Star*, January 18, 1920; *Springfield Daily Leader*, February 11, 1920.

Private and public agencies combined effectively, for example, in the campaign in Carthage, in nearby Webb City and in rural Jasper County. In the middle of January 1920, officials of the campaign conferred in Carthage, the Jasper County county seat, both to launch the child hygiene efforts and to establish a Red Cross (RC) center in conjunction with the survey. Dr. Thomas Parran of the PHS, stationed in vicinal Joplin as the head of the Rural Sanitation Survey of Jasper County; a representative of the Southwest Branch of the American RC; and a graduate nurse with RC training drafted plans to conduct a house-to-house survey to determine the community and child health needs. Lutie Burkholder, a home demonstration agent from the Agricultural Extension

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[14] Dr. Knight, "[Discussion and Paper-Child Hygiene]," ACSTH-PHS, *18th Transactions* (D. C , 1920), p. 85; Dr Clark to Dr. Schereschewsky, December 18, 1919, Box 336 file 3610, RG 90, Dr Knight to Dr. Clark, Jefferson City, October 26, 1920, Box 337, *ibid.*

[15] Dr Clark to Dr. Schereschewsky, D. C., September 13, 1919, Box 222, file 2240, RG 90

[16] Dr. Clark to Dr. Schereschewsky, D. C., February 3, 1920, Box 336, file 3610, RG 90, "Child hygiene in Missouri," *Pub Health Rep.*, June 18, 1920, *35:* 1454; Knight, "Child hygiene in Missouri," p 18

Service, directed the survey. To prepare herself to assist in the canvass, a local woman who was the Secretary of the Carthage RC Home Bureau resigned in the middle of January 1920 to attend a training institute under PHS auspices. Another local nurse with qualified training and experience helped in the house-to-house survey.[17] The first such canvass in the state, it aimed for the conservation of children and the establishment of health centers for the cure of defective and undernourished children. "The prevention of disease is [also] our purpose," added Dr. Parran.[18] Similar surveys in Carthage, Joplin and then rural Jasper County followed the initial Webb City study.

Cooperation became evident quickly. Although a 1919 Children's Code Commission-sponsored law called for counties appointing full-time health officers, Jasper County had not designated one by late February 1920. Emphasizing a depleted treasury, county officials suggested that to save funds they might even delay the appointment until late May. Dr. Parran of the PHS wanted to help, offering himself as a county health officer for four days per week if the county would provide him with a motor car so he could spend the bulk of his time in rural areas. Although recognized as futile because the law required a full-time health officer, the incident suggests the degree of cooperation between federal and local agencies "to one common end": the improvement of child health in Missouri.[19]

Community agencies and people gradually took over the PHS-initiated campaign. PHS surveys continued in early 1920, but real community efforts in Carthage began earnestly in April with the appointment of a local doctor as the County Health Commissioner; two other physicians, one each from Webb City and Joplin, became his official assistants. The Carthage newspaper and others anticipated a more active child health movement. The local RC installed a new public health nurse, Miss Grace Rogers, to help open its clinic, which served the child hygiene and other health campaigns. In order to continue the work of the PHS, the county court contributed $3,000 to the health center. In June, the head of the state Division of Child Hygiene, Dr. Knight of the PHS, came to Carthage to confer with local and state RC officials about the campaign.[20]

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[17] *Carthage Press*, January 15, 1920.

[18] *Ibid.*

[19] Dr Clark, "[Child Hygiene Activities of the PHS]," ACSTH-PHS, *18th Transactions* (D. C., 1921), p. 63, *Carthage Press*, February 26, 1920.

[20] *Carthage Press*, June 17, 24, April 1, 1920.

Fulfilling a goal of the campaign, the Carthage RC established and conducted its own child hygiene program. On the second and fourth Mondays of each month, RC nurse Grace Rogers held a general clinic in the RC headquarters, and every Friday afternoon she conducted the baby clinic in Carthage. Proving to be an "excellent nurse," Miss Rogers attracted increasing numbers of mothers to the clinics, helping to implement the PHS hope that rural women become aware of the importance of hygiene. In early September 1920 she attended the community's Old Settlers' Picnic, weighing and measuring 200 children and asking all the mothers to bring their children to the clinic. The next clinic, which an enthusiastic young doctor conducted, was "well attended." Between June 14 and September 30, 1920, because of Miss Roger's efforts, the RC cared for 178 cases, and 63 well babies were under her supervision.[21]

Miss Rogers and the Carthage RC also developed clinics in previously neglected rural areas. Rural Sarcoxie held a baby clinic on the first and third Tuesdays of each month at a local church, there being no RC or other medical facility in the small community. Three local doctors voluntarily rotated their services for the clinic. A local woman conducted the clinic and Grace Rogers, the energetic RC nurse, attended and assisted, giving dietary and nutritional information to three mothers who attended the first clinic. In the rural Madison Township home of a volunteer, Miss Rogers conducted the initial first-aid and baby clinic in the middle of July. Thirty women attended, prompting the observation, which others repeated later, "The Madison township[RC] club is very enthusiastic . . . and is one of the banner RC . . . organizations in Jasper County." In Madison Township, the August 11 baby clinic, at which Miss Rogers examined five infants, served another function as well: 50 women from the Community Club came to the clinic, where a local physician instructed them in repairing broken arms.[22] Such local efforts helped make Madison Township the "best community [RC] organization [one worker] ever saw"; 60 women often attended the clinics. The group even planned a play "to get the [community] people together" and to inform them about the RC work. To develop another baby clinic in the rural hamlet of Jasper, Missouri, the Secretary of the Carthage RC and Grace Rogers drove the ten-odd miles to hold it in a room on the second floor of the general merchandising store of a volunteer. The clinic in

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[21] *Ibid*., October 7, July 15, 8, 1920, Saidee Haussmann, "[Report-Carthage, Missouri]," n. d. [September, 1920], RC Archives, St. Louis

[22] *Carthage Press*, August 15, July 15, 1920.

Jasper met the second and fourth Wednesdays of each month. By September 1920 the untiring Grace Rogers also established a similar infant clinic in Stotts City, a rural village in adjacent Lawrence County in southwest Missouri.[23]

Across the state, in southeast Missouri, particularly in the Cape Girardeau area, similar PHS initiative and local cooperation produced meaningful child hygiene programs. Both the MTbA and the Cape Girardeau County Health Society helped in the examinations of the school children, which began in the middle of February 1920. Dr. Lydia deVilbiss of the PHS, a prominent physician who worked in other areas of the state, supervised the inspections. Dr. deVilbiss said in February 1920 that Cape Girardeau mothers cooperated more ardently than at any other locality she had observed. And, in conjunction with the examinations in another school district, PHS's Dr. Viola Russell and two of her field workers drove through the area to survey the condition of the children's homes.[24]

Characteristically, PHS agents in southeast Missouri worked energetically to develop programs throughout the area. For example, after supervising the examinations in Cape Girardeau, Dr. deVilbiss left to found the program in rural Farmington, some 55 miles away. And, while she was conducting investigations in Cape Girardeau, Dr. Russell traveled in March and April to initiate programs both in Byrd Township and in Jackson, where PHS Assistant Surgeon and head of the Missouri Division of Child Hygiene, Dr. Carlisle P. Knight, also spoke at a mass community meeting.[25]

Another campaign, in Lexington in the middle of the state, demonstrated how the initiative of the PHS revitalized community health work and developed constructive new programs. In February 1920, increasingly inactive and divided internally, the Lexington chapter of the RC "decided they were dead" and even requested the regional office to close the chapter. The PHS campaign, however, changed the situation radically. It opened in early March with an enthusiastic meeting of local doctors and others. At the gathering, Professor Homer T. Phillips of the State Normal College at Warrensburg and a representative of the MTbA, explained the PHS plans. After some further local discussions, PHS agents came to Lexington and began immediately to implement its campaign. On May 1, 1920, the PHS was instrumental in opening the Lafayette County Health Center, located in the Lexington RC Building. The revitalized RC group, especially its Executive Secretary, cooperated, but Emma Brown of the PHS did the real work. The RC "could not in year have made the progress which was made possible . . . [by] Miss Brown."[26] Although nearly defunct three months earlier, the RC even planned to conduct PHS-inspired nutrition clinics at the Health Center using one of its own workers.[27]

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[23] Haussmann, "[Report]," 1920; "Inter-office Letter," both in RC Archives, St. Louis, *Carthage Press*, July 8, 1920. The Haussman report states that Miss Rogers also developed clinics in Centerville, which is more than 200 miles from Carthage over country roads. I have not, however, verified this statement.

[24] *Cape Girardeau Southeast Missourian*, February 27, 20, 1920.

[25] *Ibid*., March 5, 19, April 23, 1920.

The PHS examinations of the Lexington school children, begun in early May 1920, solidified local cooperation. A PHS doctor and an assistant conducted the work, but the Lexington Women's Club and the MTbA helped. A nutrition specialist from the Agricultural Extension Service of the University held the nutrition clinics. Praising both the school examinations and the local agencies participating, the Lafayette County Superintendent of Schools concluded in mid-1920 that the PHS "is doing some very effective work in a number of places in the county."[28] The first general clinic at the RC office in late May 1920 "was a splendid success," as Miss Brown of the PHS donated so much time and the RC assisted. The RC office quickly became "one of the busiest places in town."[29]

PHS agents left Lexington in June 1920, but they had initiated "one of the most constructive pieces of work ever accomplished in Lexington." As the PHS hoped, local groups assumed the health work; the RC and local physicians continued to conduct the general clinic, and the RC even extended the child hygiene program throughout Lafayette County, cooperating, for example, with women in rural Odessa in August 1920 to establish a new public health center.[30]

A similar but even more energetic and locally created child hygiene campaign developed in Greene County, in southwest Missouri, where a coalition of "public spirited citizens" organized a County Health Association in June of 1919. Although the Association dedicated itself also to tuberculosis and venereal disease work, it concentrated on the health of local children. Throughout the summer and fall of 1919, organizational and public meetings abounded with commitments to child hygiene. For instance, the chairman of the organization urged in November 1919, "Let us take up the cudgel and fight for the lives of your children and mine."[31] Within months after its establishment, even national health figures like Professor Thomas B. Wood of Columbia University and Dr. Merrill E. Champion, the Director of Hygiene in Massachusetts, recognized the vigor of the Association. The enthusiasm and promise of the group also influenced officials of the child hygiene campaign to select Greene County as one of its demonstration areas.[32]

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[26] "[Report-Lexington, Missouri]," June 19, 1920, Edith Stringer, "[Report-Lexington]," February 29-31 (?), 1920, both in RC Archives, St Louis; *Lexington News,* March 4, 1920

[27] "[Report-Lexington]," June 19, 1920, *ibid* , *Lexington News,* June 9, 1920

[28] Elmer T. White, "Report," Missouri Superintendent of Schools, *71st Annual Report* (1920), p 104; on continued PHS support, see e g., Dr Knight to Dr Florence McKay, Jefferson City, April 14, 1921, Box 179, 4-11-1-3(27), Central Files, 1921-1924, RG 102; *Lexington News,* May 6, 1920.

[29] *Lexington News,* June 6, 10, May 27, 1920.

[30] *Ibid* , August 5, 19, July 1, 1920; *Important Items,* IV (June 7, 1920), p. 4

Local agencies, however, actually initiated the movement. The County Health Association and others, for instance, conducted an "extensive campaign of publicity and education . . . especially in the rural districts." And Grace Dulaney, the county home economics agent from the Agricultural Extension Service of the University, worked to have rural children drink more milk. She began by circulating information about the drive, urging cooperation and describing the best bottles for milk and how to purchase straws cheaply. She contacted every rural school in the county, and by early December 1919 a number of them pledged cooperation. Miss Dulaney traveled throughout Greene County, demonstrating the importance of drinking milk. Within a few weeks, her work showed success, as perceptibly more rural children were drinking milk. And by early December 1919, 70 local doctors and 50 nurses agreed to spend at least 120 hours each examining Greene County school children.[33]

The most crucial aspects of the campaign in Greene County, as elsewhere, were the medical examinations in the schools and the nutrition and baby clinics. Ever since its inception, the Greene County Health Association had been planning to examine the school children and to develop such clinics. All the Springfield circles of the Congress of Parents and Teachers (CPT) resolved to supply scales in every school, making the health crusade an integral aspect of their work. With the appointment of a full-time health officer in the county in December 1919, and with the inclusion of Greene County in the PHS plans, the campaign awaited the arrival of a team of federally financed child hygiene experts.[34]

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[31] *Springfield Daily Leader,* November 18, May 28, 1919 (hereafter cited as *Leader*), Dr Thomas Parran to Surgeon General, Joplin, Missouri, December 12, 1919, Box 222, file 2240, RG 90

[32] Dr L. Lumsden to Congressman Major, in *Leader,* December 26, 1919, Lumsden to T. P [Dr Parran], D. C , December 16, 1919, Box 222, file 2240, RG 90; "Pioneer Work Being Done By Health Association," *Leader,* December 28, 1919.

[33] Dr. Parran to Surgeon General, Joplin, December 12, 1919, Box 222, file 2240, RG 90, *Leader,* December 1, November 5, 1919; Extension agents conducted similar milk campaigns in other rural Missouri areas University of Missouri College of Agriculture. Agricultural Extension Service, *Annual Report, 1920,* Project Announcement #13 (Columbia, 1921), p. 14.

Anxious to implement its plans, the County Health Association, however, initiated the school examinations in January 1920; a PHS nurse did not arrive until late February. At the Boyd School in Springfield, two local physicians and two nurses examined the children for eye strain, nose, throat and hearing defects, discharging ears, skin and scalp diseases and other deformities. They also studied the children's nutritional habits and looked for both nervous disorders and tooth decay. Physicians conducted similar examinations at three rural schools. Early reports showed high incidences of impaired vision, decayed teeth, enlarged adenoids and tonsils. Throughout February and March, the examinations continued, taking about two days to complete at each of the city schools and only one day at each of the rural ones. By the end of March, the Association-led team examined over one thousand children in the Springfield schools as well as hundreds in the rural areas of the county.[35]

Because parents cooperated fully, the nutrition clinics at the various schools proved particularly beneficial for the children. At the Boyd School in the city, for example, the CPT circle held a well-attended meeting with a home economics specialist from the University, Anna Jensen, who came to Springfield after establishing similar clinics in Jefferson City, Joplin, Carthage and Cape Girardeau. At the Boyd School, 25 students from the 5th and 6th grades, along with their mothers, attended the first nutrition clinic, where specialists pointed out the children's defects and prescribed remedies to the mothers. At the second clinic a month later, 90 students participated. Proper diets and full parental cooperation meant that a large number of the children found to be underweight gained between one-half and seven pounds; a few pupils lost weight, but they had been sick or had tonsil or adenoid operations. At another school, over 100 children gained between one-half and eight pounds, with an average of 2.1 pounds gained per child; 13 students remained the same weight and eight lost less than one pound. Sixty mothers had attended the previous clinic and they cooperated fully. The children's increasing use of milk was the most noticeable accomplishment. Commenting particularly on the successes of Missouri nutrition clinics, like these in Springfield, the Director of Child Hygiene of the PHS wrote in August 1920, "The investigations . . . in Missouri . . . have shown that definite and permanent results can be accomplished." Miss Dulaney, the county home economics agent, emphasized that the parents and teachers deserved credit for the success of the clinic at the second school.[36]

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[34] "Pioneer work"; *Child-Welfare Magazine*, September 1920, *15*: 12

[35] *Leader*, April 1, January 30, 28, 1920

The mothers' enthusiasm was evident elsewhere. At the Cave Springs School, the examinations showed ten of fifteen children underweight and Misses Dulaney and Jensen, both from the Agricultural Extension Service, urged the mothers of the ten children to come to the nutrition clinic. They did. At the clinic, Miss Jensen interviewed each of them, instructed each privately about nutrition and then spoke to the entire group. Following the clinic, the local women organized a circle of the CPT, elected officers and vowed further cooperation.[37] As officials had hoped, Missouri mothers showed ardent interest in the child hygiene movement.

Not all Missouri parents, however, displayed such excitement. In April, the County Health Association began a series of evening lectures which utilized charts and illustrations. Pupils in all the schools worked hard to get their parents to attend. The first such meeting, at a school in the northwest section of the city, featured a doctor active in the Association and another prominent pediatrician. Despite the organizers' enthusiasm, attendance was small. Irritated by this first disappointment, a representative of the Association recalled that during the house-to-house canvass, mothers in the northwest section claimed they knew the essentials of hygiene and nutrition, but they were surprised to hear that children should not eat too many biscuits. "Parents pay all sorts of attention to the latest in automobiles, clothes and even pure-bred cattle," claimed one frustrated Association member, "yet are criminally negligent when it comes to their own children." Those parents who attended, however, were happy with the knowledge they gained. A program at the Lincoln School revealed that Negro mothers were "much more vitally interested'' in child hygiene than were whites. Attending in greater numbers than the total of the two previous audiences, Blacks showed the keenest interest in lectures on general child hygiene and on preventive medicine.[38]

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[36] *Leader*, April 15, 1920; "Child Hygiene," in letter, Dr. Clark to Surgeon General, D. C., August 5, 1920, Box 337, file 3610, RG 90; *Leader*, April 8, March 10, 20, February 11, 1920.

[37] *Leader*, February 15, 1920; Knight argued that these nutrition clinics were especially interesting and beneficial, "[Discussion and Paper]," p. 85

Springfield mothers were interested actively in the special ''Baby Week,'' when community-wide cooperation was again evident. Mrs. William Ullmann, the Chairman of the Missouri CPT Committee on Child Hygiene, organized the clinics. A local doctor, PHS nurse Margret Duffey and a local nurse worked to help make the clinics, held on the sixth floor of Heer's downtown Department Store, successful. The Springfield newspaper publicized the baby clinics, and Missouri mothers participated eagerly. On the first day of the clinic, despite the bad weather, 50 children under six years old attended. Other local physicians volunteered their services and lectured to mothers on such topics as ''Child Care and Feeding.'' Mothers came from all over, with many coming from distant rural areas.[39] The week-long clinic showed that few youngsters had proper diets, but the demonstrations, lectures and exhibits taught the mothers much. Attendance was so large by the end of the week that Miss Duffey of the PHS organized a Little Mothers League for girls between ten and sixteen who cared for their younger brothers and sisters. Follow-up work for the underweight children took place in June at the department store clinic.[40]

The kinds of community-sponsored permanent health programs in Springfield, Cape Girardeau, Carthage and Lexington—and duplicated throughout the state—were what, from the beginning, the PHS tried to encourage. Local commitment was strong throughout the movement. Following a visit to Missouri in early 1920, the head of the Division of Child Hygiene of the PHS observed privately, ''The sentiment throughout the state is decidedly in favor of this [child hygiene] work.'' And, as another official said later, ''In all instances the movement has been very enthusiastically received by the public.'' Satisfied fully with the campaign and with local health activities, the PHS ended its Missouri commitment formally on June 30, 1921, but left behind some of its field workers. Although the delivery of services lagged slightly, ''so much favorable [public] sentiment has been aroused for better child health," reported Governor Hyde in October, 1921.[41]

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[38] *Leader*, April 14, 11, 1920 I am studying the movement to reduce Negro infant mortality in New York City, where Black mothers were interested similarly in child hygiene.

[39] *Leader*, April 27, 26, 21, 22, 1920; Editorial, ''Baby Week,'' April 27, 1920

[40] *Child-Welfare Magazine*, October 1920, *15:* 44; *Leader*, April 30, May 19, 1920. Interest in child hygiene in Springfield remained high See e. g., attachment to C. Dinwiddie to Dr. Anna Rude, D C , October 13, 1921, Box 177, 4-11-0-4, Central Files, 1921-1924, RG 102

Writing in 1923, the head of the Child Hygiene Division of the PHS outlined the future goals of the rural child health movement: full-time county health officers, more public health nursing, greater interest in child hygiene through school surveys and the cooperation of both parents and local social work and medical organizations. Although he did not mention it explicitly, all of these goals, and more, had been fulfilled in Missouri: the number of public health nurses in Missouri, for instance, leaped from three in January 1920 to 35 when the PHS left 18 months later, and community requests for such nurses could not be met. And, the PHS used the "potentialities of the voluntary organizations in Missouri with greater success" than in any other state.[42]

The baby clinics, the school examinations, the house-to-house surveys and the establishment of community health centers in Missouri demonstrated that community-wide cooperation could and did improve local health conditions. The Missouri child hygiene movement suggested also that World War I promoted rather than retarded social progress and that federal, state and local groups worked effectively, in the best progressive tradition, "to one common end."

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[41] Governor Hyde to Senator Spencer, Jefferson City, October 27, 1921, Box 338, file 3610, RG 90, Dr. Clark to F. Munroe, D. C February 12, 1920, RC Archives, D. C.; see also, Dr. George Jones to Dr Cumming, Jefferson City, July 3, 1920, Box 337, file 3610, RG 90

[42] Dr Clark, "[Child hygiene activities of the PHS]," p. 63; *J Missouri State Med Ass* (August 1921) 295, Dr. Clark, "The physical care of rural school children," *Pub Health Rep.*, 38 June 1, 1923, 1189-1190

# AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE REPORT OF THE FIFTY-FIRST ANNUAL MEETING

## Kansas City, Missouri
## May 10-13, 1978

The Fifty-first Annual Meeting of the American Association for the History of Medicine was held in Kansas City, Missouri, May 10-13, 1978. Sessions were held at the Radisson-Muehlebach Hotel. Preceding the meeting, two continuing education seminars (6 hours of Category 1 credit), sponsored by the University of Kansas Department of the History and Philosophy of Medicine and the Division of Continuing Medical Education in co-operation with the American Association for the History of Medicine, were offered: "Health and Healers in American History," Gert H. Brieger; and "The Anatomy of Medical Books," G. S. Terence Cavanagh.

The following report, which includes the program, abstracts of the minutes of the Council and Business Meetings, as well as the financial report for fiscal year 1976-77, has been prepared by the Secretary-Treasurer, Dr. Charles G. Roland, and is intended for the information of the members of the Association. The official minutes and financial report are preserved in the office of the Secretary-Treasurer.

## PROGRAM

### Wednesday, May 10

5:00 p.m. Registration and Welcoming Reception
7:00 p.m. Council Meeting

### Thursday, May 11

9:00 a.m. General Session
  Robert P. Hudson, *presiding*
  Opening Remarks
  Presidential Address: "The American Medical Profession and Public Health: From Support to Ambivalence," John Duffy
10:30 a.m. General Session
  Gerald N. Grob, *presiding*
  "Recovery and Recidivism in Two Ante-Bellum Massachusetts Asylums," Barbara Gutmann Rosenkrantz and Maris Vinovskis
  "The Hospital: From Charitable Institution to Modern Medicine," Morris J. Vogel
  "The Rise of the Teaching Hospital and the Reform of Clinical Instruction in America," Kenneth M. Ludmerer

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12:15 p.m. Luncheon Roundtables
1. "What Should Be the Role of the AAHM Today?" Genevieve Miller, Gert H. Brieger, *Coordinators*
2. "Thomas McKeown: The Reliability of Medical Explanations in History," Lloyd G. Stevenson, Peter H. Niebyl,* *Coordinators*
3. "Reflections on a Life in Medicine," George W. Corner, *Coordinator*
4. "Teaching Medical Humanities: The Historian's Role," Todd L. Savitt, James B. Speer, Jr., *Coordinators*

2:00 p.m. Session A
James O. Breeden, *presiding*
"The Controversy over Medical Inspection in Boston's Public Schools, 1876-1894," Martha H. Verbrugge
"The Dreaded Scourge of Childhood: The Impact of New York City's Anti-diphtheria Campaign upon American Medicine," Lewis P. Rubin
Session B
Gerald L. Geison, *presiding*
"Gerhard's Distinction between Typhoid and Typhus and Its Reception in America, 1836-1860," Dale C. Smith
"Theory and Practice in Mid-Nineteenth Century Medicine: John Hughes Bennett's Molecular Theory of Organization," John Harley Warner

3:15 p.m. Session A
James Harvey Young, *presiding*
"The FDA's Regulation and Control of Antibiotics in the 1950s: The Henry Welch Scandal," Richard E. McFadyen
"Black Lung: The Political Consequences of Medical Uncertainty," Daniel M. Fox, Judith F. Stone
Session B
Luis H. Toledo-Pereyra, *presiding*
"The Reception of Antisepsis in Britain, 1867-1880," Lindsay Granshaw
"Antisepsis in the United States," Jonathan Liebenau

4:30 p.m. The Fielding H. Garrison Lecture
John Duffy, *presiding*
"An American Clerical Crisis: Minister's Sore Throat, 1830-1860," James H. Cassedy

Friday, May 12

9:00 a.m. Session A
Regina Markell Morantz, *presiding*

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* Dr Niebyl could not be present.

"Dr. Veit—Charlatan or Martyr to Obstetrics?" Thomas G. Benedek

"Confusion Factors in 19th Century Studies of Labor and Delivery," Dorothy I. Lansing, W. Robert Penman

"The Rise and Fall of 'Battey's Operation': A Fashion in American Surgery," Lawrence D. Longo

Session B

John C. Burnham, *presiding*

"Health, Diet, and the Medical Profession in the 19th Century," Janet Blackman

"Anthropometric Study of Human Growth in Nineteenth Century American Medicine," James A. Young*

"The Nineteenth Century Opiate Addict and What She has to Tell Us about the Nature of Opiate Addiction," David Courtwright

Session A

10:45 a.m. Jerry Stannard, *presiding*

"The Galenic Question," John Scarborough

"The Sceptic Physician against Euthanasia, 400 B.C.-200 A.D.," Peter Krivatsy

Session B

Nancy M. Frieden, *presiding*

"Plague Tractates as a Response to the Epidemic of 1770-72 in Russia," John T. Alexander

"Medicine and Bureaucracy: Jean Colombier's Regulation for the French *Dépôts de mendicité* (1785)," Thomas M. Adams

12:15 p.m. Luncheon Roundtables

1, 2, and 3 (repetition of those presented on May 11)

4. "Preserving the Past: Oral History," Gwendolyn Safier, *Coordinator,* with Saul Benison, Regina Markell Morantz, George Emil Erickson, and Peter D. Olch

2:00 p.m. "Neurology in New York: A Case Study in American Medical Specialism," Bonnie Ellen Blustein

"On the Evolution of Women's Medical Societies," Cora Bagley Marrett

3:15 p.m. General Session

Daniel L. Weiss, *presiding*

"The Adoption of German Medicine in Japan, 1870-1881," John Z. Bowers

"Patterns of Professional Development in 19th Century Ontario and Quebec," Charles G. Roland

4:30 p.m. Business Meeting

John Duffy, *presiding*

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* Not presented.

7:30 p.m. Annual Banquet
"The Witty World of Russell Baker," Russell Baker

Saturday, May 13

9:30 a.m. General Session: "William Harvey and His Age." A Symposium to Commemorate Two Harveian Anniversaries. Birth, 1578, and *De motu cordis,* 1628
Saul Jarcho, *presiding*
"The Medical Side of Harvey's Discovery. The Normal and the Abnormal," Jerome J. Bylebyl
"William Harvey and the Crisis of Medicine in Jacobean England," Charles Webster
"The Physician-Scientist as Hero: The Image of Harvey in Restoration England," Robert G. Frank, Jr.

## MINUTES OF THE ANNUAL MEETING OF THE COUNCIL

May 10, 1978

The annual meeting of the Council of the American Association for the History of Medicine, Inc., was called to order at the Radisson-Muehlebach Hotel, Kansas City, Missouri, at 7:30 p.m. on May 10, 1978. President John Duffy served as Chairman, and Charles G. Roland as Secretary. The following Council members and committee chairmen were present: Edward C. Atwater, Chester R. Burns, Thomas G. Benedek, John C. Burnham, William Coleman, Audrey B. Davis, Leslie Falk, Genevieve Miller, Wilhelm Moll, Ronald Numbers, Ynez V. O'Neil, Guenter B. Risse, A. Benedict Schneider, Lloyd G. Stevenson, Dora B. Weiner, and Richard J. Wolfe.

The minutes of the last meeting, as circulated to the Council in May 1977, were approved. An abbreviated version will be published in the Summer 1978 issue of the *Bulletin of the History of Medicine,* now in press.

The Secretary reported that the new *Membership Directory* will be distributed to members in July 1978, and that the fourth annual *Research in Progress,* to be compiled by James H. Cassedy, will be distributed early in 1979. Thirty members have applied for membership in the International Society of the History of Medicine.

Dr. Roland presented the Financial Report for the Fiscal Year 1976/77, ending 31 October 1977. Council approved his recommendation that $6,000.00 be transferred into the endowment fund. The report was approved for presentation to the Business Meeting, and the President appointed Richard J. Wolfe and William Coleman as Auditors. The Treasurer pointed out that the Association has no formal policy with regard to the accumulation or the use of its endowment funds. The Council requested that this entire question be investigated by the Treasurer before the next annual meeting. Dr. Roland commented that the membership increased, in the past fiscal year, from 918 to 956.

Dr. Duffy announced that James Harvey Young has been selected as Garrison Lecturer for 1979. The Chairman of the selection committee was Peter D. Olch

The William H. Welch Medal Committee (Lloyd G. Stevenson, Chairman) unanimously recommended and the Council approved by mail vote that the medal be given to Frederic Lawrence Holmes, of London, Ontario, for his book, *Claude Bernard and Animal Chemistry: The Emergence of a Scientist* (1974).

The William Osler Medal Committee (Thomas G. Benedek, Chairman) recommended and the Council approved the award of the 1978 Osler Medal to James Tait Goodrich, the class of 1979 at Columbia University School of Medicine, for his essay on "Sixteenth Century Anatomy and Andreas Vesalius: The Contribution of Renaissance Art to Modern Anatomical Studies."

The Loeb Library Committee (Saul Jarcho, Chairman) reported that Paul Potter, University of Western Ontario, London, is proceeding with the translation of volume V of the Hippocratic treatises for the Loeb Classical Library. The proposal that Dr. Wesley D. Smith, of Philadelphia, do volume VI has been temporarily delayed by administrative problems in Bethesda, MD.

The Association's Delegate to the International Society for the History of Medicine, Dora B. Weiner, reported that the first bilingual (French and English) issue of the Society's *Bulletin* was issued recently. The next International Congress is to be held at Plovdiv, Bulgaria, August 20-25, 1978. The 1980 meeting will be in Barcelona, Spain. Dr. Weiner was requested to continue her efforts to establish permanent bilinguality in the publications of the Society.

The Education Committee presented an interim report on the survey undertaken in the autumn of 1977 regarding the teaching of the history of medicine to undergraduate college students. Almost two-thirds of the questionnaires were returned, and it was discovered that courses are being conducted at seven Canadian and seventy-eight American universities. Considerable interest in beginning such teaching was expressed by other respondents. A method for making syllabi and other information available will be considered by the committee. A sum of $500.00 was approved by the Council to be used in pursuing the results of the study and making them generally available.

Invitations had been received for the Association to meet, in 1981, in Rochester, N.Y., and in Toronto, Ontario. After discussion the Council accepted the invitation from Toronto.

Several items relating to publications were discussed at length.

(a) It was pointed out that our existing contracts with the Johns Hopkins University Press for Supplements to the *Bulletin* contain no provision for the payment of any royalty to the author or to the Association. This came to light in conjunction with the contract signed recently for the Supplement *Almost Persuaded,* by Ronald Numbers. The Council empowered the President and the Secretary-Treasurer to negotiate with the Press an agreement to ensure that, for any second printing of volume one of *Almost Persuaded,* and all printings of all future volumes of *Bulletin* Supplements, a royalty clause is included in the contract. The clause should specify that any royalty paid would be divided equally between the author(s) and the Association.

(b) Lloyd Stevenson initiated a discussion of the possibility that the Harvey Symposium, part of the program of the annual meeting for 1978, be published as a Supplement to the *Bulletin.* The Council approved this project subject to the agreement of the Editorial Committee and provided that subvention expense not exceed $2,400.00.

(c) After some discussion, Dr. Stevenson was requested to investigate the possibility of issuing a cumulative index for the *Bulletin* to encompass the period since 1962, when the last index appeared.

(d) The Council recommended to the editor of the *Bulletin* that the Association's By-Laws be published in the near future. Many changes have been made since the date of last publication—1958.

The generous donation, by James H. Cassedy, of his $500.00 honorarium as Garrison Lecturer, to the endowment fund of the Association, was acknowledged with great appreciation.

The Council meeting adjourned at 10:30 p.m.

Charles G. Roland
*Secretary-Treasurer*

## MINUTES OF THE ANNUAL MEETING OF THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE, INC.

May 12, 1978

The Annual Meeting of the American Association for the History of Medicine, Inc., was held in the Radisson-Muehlebach Hotel, Kansas City, Missouri, at 4:30 p.m. on May 12, 1978. John Duffy, President of the Association, served as Chairman, and Charles G. Roland as Secretary.

The minutes of the previous meeting were approved. Dr. Roland read the names of members who had died since last May. They are as follows:

Richard L. Butler, Fort Sill, Oklahoma
Mrs. Nora H. Guthrey, Stewartville, Minnesota
Emerson Crosby Kelly, Albany, New York
Chauncey D. Leake, San Francisco, California
George Rosen, North Haven, Connecticut
J. I. Waring, Charleston, South Carolina

The members rose for a moment of silence.

The Secretary-Treasurer presented a summary of the Financial Report for the Fiscal Year 1976/1977. The Auditing Committee (Richard J. Wolfe, Chairman) reported that the books were in order, and the reports of the Treasurer and the auditors were approved.

The Secretary-Treasurer also reported on membership. Overall membership has increased from 918 to 956. Membership in the International Society for the History of Medicine has declined from 124 to 93.

### *Old Business*

The President commented briefly on the activities of the various committees. Dr. Saul Jarcho, on behalf of the Loeb Library Committee, reported that work is proceeding with Dr. Paul Potter's translation of the fifth volume of Hippocrates. Dr. Dora Weiner summarized her efforts, as the Delegate to the International Society for the History of Medicine, to introduce bilingualism into that group's publications and meetings.

*New Business*

Peter D. Olch, Chairman of the Garrison Lecture Committee, announced the 1979 Garrison Lecturer, James Harvey Young. Lloyd Stevenson, on behalf of the Welch Medal Committee, reported that the 1978 medal would be awarded to Frederic Lawrence Holmes for his book, *Claude Bernard and Animal Chemistry: The Emergence of a Scientist.* Chairman of the Osler Medal Essay Committee, Thomas G. Benedek, informed the membership that 6 essays were considered and that that of James Tait Goodrich, Columbia University School of Medicine, had been awarded the medal.

Chester R. Burns reported that the survey of courses in medical history given at the undergraduate level in North American universities had been productive and that full details would be available later.

The President announced that the next annual meeting of the Association will be held in Pittsburgh, Pennsylvania, from May 3-5, 1979, under the chairmanship of Thomas G. Benedek. Future meetings will be held in Boston (1980) and in Toronto (1981). The amendment to the By-Laws authorizing the appointment of a permanent Education Committee, having been circulated to all members previously, was adopted on vote of the membership at the annual meeting.

The following officers were elected unanimously:

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|---|---|
| President: | Genevieve Miller, Cleveland, Ohio |
| Vice President: | Gert H. Brieger, San Francisco, California |
| Members of Council: | Pauline Mazumdar, Toronto, Ontario |
| (3-year term) | Robert J. T. Joy, Chevy Chase, Maryland |
| | Arthur J. Viseltear, New Haven, Connecticut |
| | G. Grob, Bridgewater, New Jersey |

Peter D. Olch commented that many persons had been making audiotape recordings of sessions at the meeting, some at least without requesting the speaker's permission. It was recommended that such permission be sought routinely in future.

The meeting was closed at 5:15 p.m. by the new President, Genevieve Miller. Enthusiastic thanks were expressed to the Program Committee, chaired by Ronald Numbers, and to the Local Arrangements Committee, led by Robert Hudson, for the delightful meeting in Kansas City. A formal vote of thanks was tendered to John Duffy, whose term of office as President ended at this meeting.

Charles G. Roland
*Secretary-Treasurer*

## AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE, INC.

### Financial Report on the Fiscal Year 1976/77
### Ending 31 October 1977

### Classified Statement of Receipts and Expenditures
### 1 November 1976—31 October 1977

*General Fund*

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|---|---|---|
| RECEIPTS | | |
| Dues | | |
| Active Members (all categories) | $15,286.87 | |
| Non-Resident Members | 504.00 | |
| Constituent Societies | 550.00 | |
| International Society for the History of Medicine | 982.25 | |
| | | $17,313.12 |
| Directory and Mailing List sales | | 100.00 |
| Research in Progress sales | | 122.00 |
| Refund from Johns Hopkins University Press | | 1,056.25 |
| Transfer from Cleveland account | | 7.50 |
| 1976/77 Total Receipts | | 18,598.87 |
| Balance 1 November 1976 | | 7,292.36 |
| | | $25,891.23 |
| DISBURSEMENTS | | |
| Subscriptions to *Bull. Hist. Med.* | | 12,545.00 |
| Administrative expenses | | |
| Printing, postage, stationery, etc. | | 829.70 |
| Clerical assistance and FICA taxes | | 334.72 |
| Research in Progress | | 380.47 |
| Dues refunds | | 43.00 |
| Honors and Awards | | |
| Osler Medal | | 486.43 |
| Welch Medal | | 30.00 |
| Garrison Lecture | | 791.50 |
| Bank charges | | 11.52 |
| Transfers to other funds | | |
| Int. Soc. Hist. Med. | | 982.25 |
| Canadian account | | 300.00 |
| Savings account | | 6,000.00 |
| Education Committee questionnaire | | 557.94 |
| 1976/77 Total Disbursements | | 23,292.53 |
| Balance 31 October 1977 | | 2,598.70 |
| | | $25,891.23 |

# *MEDICO-HISTORICAL NEWS AND ACTIVITIES*

## CORRESPONDENCE AND REPORTS

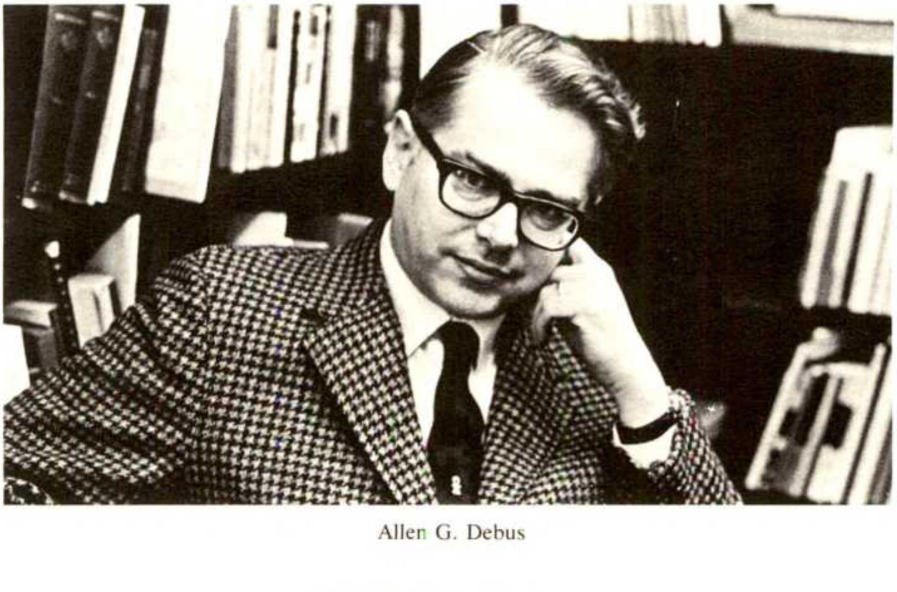
## APPOINTMENTS IN THE HISTORY OF MEDICINE

*University of Chicago*

Allen G. Debus has been named the first Morris Fishbein Professor of the History of Science and Medicine at the University of Chicago. He received a B.S. in chemistry at Northwestern University and an A.M. in history from the University of Indiana where he also took post-graduate courses in chemistry. He was research and development chemist for the Abbott Laboratories, 1951-56. His chief discovery there was the first new commercial method for the production of novocaine since 1919. Dr. Debus returned to graduate study in 1956, enrolling at Harvard to study history of science under I.B. Cohen. He received his Ph.D. in 1961 with a dissertation, "The English Paracelsians: A Study of Iatrochemistry in England to 1640."

Dr. Debus held a teaching fellowship at Harvard, 1957-59, and was awarded a joint Fulbright and SSRC fellowship for research in England under Douglas McKie of University College, London, and the personal supervision of Walter Pagel. He was Overseas Fellow, Churchill College, Cambridge and was in residence there in 1966-67 and for three summer sessions. He has held a Guggenheim Fellowship, was a member of the Institute for Advanced Studies, Princeton, and a National Endowment for the Humanities Fellow. He also has received research grants from the APS, NSF and NIH.

A member of the faculty of the University of Chicago since 1961, Dr. Debus has been successively Assistant Professor, Associate Professor and Professor. In 1971 he



Allen G. Debus

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became the first Director of the Morris Fishbein Center for the Study of the History of Science and Medicine and organized its graduate and undergraduate programs. He arranged the lecture series inaugurating the Center as well as subsequent Fishbein Lectures. Author and editor of numerous publications, Dr. Debus' research interests center mainly on Renaissance and early modern science and medicine, early chemistry and medical chemistry (pre-Lavoisier) and alchemy.

Toby Gelfand

*University of Ottawa*

Toby Gelfand has been appointed first incumbent of the Jason A. Hannah Chair in the History of Medicine at the University of Ottawa. This is one of the five Hannah Chairs established in Ontario (see *Bull. Hist. Med.*, 1978, *52:* 125-27). Dr. Gelfand received his B.S. from Ursinus College in Pennsylvania in 1963. At the University of Pennsylvania he studied medicine and then began graduate studies in the Department of the History and Philosophy of Science. In 1967 he became a degree candidate at the Johns Hopkins University, Institute of the History of Medicine, receiving his Ph.D. in 1973 with a dissertation, "The Training of Surgeons in Eighteenth Century Paris and Its Influence on Medical Education," written under Professor Temkin.

Dr. Gelfand has been awarded fellowships by the NIH and the Josiah Macy, Jr. Foundation. From 1971 to 1974 he was Instructor, then Assistant Professor in the Department of the History of Medicine at the University of Minnesota. In 1974 he became Assistant Professor in the Program in History and Philosophy of Science at Princeton where he remained until his Ottawa appointment in 1977.

In extended visits to France, Dr. Gelfand has examined material in archival repositories, incorporating his findings in writings on French medicine and surgery, especially of the eighteenth century. His interest in the history of medical education has led him to a more general study of the process of professionalization. Also among his interests are the social history of medicine, the relationship of science and medicine, and the history of medical specialization. He has frequently read papers before the AAHM and other groups and has published extensively.

# ANNOUNCEMENTS

## AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE

### *Osler Medal Essay Contest, 1979*

Students in schools of medicine and of osteopathy of the United States and Canada and graduates of the class of 1978 are encouraged to enter the William Osler Medal contest. Essays in the history of the health sciences, of a maximum length of 10,000 words, must be submitted by March 1, 1979, to the Chairman of the Osler Medal Committee of the American Association for the History of Medicine: John Norris, Ph.D., Department of History, University of British Columbia, 2075 Wesbrook Place, Vancouver, B.C. V6T 1W5, Canada. Further information about the rules may be obtained from Dr. Norris.

### *Research in Progress: History of Biomedical Sciences*

The American Association for the History of Medicine is planning, in early 1979, to issue a new listing of current research being conducted on topics in the history of medicine and related sciences, including dissertations. Individuals and department heads desiring to have their projects and those of their students listed are urged to send the following information: (1) name; (2) institution or mailing address; (3) title of substantial research topics; (4) estimated completion date. Send communications before December 1, 1978, to: James H. Cassedy, National Library of Medicine, 8600 Rockville Pike, Bethesda, Md. 20014.

## NATIONAL NEWS

### *American Institute of the History of Pharmacy*

The 1978 meeting of the AIHP was held in Montreal, May 13-16, 1978. One of the highlights of the meeting was a symposium on "Drugs across the Atlantic: New World Drugs in the Old World." The Edward Kremers Award for distinguished pharmacohistorical writing by an American was presented to Professor Allen C. Debus of the University of Chicago for his publications on the contributions of Paracelsus and the Paracelsians to pharmacy and drug therapy.

### *Boston*

*Medicine in Massachusetts, 1630-1800* was the subject of a four-session conference held May 25-26, 1978, under the sponsorship of the Colonial Society of Massachusetts. Twelve papers were presented on various aspects of medicine in Massachusetts by Richard D. Brown, Eric H. Christianson, Gerald B. Warden, J. Worth Estes, William C. Wigglesworth, James H. Cassedy, Philip Cash, Douglas L. Jones, George E. Gifford, Helen Brock, Whitfield J. Bell, Jr., and John C. Greene. Complete texts of the papers, with illustrations, will be published by the Society.

### *Cheiron*

The eleventh annual meeting of Cheiron, The International Society for the History of Behavioral and Social Sciences, is to be held at the University of Akron, Akron,

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Ohio, June 8-10, 1979. Papers may deal with aspects of any of the behavioral and social sciences. Information about the program may be obtained from Dr. Robert G. Weyand, Department of Psychology, University of Calgary, Calgary, Alberta, Canada.

### *Fulbright Alumni Association*

Former Fulbright grantees are invited to join the recently created Fulbright Alumni Association. Its purposes are to mobilize the experienced wisdom of former Fulbrighters, to promote international educational and cultural exchange and to involve them in this effort. The Association plans to hold a national meeting each year and it will circulate a newsletter to its members. Annual membership dues are $10.00. Questions about the Association may be addressed to Arthur P. Dudden, President, Fulbright Alumni Association, P.O. Box 1042, Bryn Mawr, Pa. 19010.

### *Johns Hopkins Institute of the History of Medicine*

The John J. Gilbride Memorial Lecture was presented on May 9, 1978, by Charles Webster, Wellcome Unit for the History of Medicine, Oxford University. Dr. Webster's topic was "The Genesis of the Cambridge Medical Faculty in the Sixteenth Century."

### *Macy Conference*

The Josiah Macy, Jr. Foundation, with the Johns University, Institute of the History of Medicine and School of Hygiene and Public Health, on May 5, 1978, held a one-day Conference on the History of Epidemiology. Introduced by Lloyd G. Stevenson, the two sessions were chaired by John Duffy and Reuel A. Stallone and included papers by John Eyler, Abraham and David Lilienfeld, Victor Hilts, Daphne Roe, Phyllis A. Richmond, Donald A. Henderson and Theodore Woodward. Discussers included Alexander Langmuir, Caroline Hannaway, William G. Cochran, Peter H. Niebyl, Genevieve Miller and Saul Jarcho. Publication of the proceedings of the Conference is planned.

### *National Academy of Sciences*

Dr. Owsei Temkin, William H. Welch Professor Emeritus, The Johns Hopkins University, Institute of the History of Medicine, has been elected to the National Academy of Sciences. He is an expert on the history of disease, particularly epilepsy, and on the cultural and philosophical aspects of both science and medicine. Election to membership in the Academy is considered to be one of the highest honors that can be accorded an American scientist or engineer. Dr. Temkin was one of 60 new members elected to the Academy this year in recognition of their distinguished and continuing achievements in original research.

### *Rockefeller Archive Center*

During 1978 the Rockefeller University made twelve grants-in-aid to support projects requiring substantial research in the Rockefeller Archive Center. Further grants, ranging from $500 to $1000, will be made to graduate students or advanced scholars engaged in similar projects for the year 1979. Applications for grants must be made before December 31, 1978. Inquiries about the program and the open collections at the Center should be addressed to: Director, Rockefeller Archive Center, Hillcrest, Pocantico Hills, North Tarrytown, N.Y. 10591. Rockefeller Foundation records, except for the last twenty years, have been opened for research.

*Vesalius Project*

John Howell-Books takes pleasure in announcing an important bibliographical discovery of great interest to medical libraries and collectors. We have at present in our shop a copy of Andreas Vesalius' *De Humani Corporis Fabrica* (Basel: Johann Oporinus, 1543) in which the six preliminary leaves, including the famous woodcut title-page and portrait, exhibit significant typographical differences from their counterparts in regular first edition copies. We believe that the preliminary leaves in our copy possibly represent the first state of the typesetting for the introductory material of Vesalius' great work. We have undertaken a census of copies of the 1543 *Fabrica* and have located only three other copies similar to ours. Seventy-five institutional and individual owners of the book in North America, Great Britain, and Western Europe have responded to our survey. We would like to locate more copies, especially outside North America, and invite your assistance. A detailed letter explaining the discovery and the questionnaire we have designed for our census are available from us at 434 Post Street, San Francisco, California 94102.

*Yale University*

The first George Rosen Prize in the History of Medicine was awarded to Dr. Thomas R. Forbes, Ebenezer Hunt Professor of Anatomy, Yale University School of Medicine Named for the late Dr. George Rosen the award is presented annually by the Beaumont Medical Club to a prominent scholar in the history of medicine. Dr. Forbes, a member of the Yale Faculty since 1945, is also a present member of the Special Study Section on the History of Life Sciences of the National Institutes of Health. Author of numerous articles and books on the history of medicine, Dr. Forbes has in recent years produced a series of medical historical films. The Rosen Prize, a gift of Neale Watson Academic Publications, consists of a cash award and a plaque.

## PROGRAMS OF MEETINGS

*Baltimore Blood Club*

*May 23, 1978* "The History of Sickle Cell Disease," C. Lockard Conley; "The Early History of Blood Transfusion," Caroline C. Hannaway.

*Benjamin Waterhouse Medical History Society, Boston*

*October 20, 1977*· "Paleopathology: What We Can Learn from Very Old Bones," R. Ted Steinbock.

*December 1, 1977:* "A Generation of Changes in Medicine," Herbert Black.

*January 12, 1978* "Treason and Other Medical Problems at the Siege of Boston," J. Worth Estes.

*March 9, 1978* "Massachusetts' Medical Roots," John J. Byrne.

*April 13, 1978.* "The Humble Origins of American Medicine, 1660-1760," David R. Edelstein

*Johns Hopkins Medical History Club. Baltimore, Md.*

*May 17, 1978:* Special meeting held in conjunction with the dedication of the Alan Mason Chesney Medical Archives. "Archives in the Perspective of Historical Scholarship," Carleton B. Chapman.

*Josiah C. Treent Society for the History of Medicine, Durham, N.C*

*April, 27, 1978:* "American Physiology in 1900: The Problem of Medical Relevance," Peter English.

*George W. Corner History of Medicine Society, Rochester, N Y.*

*April 6, 1978.* "The Reform of American Medical Education, 1870-1920," Kenneth M. Ludmerer.

*April 20, 1978:* "Dr. William L. Bradford," videotape interview by Gilbert B. Forbes.

*May 4, 1978:* "Mrs. George H. Whipple," videotape interview by Priscilla L. Cummings.

*May 19, 1978:* "Dr. Samuel J. Stabins," videotape interview by Alvin L Ureles.

*Josiah Trent Society for the History of Medicine*

*July 11, 1978:* "Harmony and Disharmony: Politics and Religion in the Development of Medicine in 19th and 20th Century Ontario and Quebec," Charles G. Roland.

*August 10, 1978:* "Canadian Medical Education and the Hannah Institute," G. R. Paterson.

*Mayo Foundation History of Medicine Society, Rochester, Minn.*

*April 17, 1978:* "The Acceleration Laboratory of the Mayo Aero-Medical Unit," a program commemorating the Mayo contribution in World War II: "The G Wheel of the Mayo Aero-Medical Unit," Charles F. Code; "Anti-blackout Protection—The Suit and the M-1 Maneuver," Earl H. Wood; "The Centrifuge of the Air," Edward H. Lamberg.

*July 12, 1978:* "Cortisone: Mayo Medical Milestone," Howard F. Polley

*Washington Society for the History of Medicine*

*May 20, 1978.* "The Scientific World of Thomas Jefferson," John C. Greene.

## NEWS FROM ABROAD

*Iran*

The newly established Ibn Sina Institute has been created with the goal of providing scholars with a place where a most complete collection of existing documents and studies concerning Ibn Sina (Avicenna) will be available for research. The Institute, which is funded by its patron, the Shahbanou of Iran, for its first three years will be administered by Bu-Ali Sina University, Hamadan. By 1981 it will have gained independent status. It will publish its own journal and eventually the complete works of Ibn-Sina in several internationally important languages. In the meantime the Institute is asking the cooperation of collectors and libraries and will appreciate receiving lists of their holdings. These should be sent to Dr. E. Moslehi-Shad, Ibn Sina Institute, P.O.B. 303, Hamadan, Iran.

### *Israel*

In April 1978, an exhibit, "The History of Embryology and Teratology," was opened at the Hebrew University-Hadassah Medical School in Jerusalem. An illustrated catalog of the exhibit has been compiled by Dr. Samuel S. Kottek of the Division of History of Medicine.

### *McGill University, Montreal*

The W. W. Francis Wing of the Osler Library of McGill University was officially opened on Osler Day, April 19, 1978. The ceremony was chaired by Dr. Philip M. Teigen, History of Medicine Librarian, and included addresses by Lloyd G. Stevenson, Samuel O. Freedman, Leo Jaffe and H. Rocke Robertson. Dr. Robertson, a former Principal and Vice Chancellor of McGill University, officiated at the ribbon cutting ceremony. The new wing, named in honor of the late Dr. W. W. Francis, a cousin of William Osler, editor of the *Bibliotheca Osleriana*, and long-time librarian of the Osler Library, will provide needed shelving for the library's holdings as well as additional space for staff and for the scholars who use this important collection.

### *Oxford*

The Physiological Society and the Wellcome Unit for History of Medicine on June 9, 1978, sponsored an informal symposium to celebrate the fourth centenary of the birth of William Harvey. In the day-long program papers were presented by Christopher Hill, Gweneth Whitteridge, Charles Webster, Jeffrey Boss, David Whitteridge, Denis Noble, Charles Michel and William Bynam.

### *University of Montreal*

Dr. Henri F. Ellenberger, professor in the Department of Crimonology of the University of Montreal, has been awarded the Jason A. Hannah Medal of the Royal Society of Canada "for an important publication in the history of medical and related sciences which is Canadian." He has written more than ninety papers and studies on various aspects of psychiatry. However, it is his remarkable book, *The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry*, that has earned him the award. A grant of $1000 accompanies the award.

### *Vienna*

On April 29, 1978, Professor Erna Lesky, M.D., Ph.D., Director of the Institute of the History of Medicine at the University of Vienna, received an honorary medical doctorate from the University of Zürich, Switzerland.

### *Visiting Professorship*

The Hannah Institute for the History of Medicine invites applications for the post of Visiting Professor for one term (either but not both) during the academic year 1979-80. Candidates must hold a doctorate and have teaching experience at the university level. For further information please contact: G. R. Paterson, Ph.D., Hannah Institute for the History of Medicine, 50 Prince Arthur Ave., Suite 105, Toronto, Ont., Canada M5R 1B5, or T. Gelfand, Ph.D., Program History of Medicine, Faculty of Health Sciences, University of Ottawa, 275 Nicholas St., Ottawa, Ont., Canada K1N 9A9.

# *BOOK REVIEWS*

Owsei Temkin. *The Double Face of Janus and Other Essays in the History of Medicine*. Baltimore and London: The Johns Hopkins University Press, 1977. x + 543 pp. $22.50

This volume brings together thirty-six of Owsei Temkin's essays and addresses, published over the years in journals and collective volumes. Temkin's admirers would not have minded if this 543-page book had been even larger and had included all of the articles published to date. But let us not grudge our approval; what we are offered in this elegant volume from the Johns Hopkins University Press is a major work. For it is certainly no mere collection of articles written at various times on various subjects. Rich and multifaceted, this work forms a self-evident unity which will strike any attentive reader; its unity is a matter of style of thinking. By this I mean not only the method of exposition and investigation—invariably carried through with exemplary clarity and precision; in a deeper sense I am speaking of the choices made, which always lead Temkin to problems that *matter*, even (or especially) when he is dealing with relatively circumscribed topics. I am speaking of his constant concern with enlarging the horizon by precise correlations, with interrelating different problems in order to make them significant for us. In short, in all the essays contained in this book we find the quality that defines the great historian: a balance between the exigencies of subjective interest and those of objective precision.

The historian himself has a history. He is part of his time; his mind has developed at a certain moment in civilization. He can only speak of the past from the vantage point of his own present. Of this Temkin is clearly aware. The introductory chapter, from which the book takes its title, is an intellectual autobiography. In an account of exemplary sobriety and modesty, Temkin tells us how he first became a physician, and then a historian of medicine; how he took his first steps in that discipline; what kind of work he did with Sigerist at Leipzig (between 1925 and 1932); under what circumstances he came to the United States and the Institute of the History of Medicine at Johns Hopkins University; about the books he brought to completion and the projects that have remained suspended. What makes this account so fascinating is the manner in which Temkin shows the interplay between personal choice and historical circumstance; in the process he sheds a great deal of light on the role played by institutions (the Leipzig institute, the institute in Baltimore), which are themselves subject to the vicissitudes of history and in need of material resources and the guarantees of freedom without which research would become impossible. In writing the history of his ideas, Temkin introduces the history of his life, and finally, in rapid and firm strokes, that of his time. Those who know how to read will find that the theoretical implications of this intellectual autobiography are considerable, for it amounts to a reflection on the historian's vocation and his craft, and on the sense of purpose an entire life can find in them. At the same time, it is also a reflection on the changes and developments that the history of medicine has undergone from the time when it was

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practiced by the physician-philologist (and often the physician-antiquarian) to the present time when the history of medicine is becoming a special professional field open to non-physicians interested in the social, demographic, and economic aspects of the history of diseases and their treatment.

The first sentence of the introduction is revealing: "When matriculating at the University of Leipzig, Germany, in the spring of 1922, I was asked to state my field of study: 'Medicine and philosophy,' I said." It would be no exaggeration to say that in everything he has accomplished, Temkin has remained faithful to this plan of his youth. At Leipzig he first considered writing a logic of medicine and then became interested in the concept of meaning (Sinnbegriff) in medicine. Even if today these studies no longer meet with their author's full approval, he has not, by any means, disowned their orientation and their underlying idea: "Health and disease involve man as a cultural being." It is this conviction that imports their intellectual tonus to Temkin's works; it is also the reason why we cannot be indifferent to any of his studies. But Temkin understood early on that a purely philosophical approach to medicine is not without danger, for he encountered some negative examples of such an approach in the excessive fascination with "romanticism" and Paracelsism on the part of certain minds at Leipzig. In the triangle traced by Sigerist, medicine, history, and philosophy should have been joined together in a balanced reciprocal and circular relationship; but on German soil, and at that particular date, it was to be expected that philosophy should make some claim to a position of hegemony. The intellectual climate of America (as in the case of Leo Spitzer, among others) had the effect of moderating this philosophizing ambition, or at least of making it increasingly amenable to the testing of facts. The study written in 1956 and entitled: "On the Interrelationship of the History and the Philosophy of Medicine" (pp. 101-109), is both cautious and subtly varied. Above all, it shows that any solid philosophical reflection on medicine needs to be grounded in the given facts of history, for the very reason that medicine is a changing and evolving activity. Indeed one might go so far as to say that all good philosophy of medicine should be a philosophy of the history of medicine. Nor does this philosophy need to manifest itself otherwise than by the manner in which it conducts or makes use of historical research. Such a philosophy knows how to find the real problems, makes sure that the facts of which it speaks are true, or tries to test its hypotheses at the earliest possible stage by confronting them with the historical data. There is no need for brilliant argumentation (after the manner of half-informed philosophers); it is enough to move about among well-established facts. This is always Temkin's method: his philosophy becomes invisible because he constantly stays in touch with documents, evident proofs, and examples that speak for themselves. Positive knowledge (which the pure "positivists" pile up to their hearts' content in order to show it off) is a principle of economy for Temkin. His learning is a means that enables the philosophical reflection to attain its goal more rapidly and more surely, yet it does not tend to impose a doctrine and to formulate peremptory conclusions. To *shed light* on a problem and to leave it open to what may be found in the future often implies greater courage and greater intellectual honesty; and in this sense, at any rate, Temkin has remained the philosopher he wanted to be when he entered the university.

The set of studies grouped together under the heading: "The Historical Approach to Medicine" (pp. 4-134), invites us to examine the historiography of medicine, its aims, its methods, and the various ways of approaching it (from the history of ideas to sociology). It is, without question, the best introduction to medical historiography available, and I should advise all those who consider going into the history of medicine to begin by reading these pages, and especially the masterful study entitled: "The Usefulness of Medical History for Medicine."

These pages must be reread in the light of the symbol proposed by Sigerist and evoked again by Temkin: the history of medicine, like the statue of Janus, has two faces. One looks toward the past, the other, turned toward the future, blends into the fact of contemporary medicine; and this is why, as Sigerist had hoped, the history of medicine might be able to "participate in the solution of the great problems which today occupy the physicians' world" (p. 9). Sigerist, as we know, had come to conceive its role not so much as a participation in the advance of medical science, but rather as a contribution to be made to the elaboration of a health care policy. As for Temkin, he tells us that upon looking into the past in his studies on Hippocratism in late Antiquity, he conceived the project that was to culminate in his great work on epilepsy as a contribution to the solution of a medical problem that was still poorly understood: "This disease had received attention in an unbroken tradition since the days of Hippocrates. Nevertheless, it was still badly understood and historical clarification might be of some help to neurologists" (p. 20).

Yet, in reading Temkin's later studies, one does wonder whether the idea of a direct contribution of the history of medicine to medical science itself—as a body of knowledge, hypotheses, and techniques—can be maintained. For such a contribution can never be more than very limited, almost accidental—somewhat after the manner in which a plant used by "primitive" peoples sometimes finds its way into the scientific pharmacopoea. In fact, the principal benefit of the history of medicine does not accrue to the content of the physician's medical knowledge but to his *relationship* with his knowledge. Without the presence of the historian, today's physician, the man of our time, would not know how to ask himself the questions he must ask, questions that have to do not so much with how scientific knowledge is elaborated as with what he is to do with his knowledge. And this, precisely, is a question for which the science itself provides no answer. If any answer can be given, it must come out of an accumulated body of wisdom, and the sources of that wisdom are found in the domain defined by the general term of "humanities." And for today's physician, as Temkin very rightly points out, the history of medicine is the "connecting link with the humanities" (p. 95). In giving a detailed account of the function of the history of ideas in particular, Temkin makes a statement that is undoubtedly among the best justifications for the writing of history: "When dealing with the history of ideas, man's intellect is in its true milieu where thought meets thought. . . . Beyond this, asking why others thought as they did challenges us to ask why we think as we do. For this question there is a great need in medicine, whose progress now affects peoples everywhere for better or, sometimes, for worse. Because as a whole the situation is unprecedented, guidance is expected from insight into how we happen to be where we are, not only in our knowledge and our institutions but also in our thinking" (p. 125). To this he adds an argument that should be remembered by all historians who are asked what purpose is served by their work: "Since nobody thinks or acts without some picture of the past, we are all apt to accept a historical myth where we cannot rely on historical knowledge. Where history is lacking, mythology takes its place and those who disdain history are among the foremost victims of mythology" (p. 69).

Historical research is subject to certain conditions of feasibility—or, if you will, to an ethic—which demands that the investigator remain within a relatively circumscribed field, so that he can speak from first-hand knowledge, fully informed of the facts. Good history is always partial history and compensates for its limitations by the originality of its approach, the solidity of its findings, and the scope of the consequences to be drawn from them. "We can behold the truth only in the form of samples" (p. 37), writes Temkin in order to justify the choice of history of medicine in the more general field of history. But within medical history it is again necessary to narrow down one's choice to well-defined topics that lend themselves to fruitful research and interpretation. Speaking of the impact of his encounter with Edelstein, Temkin writes: "Exploration of one period in depth was needed, even if only to make one aware of the craftsmanship necessary for dealing with other periods" (p. 19). Temkin, for his part, has resolved the difficulty in a two-fold manner. On the one hand he has selected, from the very beginning of his work as a historian, the period of late Antiquity—"the time when Hippocrates and Galen rose to uncontested authority, and when the reading and interpretation of their works constituted the teaching of medical science, while encyclopedias served the needs of the practitioner" (p. 19). On the other hand, he has chosen well-defined problems whose study implied a historical sweep. These he has developed sometimes according to the formula "from the earliest times to the present," and sometimes within a more restricted segment of time: *Falling Sickness*, *Galenism*, and soon (we hope) the history of the concept of irritability are examples of this approach that have appeared in the form of books. But the different groups of studies assembled in *The Double Face of Janus* are equally illustrative of a method in which the historian's perception isolates a problem in order to replace it into its historical context and its evolution through the centuries. In this manner an isolated theme, despite its apparent limitation, is placed in a light that reveals the various stages in its development and its mutations over time, so that it becomes an indicator of a more general movement of ideas. Indeed, it assumes the value of a sign or expression for an entire cultural history, just as one of Leibniz' monads expresses the totality of the universe. I am thinking in particular of the study entitled: "Medicine and the Problem of Moral Responsibility" (pp. 50-67), and of the group of papers gathered togethered under the heading "Health and Disease" (pp. 419-484), among which one is glad to find "On the History of 'Morality and Syphilis,' " the English version of the study of 1927 that establishes Temkin as a precursor of what today in France is called the *histoire des mentalités*.

Everywhere in this volume, Temkin's interest in the "conceptual tools" used by medicine throughout the ages is evident. Health, disease, infection, the organism, irritation, are so many notions with which physicians were working in all innocence and in a perfectly objective manner, or so they thought; but the historian's scrutiny reveals the long semantic history with which certain terms are burdened, or indeed their metaphorical character. Studies like "An Historical Analysis of the Concept of Infection" (pp. 456-471) or "Metaphors of Human Biology" (pp. 271-283) go to the core of the preconceptions and the verbal resources that were or still are inseparable from medical thinking itself. They show that, in each period, the physician cannot help but borrow certain of his interpretive tools from the vocabulary current in non-medical systems. This is a particularly pertinent way of showing which were the precise elements that linked medical ideas and practices to the surrounding culture at various times in history. This same problem appears again in the studies bearing the numbers 24 to 28, but this time it is limited to the connections some of the outstanding scientists of the early nineteenth century maintained with the philosophical doctrines of their time, such as materialism, monism, and the philosophy of nature. Here the historian-philosopher focuses his attention on some physician-philosophers and, as in a mirror, attempts to find out both what they owed to the stimulus of philosophy and what they owed to their rebellion against the presumptuous claim that everything is known inherent is so many philosophical doctrines. The connections between Magendie and the ideologues, between Wunderlich and Schelling are elucidated in an admirably sound fashion.

Other studies, finally, deliberately remain within the compass of medical history, where they address themselves to relatively narrowly focused questions requiring the judicious use of exact information. Temkin knows how to bring his immense learning to bear, without ever permitting his account to lose any of its ease and serene simplicity. Whenever Temkin deals with particulars (and that is everywhere, since all good historical writing consists of assembling and interpreting particulars), he is master of the art of showing their signifying value at every moment. The few pages in which Temkin examines "The Role of Surgery in the Rise of Modern Medical Thought" (pp. 487-496) are remarkable for their documentation, their critical sense, and above all for the light they shed on the birth of the clinic in Europe. In the same manner I could have cited the studies on the evolution of the treatment of syphilis.

At this point I should undoubtedly set forth and comment upon the ideas developed throughout these essays. Yet, however important these ideas are, I have chosen not to do so, for I have felt that it is more valuable to present this rich, multifaceted and coherent book as an example of what the history of medicine can achieve when it is served by intelligence, learning, intellectual fairness, and a warm heart.

REVIEWED by JEAN STAROBINSKI, M.D., Professor ordinaire, Histoire de la médecine, Faculté de Médecine, and Professeur ordinaire, Langue et litterature françaises, Faculté des Lettres, Université de Genève, Geneva, Switzerland.
Translated by ELBORG FORSTER.

EDMOND A. MURPHY. *The Logic of Medicine.* Baltimore & London: The Johns Hopkins University Press, 1976. vii + 353 pp. Ill. $16.50 (hardbound).

The form and style of this text are highly reminiscent of standard introductory logic textbooks. There is an eloquent commitment to the value and power of reason over both (1) knowledge of either fact or theory (since facts and theories may change and change rapidly) and (2) rhetoric, superstition, bias and intuition. Discussions cover basic concepts of reasoning, discovery, communication and criticism. Each chapter considers a delimited topic and each is meant to be fairly independent for separate study. Exercises are provided with almost every chapter and these are answered or discussed in the appendix. A closing exercise consists in having the reader criticize a fictitious scientific essay line by line. Dr. Murphy's own critique of this is very helpful. The textbook format is appropriate, since the reader is explicitly conceived as a first-year medical student of average intelligence whose objective is to become as aware as possible of the kinds of errors in medicine. The two major difficulties in the book concern readership and what medicine is.

The text does not seem appropriate to the avowed readership. For example, Murphy refers to "the classification of disease, such as that used in the Royal College of Physicians' list" (p. 108) and he remarks that "if the dire findings of Feinstein and his colleagues about trends of the clinical investigator persist," a new breed of investigator will have to be evolved (p. 170), and he alludes to "multivariate methods that have become so popular" (p. 236). These and similar pronouncements could not be presumed to be meaningful for the freshman reader.

The most serious difficulty grows from the fact that "medicine" is left undefined and is used ambiguously as an equivalent to "biological science," or as "therapeutics" and sometimes as "decision-making." Despite a forceful distinction between science and medicine in chapter one, the bulk of the book sends mixed signals at best (cf. p. 118 where it is said that judges must rely primarily on medical evidence in determining the sex of a person). Regarding decision-making, there is an excellent discussion of how to construe the seriousness of false negative diagnoses and false positive diagnoses given both the seriousness of particular disease and the treatment prescribed for it. If a serious illness can be treated relatively inexpensively and innocuously, we would try to keep false negative test outcomes to a minimum and not regret false positive outcomes very much. This best displays what Murphy means in distinguishing medicine from science since in medicine humane concerns help determine methods. My point here is not so much that Dr. Murphy is wrong, but that if medicine is the mixed bag it seems to be, an explicit discussion of this should have been provided. Furthermore, how the topic of each chapter bears on each aspect of medicine should have been clearly stated.

The material itself is divided into three major sections: "The Underlying Ideas," "The Evidence" and "A Synthesis." In the first, the concepts which are discussed and explained include *definition, tautology, cause, classification, precision, accuracy*, and *resolution*. Many of these are well worn topics in the philosophical literature but surprisingly little reference is made to it.

For example, in the discussions of causation (there is one theoretical but optional chapter and a later practical and required chapter), several well taken and important remarks are made regarding medicine, e.g., "It would be clearly meaningless to debate whether thrombosis causes arteriosclerosis or arteriosclerosis causes thrombosis. The only fruitful discussions would be whether one particular episode of thrombosis caused one particular episode of arteriosclerosis, or which order of events was the most common" (pp. 96-7). But the discussion is insulated from much philosophic literature and could well have been strengthened by reference to current philosophic work. Thus the product is of mixed quality. The distinction between "cause" in the ontological sense of regular succession and the etiological sense of manipulation and control is helpful, while the theoretical chapter (pp. 81-99) is opaque; it is difficult to know why all of it was included and why all comes under the rubric "causation."

Dr. Murphy seems to have been striving to provide a bite-size panacea where really something much harder to swallow is needed. For example, the chapter which comes closest to being a discussion of logic in its academic sense (pp. 174-193) is over compressed and it suffers badly partially as a result. Yet there are other kinds of problems, for syllogisms are confused with rules of inference and these are confused with argument forms. This serious fault would likely mislead and confuse the novice-reader.

Further, some of Murphy's sentences go astray in a predominantly stylistic way: "syllepsis" is defined as "a rhetorical figure in which the same work is used close together in two different meanings. Commonly one is literal, the other figurative. Example: 'He wore a bright red shirt and a fatuous smile on his face'" (p. 336). The structure of the first sentence is unhappy and vague. How the example exemplifies the general formulation is puzzling. And although this is not a singular instance, and not many of these are especially drastic, they are especially annoying since the book is strident about clarity and precision.

Yet there is some excellence in the work. Chapter 9, "Disease and Normality," (pp. 117-134) is especially well done. *Disease* is explained in terms of homeostasis, unilocal inheritance and infection. "Normal" is clearly shown to have seven distinct senses which are likely to be confused without his warning. In this most demanding sort of conceptual awareness and attentiveness Dr. Murphy shows the kind of skill that we might have hoped to find throughout the entire book.

Reviewed by EDMUND L. ERDE, Ph.D., Assistant Professor of the Philosophy of Medicine, University of Texas Medical Branch, Galveston, Texas.

E. A. E. REYMOND. *A Medical Book from Crocodilopolis* (P. Vindob. D. 6257). From the Contents of the Libraries of the Suchos Temples in the Fayyum, Part I. Wien: Verlag Bruder Hollinek, 1976 (Mitteilungen aus der Payrussammlung der Oesterreichischen Nationalbibliothek, N.S., X). 304 pp. Ill.

The present work constitutes the first English translation and detailed analysis of a Demotic papyrus belonging to the extensive collection available at the Austrian National Library in Vienna. Although fragmentary in nature, this is the only document of a medical nature which has survived from the temple library of the "Mansion of Suchos" located in the north-west region of the Fayyum, a depression in the Libian Desert connected with the Nile River.

Written approximately in the second half of the 2nd century A.D., this papyrus contains enough archaisms to postulate that it was copied from another Ptolomaic book between the 3rd and 2nd century B.C. Like most other extant medical literature from Pharaonic times, it constitutes a heterogeneous collection of recipes very similar to that recorded in the Ebers Papyrus providing practitioners with useful therapeutical compounds and methods as well as indications for their application.

The translator suggests that the *Fayyum Medical Book* was probably codified in lower Egypt, in close proximity to Greek sources. While certain features recall the old Egyptian background, the text contains numerous instances of Demotic transliterations of Greek terms. As Reymond points out, however, "the essentially Egyptian character of the whole composition is prominent and manifest." At the same time, affinities between Egyptian and Greek pharmacopoeia suggest an influence of Egyptian herbalists on Greek-oriented medical practice in places such as Alexandria.

Prof. Reymond has determined that the surviving fragments originally belonged to six separate books dealing with skin diseases, swellings and internal diseases, affecting the stomach, bowels and mouth. A physician's compendium containing a collection of recipes for ear and head trouble, parasitic disease, minor bites and rectal ailments completed the text.

With regard to the elaborate compounds used for drying purposes in a variety of skin affections, there appear to be similarities with some ingredients recommended by Galen. The use of herbs such as myrrh, chick pea, poppy and pepper—the latter only imported into Egypt from India in the 4th century B.C.—reveals an integration of ancient Egyptian practices with newer Hellenistic cures. Of interest is the use of the cautery in the treatment of some skin swellings occurring secondarily to some febrile illness, oil for painful stiffness of the neck, and a mixture of herb infusions for fever.

In Reymond's view, this papyrus furnishes historians with an opportunity to further study the transmission of Egyptian medical knowledge to the Greeks. As a document of Ptolemaic practice it offers glimpses of a primarily herbal and dietetic approach to disease seemingly symptomatic in nature. For those interested in theoretical views, especially the conceptualization of disease, the fragmentary and practical character of the document may prove disappointing. Prof. Reymond should be commended for his painstaking work in translating and editing this papyrus. A Demotic glossary of disease terms and medicinal agents is included.

Reviewed by GUENTER B. RISSE, M.D., Ph.D., Professor of the History of Medicine, University of Wisconsin, Madison, Wisconsin.

# THE BULLETIN RECENTLY RECEIVED*

Ricardo Archila. *Alemenia y Venezuela, vinculos médicos.* Caracas, Venezuela, 1978. 127 pp.

Juan Aréchaga Martinez. *La anatomia Española en la primera mitad del siglo XIX.* Granada: Universidad de Granada, 1977 (Collección monográfica Universidad de Granada, 50). 244 pp. Ill.

W. David Baird (Editor). Years of Discontent: Doctor Frank L. James in Arkansas, 1877-1878. Memphis: Memphis State University Press, 1977. xxviii + 84 pp. Ill. $5.95 (paperbound).

George A. Bender and Johns Parascandola (Editors). *American Pharmacy in the Colonial and Revolutionary Periods.* Madison: American Institute of the History of Pharmacy, 1977. 43 pp. Ill. $3.00.

Carlo M. Cipolla. *Chi ruppe i rastelli a Monte Lupo.* Bologna: Società editrice il Mulino, 1977. 134 pp. Ill.

Thomas E. Cone, Jr. *200 Years of Feeding Infants in America.* Columbus, Ohio Ross Laboratories, 1976. 92 pp. Ill.

Marc Cramer, Jean Starobinski and Marc-A. Barblan (Compilers). *Centenaire de la Faculté de Médecine de l'Université de Genève (1876-1976).* Geneva Editions Médecine et Hygiène, 1978. xvi + 165 pp. Ill.

Irving I. Edgar. *The Origins of the Healing Art: A Psycho-Evolutionary Approach to the History of Medicine.* New York: Philosophical Library, 1978. x + 246 pp. Ill. $9.50.

Armando Favassa and Mary Oman. *Anthropological and Cross-Cultural Themes in Mental Health. An Annotated Bibliography 1925-1974.* Columbia, Mo.: University of Missouri Press, 1977. 386 pp. $30.00.

Thomas Rogers Forbes. *Crowner's Quest.* Philadelphia: The American Philosophical Society, 1978 (Translations of the American Philosophical Society, vol. 68, pt. 1, 1978). 52 pp. Ill. $5.00.

Luis García Ballester. *Medicina, ciencia y minorias marginadas: Los Moriscos.* Granada: Universidad de Granada, 1977 (Collección monográfica Universidad de Granada, 54). 163 pp.

Sami K. Hamarneh. *Temples of the Muses and a History of Pharmacy Museums.* Tokyo: Naito Foundation, 1972 (Naito Foundation English Publication, ser. 1, no. 1). x + 146 pp. Ill. $6.00.

Gordon Harrison. *Mosquitoes, Malaria and Man: A History of the Hostilities since 1880.* New York: E. P. Dutton, 1978. viii + 314 pp. Ill. $15.00.

William H. Helfand. *Medicine and Pharmacy in American Political Prints (1765-1870).* Madison: American Institute of the History of Pharmacy, 1978. 84 pp. Ill. $10.00.

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* The *Bulletin* reserves freedom of decision as to the publications to be included in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine

Clark Kimball and Marcus J. Smith. *The Hospital at the End of the Sante Fe Trail: A Photographic History of St. Vincent Hospital, Santa Fe, New Mexico.* Sante Fe: Rydal Press, 1977. 137 pp. Ill. $8.95.

Werner Friedrich Kümmel. *Musik und Medizin: Ihre Wechselbeziehungen in Theorie und Praxis von 800 bis 1800.* Freiburg/Munchen: Verlag Karl Alber, 1977 (Freiburger Beitrage zur Wissenschafts und Universitätsgeschichte, Bd. 2). 481 pp. Ill.

Ralph E. Landes. *The American Urological Association: A Historical Essay Commemorating the First 75 Years.* Nutley, N.J.: Roche Laboratories, 1977. 16 pp. Ill.

*Letters from the Past From John Hunter to Edward Jenner.* London: Royal College of Surgeons of England, 1976. 40 pp. Ill. £1.50.

Paola Manuli and Mario Vegetti. *Cuore, sangue e cervello: Biologia e antropologia nel pensiero antico.* Milano: Episteme Editrice, 1977. 241 pp.

Matthew T. Moore. *Archives of the International Congresses and Society of Neuropathology, 1952-1977.* Philadelphia: Lea & Febiger, 1978. xvii + 683 pp. Ill. $35.00.

Julia F. Morton. *Major Medicinal Plants: Botany, Culture and Uses.* Springfield, Ill.: Charles C Thomas, 1978. xix + 431 pp. Ill. $49.50.

Ronald L. Numbers. *Almost Persuaded. American Physicians and Compulsory Health Insurance, 1912-1920.* Baltimore and London: Johns Hopkins University Press, 1978 (The Henry E. Sigerist Supplements to the Bulletin of the History of Medicine, new series, no. 1). xii + 158 pp. Ill. $10.00.

Wilder Penfield. *The Mystery of the Mind: A Clinical Study of Consciousness and the Human Brain.* Princeton, N.J.: Princeton University Press, 1973. xxix + 123 pp. Ill. $3.45 (paperbound).

D. Ian Pool. *The Maori Population of New Zealand, 1769-1971.* Aukland, New Zealand: Aukland University Press; New York: Oxford University Press, 1978. 266 pp. $15.25 (paperbound).

Spencer L. Rogers. *An Early Human Fossil from the Yuha Desert of Southern California: Physical Characteristics.* San Diego; San Diego Museum of Man, 1977 (San Diego Museum Papers, No. 12). 27 pp. Ill.

Robert Shosteck. *Notes on an Early Virginia Physician, Dr. John De Sequeyra.* Washington, D.C.: B'nai B'rith, 1971 (Repr. *American Jewish Archives,* 1971, 23, no 2). $.75.

R. F. Timken-Zinkann. *Ein Mensch Namens Dürer:* Das Kuntslers Leben, Ideen, Umwelt. Berlin: Gebr. Hann Verlag, 1972. 225 pp. Ill. D.M. 29.

Manfred Ullman. *Islamic Medicine.* Edinburgh: Edinburgh University Press, 1978. Biblio Distribution Center, 81 Adams Drive, Totowa, N.J. 07512. (Islamic Survey Series 11). xiv + 128 pp. Ill. $9.50.

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## CONTRIBUTORS TO THIS NUMBER

Alex Berman, Ph.D., is Professor Emeritus of History and Historical Studies in Pharmacy, Department of History, University of Cincinnati, Cincinnati, Ohio 45221

Steven A. Brody, M.D., is Assistant Resident in Medicine, Yale-New Haven Hospital, New Haven, Conn. 06504

Russell C. Maulitz, M.D., Ph.D., is Assistant Professor, Department of History and Sociology of Science, University of Pennsylvania, Edgar Fahs Smith Hall D6, 215 S. 34th St., Philadelphia, Pa. 19104

Robert J. Miciotto, B.A., is a doctoral candidate in the history of medicine, Johns Hopkins University, Institute of the History of Medicine, 1900 E. Monument St., Baltimore, Md. 21205

Jane Oppenheimer, Ph.D., is William R. Kenan, Jr., Professor of History of Science, Department of History, Biology Building, Bryn Mawr College, Bryn Mawr, Pa. 19010

Peter Romanofsky, Ph.D., is Assistant Professor, Department of History, Jersey City State College, Jersey City, N.J. 07305

Charles G. Roland, M.D., Secretary-Treasurer of the American Association for the History of Medicine, is Jason A. Hannah Professor of the History of Medicine, McMaster University, 100 Main St. West, Hamilton, Ont., Canada L8S 4J9

Linda E. Voigts, Ph.D., Assistant Professor, Department of English, University of Missouri, Kansas City, Mo., is in residence as a Mellon Fellow for 1978-79, Department of English, Harvard University, Warren House, Cambridge, Mass. 02138

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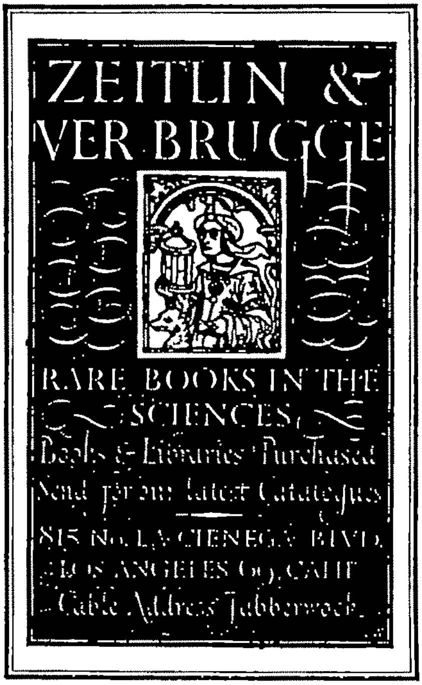

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VOLUME 52 FALL 1978 NUMBER 3

# J. J. R. MACLEOD:

# HISTORY OF THE RESEARCHES LEADING TO THE DISCOVERY OF INSULIN

## With an Introduction by Lloyd G. Stevenson

When Professor J. J. R. Macleod (1876-1935) wrote this memoir (the above title is his) he submitted it to Col. Albert Gooderham, chairman of the board of governors of the University of Toronto, with a covering letter, also printed below, dated 20 September 1922; an addendum was written in May 1923. It was in 1923 that the Nobel Prize for Physiology and Medicine was awarded to Dr. Frederick Banting and Professor Macleod "for their discovery of insulin," and although Banting divided his half of the prize money with his colleague, C. H. Best, and Macleod, for his part, shared the other half with J. B. Collip, controversy has never quite died down about the respective roles of the principals in this great discovery. Banting, Best and Collip, with Banting clearly predominant, have had their ardent advocates. Macleod, however, despite his status as a Nobel laureate, has remained a rather shadowy figure in the background of the tale. Worse still, he has been seen as encroaching on the glory of Banting and Best, as an unworthy senior (he was forty-five at the time) who actually "stole" some measure of credit from his juniors. (Nobody can deny that a Nobel Prize is some measure.) Although I do not believe that anyone who has investigated the story and talked with people who were there at the time considers that Banting was justified in the use of the angry words he threw around in 1921 and 1922, the accusations nevertheless linger in the air. The late Professor Velyien Henderson, who in 1921 gave Banting an appointment in the University of Toronto Department of Pharmacology to piece out his meagre income, was willing to say that Macleod was at first somewhat unappreciative of Banting and Best, and that Banting (a choleric hero to be sure) had reason to be annoyed; beyond this I do not think that Henderson went.* Professor C. L. Starr tried hard to heal the breach and to make Macleod and Banting friends; in this he failed.

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* It is relevant to this Macleod memoir that Professor Henderson used to maintain that in the summer of 1921 he had provided the on-the-spot counsel for which the two inexperienced young investigators could not turn to Professor Macleod, who was absent in Scotland (Henderson's assistance was later warmly acknowl-

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All of the others chiefly concerned in the story have been heard from. Not Macleod. And in a recent British film about the discovery of insulin, Sir Ralph Richardson, in the rôle of Professor Macleod, has fixed in our minds an unappealing image—complex and interesting, it is true, but finally repellant. The scriptwriters have sought, of course, for elements of conflict, of drama, and the likeness of Macleod they have subtly fashioned has been memorably realized by Richardson. So compelling, indeed, is this darkling and dubious figure that, once seen, it will be slow to fade from the mind's eye of the viewer. It is this, together with the rather deprecatory tone which is used in speaking or writing of Macleod, that now makes it seem appropriate that he should at last speak for himself. His report to Col. Gooderham was written fifty-six years ago.

For reasons which are not altogether clear, it has long been impossible (it may still be impossible) to dig a copy of Macleod's personal account of the discovery of insulin out of the gothic vaults of the University of Toronto.* But since several copies exist on both sides of the Atlantic this is not an insurmountable problem.

As the letter written by Professor David Campbell on 11 January 1949 indicates, the document was discovered among Macleod's papers left in the hands of his sister-in-law in Aberdeen; at least one copy found its way to Canada at that time. Wrote Campbell: "I have placed a copy of it in the class library of the Physiological Department of the University [of Aberdeen] and I am considering the question of sending a copy for the purposes of the record to the Royal Society in London." At some point Professor Collip arranged to have a copy sent to the Medical Research Council in Ottawa and another copy has been deposited in the Public Archives of Canada.

It is obvious that Macleod's "History of the Researches Leading to the Discovery of Insulin" is not now the most inaccessible of documents, although known to but few even yet. It is, however, the testimony of one who was both participant and witness and one who has not previously had his day on the stand. Several of the most eminent of today's Canadian academic scientists—Professor F. C. MacIntosh, Professor R. A Cleghorn and Professor Robert L. Noble among others—have shown their interest in this document and their feeling that J J. R Macleod, for all his Nobel Prize, has been a little roughly handled by history and by the University of Toronto. His distinguished research and his valued teaching in three universities—one in the

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edged, in general terms, by Professor Best.) For example, Henderson seems to have suggested to Banting and Best that the results obtained in their first successful experiments might be explained by blood dilution. They performed experiments to eliminate this possibility In the memoir submitted to Col Gooderham, Macleod asserted that he too had made this suggestion in 1921 Cf below, p 303, and note 4 Cf also Lloyd G. Stevenson, *Sir Frederick Banting*, 2nd ed. (Toronto: Ryerson Press; Springfield, Ill · Thomas, 1947), pp. 93-94 The picture of Macleod to be found in this biography of Banting, now more than thirty years old, needs to be amended in some details but is not, I think, seriously distorted. It would have been closer to a just appraisal had not the University of Toronto taken action which had the effect of suppression of the Macleod report Why Macleod remained silent after withdrawing from the Canadian scene we can only conjecture Cf. Lloyd G. Stevenson in *D.S B.*, 1973, *8* 614-615, although this essay on Macleod was also written before his report to Gooderham became available and it contains nothing that is new.

* The late President Sydney Smith threatened the present writer with legal action if he persisted in his attempts to lay hands on, and to quote from, Macleod's report to Col. Gooderham At an earlier time, not even enlisting the help of the Chancellor, Sir William Mulock forced a way into the vaults for this purpose, they would not let him in. It might be said that poor Sir William was unable to best the guards

United States, one in Canada and one in Scotland—entitle him to a respectful hearing.

What is there to be found in this document which has caused the University of Toronto to treat it as "sensitive" in the very highest degree for more than half a century? The attitude seems to have been that controversy should be allowed to die, and that among dead controversialists some are deader than others. Now that all are dead, however, it ought, I think, to be conceded that the reputations of all four are alive and wakeful. None of the controversialists should be prevented from appearing before the "bar of history." If there are here no startling revelations to be made, the details nevertheless differ markedly from common accounts and the facts are to be seen in a light very different indeed from that which is often supplied.

Out of the half illumination in which he has languished for too long, the dim cave to which recent dramaturgy has seemed to condemn him once again, Macleod must be brought forward into full light. He must be allowed to speak for himself. With minimal apparatus (the numbered footnotes are all his) here is his testimony, with Professor Campbell's brief narrative of how it was unearthed.

L. G. S.

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University of Aberdeen

Professor David Campbell, M.D., M.A., B.Sc., M.D. Materia Medica Department.
Foresterhill, Aberdeen,
11th January, 1949.

G. E. Hall, Esq., M.S.A., M.D., Ph.D., F.R.S.C.,
President and Vice-Chancellor,
The University of Western Ontario,
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My dear President,

You will remember that on your visit to Aberdeen last fall you asked me if I knew of any document bearing on the history of the researches leading to the discovery of Insulin which might have been left by the late Professor Macleod

Shortly after our talk I spoke to his sister-in-law about the matter and asked her for permission to look through any papers that he had left. Unfortunately at that time she was ill, and nothing was done about it. A month or two afterwards she died in my ward in the Royal Infirmary, and I raised the question with her executors They found the document, a copy of which I enclose for your acceptance.

I have placed a copy of it in the class library of the Physiological Department of the University, and I am considering the question of sending a copy for the purposes of the record to the Royal Society in London.

Without entering into the merits or demerits of the case, I cannot help feeling on reading the document that Macleod was made quite undeservedly unhappy

With kindest regards and best wishes,

Yours very sincerely,
[David Campbell]

# HISTORY OF THE RESEARCHES LEADING TO THE DISCOVERY OF INSULIN [1922]

J. J R. MACLEOD

*Letter of Macleod to Col. Albert Gooderham, Toronto, Sept. 20, 1922*

My dear Colonel,

I am glad to seize this opportunity to put in writing my position with regard to the most unfortunate misunderstandings which have arisen concerning questions of priority and the credit that should be given to those responsible for the discovery of Insulin. If by so doing I can help to retain for the University of Toronto the reputation which it has already acquired, through the publications on Insulin, as a place where collaborative investigation among diverse groups has been successful in giving to medical science a finished piece of work within a few months' time, I shall consider this effort well repaid.

In the statement which I made in the "Star" on September 7th, on the solicitation of Dr. Banting and Mr. Best, I thought that I had fully and fairly stated my position with reference to the priority of the discovery, and I may say that I am entirely bewildered as to why this should not be considered a most liberal acknowledgment of the contributions these gentlemen have made. Such statements, prepared on the spur of the moment under pressure, as this one was, can never be entirely fair, and when it appeared in print I saw that I had not given to Mr. Best all the credit which is his due. Besides this, I consider that I was just in criticising the head lines and the introductory paragraph (preceding my statement) as they appeared in the Star, which implied that I had given to Dr. Banting "credit for the complete discovery of Insulin for the treatment of diabetes." I gave Dr. Banting full credit for suggesting the experiment of using extracts of duct-ligated pancreas and for successfully carrying it out with Mr. Best's collaboration but this did not imply that I gave these gentlemen complete credit for the discovery of Insulin as now used in the treatment of diabetes. This has depended on the conjoint efforts of several investigators working under my direction, of which Dr. Banting was one.

I have devoted practically all my spare time during the past eighteen years to the investigation of the problem of diabetes, and like every other worker in this field I have constantly had in mind the discovery of some "internal secretion" whose absence might be the cause of diabetes. Though familiar with the various more or less unfruitful attempts that had from time to time been made to discover this internal secretion, and although in my writings I had pointed out that the pancreas might control the metabolism of sugar locally, yet I have in my lectures always emphasised the advisability of further attempts to discover an internal secretion. Dr. Banting came to consult me early in 1921 with reference to an experiment of using extracts prepared from the pancreas sometime after ligation of the ducts. For this idea Dr. Banting deserves complete credit; if he had not contributed this idea and undertaken to test it experimentally, the discovery of Insulin would probably not as yet have been made. On the other hand, with the knowledge which he possessed of the methods for attacking such a problem he could certainly not have made such rapid progress without careful guidance and assistance. In every field of research innumerable suggestions are offered as to the new pathways that should be explored, and it takes judgment and long years of experience to know which of these suggestions should be encouraged and assisted by affording the means to put them to the test. I believe I am perfectly safe in saying that there are very few investigators in the field of diabetes, at the time Dr. Banting undertook it, who would have thought the experiment with duct-tied pancreas worth while a serious trial. To Dr. Banting and Mr. Best is also due full credit for showing that extracts of foetal pancreas have a beneficial effect on experimental diabetes.

The further steps in the evolution of the problem are set down in order in the appended statement. In this letter, however, I should like to point out that I consider myself most unjustly subjected to the present criticism in view of the fact that I declined the offer of Dr. Banting and Mr. Best to add my name to theirs in the first published report of their work. In many, if not most, laboratories it is the custom for the "chief" to have his name on the papers when the investigation is in a subject related to that in which he is engaged and if he stands responsible for the conclusions and has participated to the extent that I did in the planning of the research. By this step I made it perfectly evident that I considered the full credit for this investigation to be Banting and Best's. This is surely what counts in questions of priority.

I also agreed, with the consent of the other investigators, to place Dr. Banting's and Mr. Best's names first on all the papers published on work on the physiological effects of Insulin in progress during the early months of 1922. This has been done.

I am informed that the following are some of the causes for the misunderstandings which have arisen from time to time:-

(1) My critical attitude during the early stages of the investigation. In explaining this attitude, I may state that after the investigation had been planned I encouraged Banting and Best in each step of the work, but I urged them to keep to the main problem so that no doubt might exist when their results were published that they had really achieved much more than their predecessors.

In view of my knowledge of the previous attempts that had been made to prove the presence of an anti-diabetic hormone and the extremely confusing nature of the results, I maintain that this was the correct attitude to adopt. At the same time, I constantly encouraged the investigators to continue and I placed all the available resources I could command at their disposal.

(2) My hesitation to use my influence to have the extract tried out clinically in the Toronto General Hospital until I was reasonably certain that the first trials would be successful. There have been so many premature attempts at clinical application of laboratory results in other fields, that my action in this regard was entirely justifiable.

(3) My participation in the debate after Dr. Banting's paper was presented at the Physiological Society at New Haven. I arranged for Dr. Banting presenting the results in person at the meeting of the Physiological Society, and I participated in the debate only because there seemed to be adverse criticism. I surely had a right as director of the laboratory in which the work was done to take this action.

(4) My interview with "the Press" reporter in January, 1922. This was forced upon me and my whole attention was given to impressing the reporter with the advisability of making it clear that we could not as yet say that a cure had been discovered for Diabetes.

(5) My presentation of the work of the group, with their unanimous consent, including the associated clinical investigators, Drs. Campbell and Fletcher, at the meeting of the Association of American Physicians in Washington.

This, I may say, is the hardest of the criticisms for me to understand. I did my utmost to convince my audience of the importance of the discoveries, and after a preliminary review of the work leading up to them, I fully described Dr. Banting's and Mr. Best's discovery of the effect of extracts of duct-ligated pancreas in experimental diabetes, and their names stand first among those heading the paper submitted for publication. I confess to a probable mistake on my part, namely, that I did not insist on Banting and Best attending the meeting, but we had discussed this, the advisability of their going, before going to the meeting and they decided not to go because of the expense of the trip.

I sincerely trust and hope that your endeavours to clear the atmosphere may be successful, for it is extremely important for the future success of our work that the Insulin committee should remain intact and its various members have trust and confidence in one another. I will, of course, be glad to attend any meeting with Dr. Banting and Mr. Best that you may find necessary, but I hope our statements will not conflict so far as to make it so. My statement as submitted is to the best of my knowledge and belief a true and accurate one, and if any facts are seriously challenged I would respectfully request that some of the others associated in our work be asked to attend the meeting. I have written Dr. Collip.

Sincerely yours,
[J. J. R. Macleod]

### *Researches Leading to the Discovery of Insulin*

Some time early in 1921[1] F. G. Banting called on me in my room in the Physiological Department of the University of Toronto and said that he had been giving part of his time to assisting in teaching and research in the physiological department of Western University at London, Ontario. In the course of his reading he had been struck with the indirect nature of the evidence upon which rests the theory that the Isles of Langerhans of the pancreas furnish an internal secretion the presence of which is in some way related to the occurrence of diabetes. He told me that he had discussed this with Professors Miller and Crane in London and that they suggested his consulting me.

As everyone familiar with the problem has for long realized, the preparation of an extract of pancreas containing the active principle (or internal secretion) that controls the metabolism of sugar is complicated by the presence in such extracts of digestive ferments which would destroy the active principle. This point immediately came up in our discussion and Dr. Banting made the suggestion that it might be possible to circumvent these ferments by using duct-ligated pancreas. As stated in the first paper published on the work, he formed this idea while reading in a textbook of surgery. I also told Dr. Banting that it would be useless to attempt this work unless he was prepared to give up all his time for several months to the problem, but that if he agreed to do this, I would place every facility at his disposal and show him how the investigation should be planned and conducted. He told me that he would give the matter careful consideration since it would be a serious undertaking for him to give up the connections he had already made as a surgeon in London. He talked the matter over with Dr. Clarence Starr who, a month or so after Banting's visit, called me up to ask whether I thought the problem was sufficiently hopeful of a favourable outcome to warrant Banting taking the step I had suggested. I told Dr. Starr that although it was taking considerable chances I thought the research well worth proceeding with. Some time later Banting wrote me that he had almost decided to come to Toronto, and I replied that I would help him all I could to proceed with the investigation.[2]

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[1] This date was November, 1920 See letter of Dr Banting, Nov 21, 1920

He arrived about the middle of May, 1921. I found that Dr. Banting had only a superficial text-book knowledge of the work that had been done on the effects of pancreatic extracts in diabetes and that he had very little practical familiarity with the methods by which such a problem could be investigated in the laboratory. Either at this meeting or at the earlier one we discussed other possible methods than that of using duct-tied pancreas for achieving the object of getting rid of the digestive ferment, and I suggested freezing the pancreas and then extracting it at the lowest possible temperature with alcohol. This later proved to be of great value. I worked out with Dr. Banting a plan of investigation the first step of which was to render one or two dogs diabetic by extirpation of the pancreas so that he might make himself familiar with the cause of this condition in animals untreated with extract. At the same time I advised him to tie the ducts in several other animals so that the gland might be suitably degenerated when the time came to use extracts.[3] I advised him to use the Hedon method for extirpation of the pancreas, gave him the necessary references, and assisted him in the first operation. Since he had no practical knowledge of how to conduct the estimations of sugar in blood and of nitrogen and sugar in urine, upon the accuracy of which the whole investigation depended, I arranged for one of my research fellows, C. H. Best, to collaborate with him.

I left for Britain in the middle of June satisfied that Banting and Best were in a position to carry forward the above investigations and having supplied them with full references to the literature. In August, 1921, I received from Banting and Best a report of their work in which it was quite clear that they had obtained a fall in blood sugar following injection of extracts made from degenerated pancreas, and I wrote them[4] that I considered the results most encouraging and that more should be accumulated. I pointed out, in this letter or after my return to Toronto, the necessity of making sure that the reduction in the percentage of blood sugar was not due to dilution of the blood, and suggested that haemoglobin and red corpuscles should be measured to control this. In that letter or on my return to Toronto I also said that the evidence was incomplete until the total utilization of sugar had been more carefully measured. I have no copy of this letter since it was written in ink.

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[2] See copies of letters from Dr. Banting dated March 8th and April 18th and my reply of March 11th.

[3] At one of these conferences Dr. J. B. Collip was present and the following is taken from a letter dated May 13th, 1923, which he sent me, without my solicitation: "I was in your office early in May, 1921, when you gave Banting full directions for his summer's campaign."

On my return to Toronto at the end of September I found that Banting and Best were dissatisfied with the facilities at their disposal, and early in October they formally demanded of me that I improve them.[5] Dr. Banting said that if I did not do so he would apply to the Rockefeller Institute or the Mayo Clinic for facilities. I agreed to give Dr. Banting the half-time of one of the laboratory boys, Arthur Lamb, a separate room and every other facility I could. Meanwhile, Professor V. E. Henderson had generously arranged to offer Dr. Banting a temporary position as teaching assistant in his department with a salary.

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[4] I am told that Dr Banting did not receive this letter This, I cannot understand I spent considerable time in Paisley in going over the results in order to form a proper estimate of their value, and I clearly recollect that I decided that more work would have to done to prove.

(a) that dilution of the blood was not the cause of the fall in blood sugar.

(b) that the carbohydrate balance was really improved as a result of giving the extract

It is possible that I did not specify these points in detail in my letter to Dr Banting, but I certainly insisted on them on my return to Toronto a few weeks later, and I certainly wrote Dr. Banting a letter acknowledging receipt of his charts and stating what, in general, I though of the results

[5] I have come to learn that Dr Banting considers that he was not given encouragement or adequate facilities for carrying on the work at this stage. I consider this a most unjust, ungracious and unreasonable attitude. With regard to Dr. Banting being allowed even to start the experiments there are, I imagine, very few directors of laboratories that would have been willing to allow an outside person the free use of whatever animal material was available at the time, of the only operating room and its appliances that the department possessed, of the services of the animal caretaker, of anaesthetics and other surgical necessities, of the chemical apparatus and reagents necessary in the analyses. Besides this, I gave Dr. Banting all the assistance I could in planning the details of the experiments, in searching the literature and, finding that he was entirely unfamiliar with the chemical methods necessary for the proposed investigation, I offered him the assistance of one of my research fellows, C. H Best, who had been trained in this work Mr. Best was meanwhile being paid as a research fellow, and in October when I returned to Toronto from Scotland, I arranged to have Dr Banting paid $150 00 from the research funds of the University The total extra expense to the department of the work done by Dr Banting and Mr Best from May, 1921, to December, 1921, is given in the herewith attached statement Excluding the salaries paid Mr Best and Mr. Joyce about $626.00 were used in this research exclusively out of a total available for research, after deducting Dr. Fidlar's salary, of $1800.00

[6] In the papers subsequently published in the *Transactions of the Royal Society of Canada* and in the *American Journal of Physiology*, Banting's name always came first, although he participated only very indirectly in the work on which most of them were based

About this time Banting suggested that I, or some of my associates, take an active part in the work, but I pointed out that this being his and Best's research they should independently complete the work as outlined, and that then if the results continued satisfactory I would participate in the further investigations with my assistants.

About the middle of November I asked Banting and Best to give their results at the Journal Club. Banting requested me to introduce the subject, which I did, giving briefly some of the historical relationships of their work. I was told, in January (1922), that Banting considered that in doing this I did not sufficiently indicate the share he had in the work. Had I been told of this attitude of Banting at the time, it would have served to warn me of his peculiar temperament and of his entirely unwarranted suspicions that I was trying to receive the credit for the results he had obtained. In the discussion following the presentation of this paper, Dr. H. B. Taylor made the suggestion that the most convincing evidence of the beneficial influence of the extract would be the prolongation of life of diabetic dogs by its daily administration. Next day I urged Banting and Best to try this to which they agreed. From a practical stand-point this has proved to be a most important step.

A week or so later I suggested that the work was now suitably advanced to warrant publication, and I showed Banting and Best how the results should be compiled. When their draft was ready I spent considerable time going over it and made several alterations in the manner of presentation. When finally the manuscript was ready Banting asked me if I wished my name to appear along with his and Best's, and my reply was that I thanked them but could not do so since it was their work and "I did not wish to fly under borrowed colours."

When the call came for papers for the New Haven meeting of the American Physiological Society I suggested that a preliminary report be made of the work to date, and at Banting's request I agreed that my name should appear. Banting said he wished this because it would call attention to the paper since I was known as an authority on experimental diabetes. I agreed to this arrangement but stipulated that Banting and Best should both attend the meeting and should themselves present the report. I prepared the short written report for the Proceedings.

About this time Banting independently of any suggestion from me prepared extracts from the foetal pancreas of the ox using alcohol as an extracting fluid as I had suggested at the very start of the work in May. When Banting reported this to me I suggested the air evaporation method for removal of the alcohol and concentration of the extract which I had used during several years for lactic acid estimation and which was adopted during some time in the large scale production of Insulin. It was at this stage that Dr. Banting invited Dr. J. B. Collip to collaborate in the investigation, especially in connection with the preparation of pure extracts. Dr. Collip immediately started this work extracting adult ox pancreas by means of alcohol. The positive nature of the results led me to make the suggestion in the presence of Banting, Best and Collip that the next thing to do would be to try the effect of the extracts on the blood sugar of rabbits, particularly when these were made diabetic by experimental methods. Within a day or so Dr. Collip reported that he had tried this and found the blood sugar to be lowered, thus confirming the effectiveness of the extract and making further development of its production for clinical purposes very much more simple since normal rabbits could be used to test its potency instead of depancreated [*sic*] dogs.

For the week or so preceding the Christmas vacation I met Banting, Best and Collip in company on several occasions, usually at lunch time, when we talked over plans for further investigation. It was agreed that the time had now arrived when it would be advisable to ascertain the glycogen content of the liver and other viscera in depancreated dogs treated with the extract. Dr. Collip undertook to do this and to observe the excretion of acetone bodies in these animals prior to death. It was decided to postpone any measurement of the respiratory exchange and respiratory quotient until January when it would be possible for Dr. J. Hepburn to collaborate with C. H. Best in the gas analysis involved in this work.

During the Christmas vacation Banting, Best and I travelled on the same train to New Haven, Collip joining us at Hamilton, when he told us that he had found the liver of a depancreated dog to whom extract had been given, along with much sugar, to contain about 25 per cent glycogen. On the train we continued our usual discussions of plans for future research.

At the meeting in New Haven Banting presented the paper at the last conjoint session. Banting was very nervous, and it was evident that he had not succeeded in convincing all of his audience that the results obtained proved the presence of an internal secretion of the pancreas—the primary object of the work—any more definitely than had those of previous investigators. Allen, Kleiner, Carlson and others commented on the work in such a manner as to make it appear to me that I should emphasise the value of the evidence presented. Although chairman of the meeting, I took part, laying stress on the frequency of direct relationship between the injections and the lowering of blood sugar and on the prolongation of life of two treated animals. Best also spoke. Dr. Graham told me later that Banting considered that I had deliberately so discussed his paper at New Haven as to convey the impression that I was entirely responsible for the origination of this piece of work and that he and Best had merely acted as research assistants. If this was so, it was entirely unintentional on my part, my object being to persuade the audience of the real value of the investigations.

Following the meeting, I walked back to the Hotel with E. L. Scott who told me of his work some years previously with the use of alcoholic extracts of pancreas. While in my room in the hotel Dr. Clowes rang me up to say that he considered the evidence convincing and to ask whether the firm to which he acted as scientific advisor might be given the opportunity of collaborating with us in preparing the extract in larger quantity for the profession. I replied that the work was not yet sufficiently advanced for that, but that I would bear his request in mind.

When I returned to Toronto I saw nothing to lead me to suspect that Banting considered that I had stolen his results, although I came to learn later that at this time he was freely complaining of this among his friends, in a way highly detrimental to my character. I did note, however, following a meeting of the Journal Club in January at which "Ketonuria" was under discussion, that there was a strain between him and Best on the one hand and Collip on the other.

At this time (January) Collip reported to me at lunch time practically every day the progress he was making in the preparation of the extract, and at Banting's repeated solicitations I persuaded Dr. Duncan Graham to give him the opportunity in his clinic to try the extract on a case of diabetes. Some extract prepared by Banting and Best about this time or previously was therefore injected subcutaneously into a boy, under the immediate charge of Dr. W. R. Campbell, with the result that the blood sugar and urinary sugar, I think, were temporarily reduced, but several abscesses developed at the places of inoculation. This, of course, made it impossible to continue the use of such extracts, and it served to spur on Collip to secure purer extract, which he succeeded in doing by the method now in use. Collip is entirely reponsible for this work, and it is unfair and unjust for Banting and Best to rob him of any of the credit by saying that they told him of the percentages of alcohol at which the active principle was soluble. Collip denies that they gave him any information that was of use in this connection and they never communicated any such to me. The idea of using alcohol at different concentrations was in any case not original with them since E. L. Scott had used it and I had recommended it as an extractive at the conference with Banting in May, 1921. As a result of Collip's researches a non-irritating highly potent preparation of insulin was supplied to the Medical clinic and was used in the cases reported in the *Canadian Medical Journal* in March.

I wish to emphasise here that the development of the investigation at this critical stage depended on the collaborative effort of several workers:

(1) To make an extract
(2) To make it safe for clinical use.
(3) To find a practical method for testing the potency of the extract.
(4) To demonstrate that the action of the active principle in the extracts on animals was more far-reaching than to lower the blood sugar and the urinary sugar in depancreated animals.

The 'Star' newspaper reporter came to hear of the treatment of the boy referred to above and interviewed Best about January 20th. Best brought him (the reporter) to see me, and I did my utmost to impress the reporter with the unwisdom of giving publicity to the work at the present stage because of the clamour for extract which it would cause. He agreed to emphasise the preliminary nature of the work which he did in the issue appearing that evening. He published in the paper the statement that I had been engaged in research work on experimental diabetes during the last fifteen years. I learned later that Banting took great exception to this since it implied that I and not he was mainly responsible for the work that had been done. I had mentioned this fact to the reporter not with any idea of minimising the importance of Dr. Banting's share in the work, and I cannot remember what question of the reporter prompted me to make the statement.

A few days after this Prof. Duncan Graham told me that Dr. Banting in an interview that same forenoon, had complained to him of my attempting to steal the credit for his work on depancreated dogs, and that he said he intended to see me about it. Dr. Graham said that Banting gave as evidence for this accusation my participation in the discussion over his paper at New Haven and the newspaper interview. Dr. Graham also said that he asked Banting to allow him to see me first. In this interview with Dr. Graham I learned for the first time that Dr. Banting had for some weeks been accusing me freely among his friends—and also openly in the hearing of persons in various semi-public places—of stealing his results. He impressed me with the serious character of these charges of Banting, and as Banting had been discussing this matter with other people advised me to take steps to put things right. I saw Prof. Clarence Starr that same evening and he promised to see Banting immediately. Next morning Banting after an interview with Dr. Starr came to see me and apologized for what he had done. He told me that he misunderstood my actions and assured me that he would do his utmost to undo the harm he had done me among his friends. How thoroughly he did this I cannot say, but I can easily see that my reputation for fair-dealing has been very seriously undermined in Toronto. At this conference Dr. Banting assured me that he would not misunderstand me in the future and would not conceal any doubts he might have as to whether I was treating him properly. I agreed to continue collaborating with him and I assured him that I had no intention of robbing him of any of the glory that was his due. I agreed further to have the names of those who participated in the researches, then underway in my department, in which the physiological action of pancreatic extracts was being investigated, published with the names in alphabetical order. This placed his name first and Best's second.

The researches referred to were as follows:-

1. The preparation of the extract. (Banting, Best, Collip & Macleod.)
2. The action of the extract on normal rabbits. (Banting, Best, Collip Macleod & Noble.)
3. Its action on rabbits with experimental hyperglycaemia. (Banting, Best, Collip, Macleod & Noble.)
4. The glycogen content of the liver and heart in depancreated dogs treated with Insulin. (Banting, Best, Collip & Macleod.)
5. The acetone body decretion of depancreated dogs treated with Insulin. (Banting, Best, Collip & Macleod.)
6. The Respiratory Quotient of depancreated dogs treated with Insulin. (Banting, Best, Collip & Macleod.)

I assigned E. C. Noble to No. 3 for his M.A. thesis and Best agreed to collaborate with Dr. J. Hepburn for No. 5. Dr. Collip had already made observations under Nos. 2, 4 and 5 (6). All of these researches with the exception of the first were planned in conference, and I directed the various steps in carrying them out. The results have all been published in preliminary form and the first three also in detail.

Dr. Banting from now on was less regular in his attendance in the laboratory than previously. He removed the pancreas, or Best did so for the few dogs observed in February, March and April for Respiratory Quotient and the glycogen content of the viscera. While weighing the viscera for this purpose I noted that the liver of a dog untreated with 'Insulin'—by which name we now spoke of the extract—was very fatty whereas that of a treated animal was normal, and I planned to have fat also estimated in blood, liver and heart. Dr. Banting helped in the estimation of the fat.

In February the Connaught Laboratories undertook to prepare Insulin in larger quantities, and it was arranged that Dr. Collip should devote all of his spare time to this work. After an initial success, the yield became unsatisfactory and finally failed entirely. We consulted together frequently as to the possible cause for this failure and finally after seeing whether H-ion or the grade of alcohol had anything to do with it, I found by Sorensen's titration that considerable digestion of protein was occurring in the vacuum distillation and recommended the use of evaporation in a current of dry air so as to keep down the temperature. This plan was put into operation with successful outcome. Dr. Banting meanwhile gave all the spare time he could in assisting in the preparation of extract. From March on he did practically no work in the Department of Physiology except a few fat estimations.

From this stage on the negotiations for clinical investigation, the arrangements for presentation of papers at societies, and the business affairs connected with collaborating with the Eli Lilly Co. are sufficiently well-known not to require attention here.

I may add that the present status of our knowledge of Insulin depends practically entirely on work which has been done in this Department with the collaboration of the Medical Clinic and the Connaught Laboratories. Through concentrated effort, for the co-ordination of which I have been responsible, we have given to Science in little more than one year a practically completed piece of research work—we have proved the value of Insulin—and in doing this there have been several other researches than those detailed above done in my laboratory that have contributed important information of a fundamental character.

*Added May 1923*

The foregoing report with the possible exceptions indicated was handed to Col. Gooderham in September. This was done because Colonel Gooderham requested it, and the letter sent by me along with the report is attached hereto. I was not shown any report prepared by Dr. Banting or Mr. Best, but was informed by Col. Gooderham that they did not entirely agree with mine. I do not know whether Dr. Banting and Mr. Best were shown my report.

I showed this report to Professor Andrew Hunter and Professor Duncan Graham, but I cannot remember sending a copy to President Falconer or anyone else.

I do not remember ever sending any report pertaining to Dr. Banting's work to President Falconer, although I sent several letters to be presented to the Board of Governors requesting financial assistance for the work done by him and others.

## APPENDIX

*Expenses incurred in initial research on insulin.*
*June to December, 1921.*

| | | |
|---|---|---|
| *Salaries.* | | |
| Dr. F G. Banting, July & September | 150.00 | |
| Mr. C. H. Best, July & September | 170.00 | |
| *Mr. Best, salary as Fellow but engaged on insulin research. | 800.00 | |
| | 1,120.00 | 1,120.00 |
| *Ingram & Bell.* | | |
| Apparatus and chemicals, gauze, catgut, etc., as per bills in Department of Physiology. | | 64.71 |
| *Lyman Bros.* | | |
| Chemicals and other, as per bills in Department of Physiology | | 116.00 |
| *Animals*, dogs, as per bills in Dept. of Physiology. | | 126.00 |
| | | 1,426.71 |

Mr. Wm. Joyce was instructed to take care of the animals, prepare for the operations and to clean the instruments. This he did throughout the summer.

All of the above with the exception of the item * were paid from the research appropriation. [Including this item, the grand total cost was $2546.71.]

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Dr. F. G Banting,
442 Adelaide Street
London, Ont., Nov. 21st, 1920.

Dear Professor McLeod [*sic*],

You will remember that I was in your office two weeks ago discussing a problem on the Pancreas. After leaving you I talked the matter over with Dr. C. L. Starr, and he said he would think it over and write advising me. I have not received word from him yet.

I am still very anxious to do the work, but as Dr. Starr is my "father advisor" I would not like to commence until I have heard from him. I will write and tell you as soon as a decision is reached.

Sincerely,
(Sgd) F. G. Banting.

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Dr. F. G. Banting,
442 Adelaide Street,
London, Ont. March 8.

Dear Professor McLeod,

You will possibly remember me consulting you last November concerning experimental work on the internal secretion of the Pancreas. On the advice of Dr. Starr I will finish the term here.

I will be free from University work about May 15th and if your offer for facilities to do the research still holds good, I will plan to spend the remainder of May, June and July in your laboratory.

If it were possible, I would like to do half-a-dozen dogs during the Easter Holidays, so that they would be ready for investigation in May.

Please let me know soon in order that I can make arrangements.

Believe me,
Yours very truly
(Sgd) F. G. Banting.

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Dr. F. G. Banting,
442 Adelaide Street,
London, Ont., April 18, 1921.

Dear Professor McLeod,

I am making plans to leave here about May 15th for Toronto, and would like to start right in as soon as possible as finance is pressing, and I do not want to waste time. If animals and rooms cannot be ready about that time kindly let me know.

Sincerely,
(Sgd) F G. Banting

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University of Toronto
Toronto, Canada.

Department of Physiology.

March 11th, 1921.

Dr. F. G. Banting,
442 Adelaide Street,
LONDON, Ont.

Dear Dr. Banting,

I will be glad to have you come up here on May 15, as you suggest, to see what you can do with the problem of Pancreatic Diabetes, which we spoke about. I doubt, however, whether it would be advisable to attempt any preliminary operations during the Easter holidays, since between that time and May 18th there will be no one here personally interested in the supervision of animals, and this supervision, as you know, is of extreme importance in all researches of this character. Between these dates everyone on the staff in the laboratory will be under the unfortunate necessity of devoting a great part of his time to examination details and to other things pertaining to the winding up of the session. I think that after May 15th when you could get close personal attention to the work, it could proceed satisfactorily within the time which you suggest you could give to it.

With regards,

I am,
Yours sincerely,
(Sgd.) J. J. R. Macleod.

J. J. R. M./M. E. A.

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University of Toronto,
Toronto, Canada.

Department of Physiology. April 20th, 1921

Dr. F. G. Banting,
442 Adelaide St.,
LONDON, Ont.

Dear Dr. Banting,

Unless the unforseen happens, everything will be ready for you here on May 15th.

I am,
Sincerely yours,
(Sgd.) J. J. R. Macleod.

J. J. R. M./M. G.

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University of Toronto,
Toronto, Canada.

Department of Physiology. September 30th, 1921.

Sir Robert Falconer,
President,
University of Toronto.

Dear Sir Robert,

The following have been engaged in the Department of Physiology in research work during the Summer months, with decidedly satisfactory results, and I request you to recommend to the Board of Governors that the following payments be made from the research funds appropriated to this department for the research assistants during vacation months:-

| | |
|---|---|
| S. U. Page for work during September | $ 85.00 |
| C. H. Best for work during July and September | 170.00 |
| F. G. Banting for work during July and September | 150.00 |

I am,
Respectfully yours,
(Sgd.) J. J. R. Macleod

J. J. R. M./M. E. A.

# "SQUEEZING MOTHER NATURE"

## EXPERIMENTAL PHYSIOLOGY IN THE UNITED STATES BEFORE 1870*

EDWARD C. ATWATER

Experimental physiology before 1870 means William Beaumont to most Americans. This was as true in Beaumont's time as in our own. The impression that Beaumont and his investigations were ignored by professional contemporaries is incorrect.[1] His work attracted considerable attention at home as well as abroad. The demand for the original edition of his book was great enough to require a second printing the year after its publication and a second edition in 1847.[2] Some of the attention Beaumont received was probably curiosity regarding his subject, Alexis St. Martin. When asked to speak before medical societies Beaumont was expected to bring St. Martin with him for display[3] and, by mid-century, there was hardly a school-boy who

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* A shorter version of this paper was presented at the annual meeting of the American Association for the History of Medicine, Madison, Wisconsin, May 13, 1977

[1] The idea that Americans did not appreciate the work of Beaumont apparently resulted from Dr George Rosen's 1935 doctoral thesis at the University of Berlin and the controversy which followed its publication seven years later Rosen attempted to show that Beaumont's work had greater impact in Germany than in France or Great Britain (George Rosen, *The Reception of William Beaumont's Discovery in Europe* [New York: Schuman's, 1942]) This thesis was soon challenged and the dispute which erupted periodically between 1943 and 1957 exaggerated the importance of Beaumont's reception in Europe and left the impression that he was virtually ignored at home. See Genevieve Miller, *Bull Hist Med.*, 1943, *13*: 111-13; George Rosen, *ibid.*, 1943, *13* 631-42; F N L. Poynter, *J. Hist Med* , 1957, *12*: 511-12; George Rosen, *ibid* , 1958, *13*. 404-406, and F N. L. Poynter, *ibid.*, 1958, *13*: 406-409 This was emphasized by Richard H Shryock in *American Medical Research Past and Present* (New York: Commonwealth Fund, 1947) p. 28, and in "American indifference to basic science during the nineteenth century," *Arch. Internat d'Hist des Sci.*, 1948, *28*: 50-65; p. 58 There Shryock says flatly that Beaumont was "more appreciated in Europe than in [his] own country."

[2] The first edition (Plattsburgh, 1833) had a second printing (Boston, 1834) making a total of 3000 copies A second, revised edition was published in Burlington, Vermont, in 1847, by which time the book had also been published in Great Britain (Edinburgh, 1838), France, and Germany (Leipzig, 1834) Favorable reviews appeared promptly after publication in the American medical press: *Boston Med & Surg. J* , Jan 15, 1834, *9* 365-68; *Amer J Med Sci* , May 1834, *14*: 117-49; *Transylvania J Med & Allied Sci.*, 1834, *7* 337-78 D. Francis Condie wrote in the *Amer J Med Sci* that the book "cannot fail to be favorably received by the members of the profession" and called it "a valuable addition to their knowledge . a means of facilitating the inquiries of future experiments " He predicted that a second edition would soon be needed

[3] See *Boston Med & Surg J* , April 16, 1834, *10*: 163, for a description of St Martin's visit to Boston during which "the faculty" had an opportunity of examining him. See also Jesse S Myer, *Life and Letters of Dr William Beaumont* (St. Louis. C V Mosby, 1912), p. 224. Charles Caldwell comments on the uniqueness

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had not read in his hygiene textbook of the wonderful man with the hole in his stomach.[4] The public was fascinated, the profession was interested.

The lack of immediate clinical application and the fact that the work did not stimulate others to experimental activity, however, may have given to those living at a later time, when experimental investigation had become a highly organized and productive endeavor, the appearance of indifference by comparison.[5] The failure of Beaumont's feat is certainly not unique. It took more than 200 years after the work of Vesalius (1514-64) until most physicians learned anatomy by dissecting cadavers. The crucial experiments of William Harvey (1578-1657), and Albrecht von Haller (1708-77), and John Hunter (1728-93) attracted the attention of the medical world but did not lead to systematic research by others. Discovery alone may not lead to evolution in science.

In the case of experimental physiology there were two particular impediments which retarded the development of a research establishment—the primitive state of experimental technology and the lack of financial subsidy. The latter, in turn, was less a reflection of disinterest in research than customary distaste for the idea of supporting able-bodied men whose profession made it possible for them to earn a living. This feeling applied to medical education as well as research.[6] Beaumont was able to sustain his research because he had an institutional sponsor—the U.S. Army, a salary,

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of the experimental subject in his review in the Transylvania Journal (see n 2 above). On a later tour (1856) St Martin was exhibited in at least ten cities from Boston to St Louis. Though experiments were performed on him in Philadelphia by Francis G. Smith, Professor of the Institutes of Medicine in the medical department of Pennsylvania College, the purpose of the trip was primarily to make money and one editor said that St Martin had been "Barnumized" by his new sponsor, Dr T G Bunting Cf Edward H. Bensley, "Alexis St Martin and Dr Bunting." *Bull. Hist Med*, 1970, *44* 101–108.

[4] Between 1836 and 1850 at least seven physicians wrote popular physiology books All but one of these mention Beaumont and his experimental subject, some of them at length The date of the first edition precedes the title. J L. Comstock, M D (1836), *Outlines of Physiology* (New York, 1836), pp 128-30, Charles A Lee, M D (1838). *Human Physiology* . . (New York, 1939), pp 284-85, John H Griscom, M.D (1839), *Animal Mechanism and Physiology* . . (New York, 1840), pp 305-15; Reynell Coates, M D (1840), *Physiology for Schools* (Philadelphia, 1840), no mention; Calvin Cutter, M D (1845) *Anatomy and Physiology Designed for Academies and Families* (Boston, 1845), pp 106-8, 113-16, Edward Jarvis, M.D. (1847), *Primary Physiology, for Schools* (Philadelphia, 1848), pp 26-28; T. S Lambert, M D (1850), *Hygienic Physiology* (Portland, Me , 1851), pp. 109-13

[5] Phyllis A. Richmond, "The nineteenth century American physician as a research scientist," in Felix Marti-Ibañez (ed.), *History of American Medicine A Symposium* (New York. M D. Publications, 1958)

[6] With exceptions, public and private support for medical education was restricted to buildings and equipment in the period 1790-1840 and to hospital construction between 1840 and 1875. See E. C Atwater, *Financial subsidies for American medical education before 1940* (Master's Essay, Johns Hopkins University, Baltimore, 1974)

and the enthusiastic support of his superior. Such assets were rare among would-be investigators before 1870.

This paper will describe the attitudes and activities of the American community regarding experimental physiology before 1870, by which time continuing activities in this area were becoming established under the leadership of men like John W. Draper (1811-82), John C. Dalton, Jr. (1825-89), Henry P. Bowditch (1840-1911), S. Weir Mitchell (1829-1914), Austin Flint, Jr. (1836-1915), and Henry Newell Martin (1848-96). It will consider some of the intellectual, social, economic, and technological factors which determined those attitudes and activities. It will make use especially of medical student dissertations, and of the introductory and valedictory addresses given annually at most medical colleges, in order to provide a chronological record of the change in the activities and opinions of physicians and an understanding of the factors which retarded development of this basic medical science before 1870.[7]

Experimental physiology, with animals or human subjects, was a common activity of medical students at the University of Pennsylvania and an occasional one at other schools at the end of the 18th century and in the very early part of the 19th century. In 1801, for example, seven of the nine dissertations submitted at Pennsylvania included original experiments and, in 1803, eight of sixteen did so. Of the 322 theses published by 1821 and listed in

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[7] For the history of American medical research see (1) S Weir Mitchell, "American progress of medical science. Report on the progress of physiology and anatomy," *N Amer. Med & Chir Rev* , 1858, *2* 105-119, (2) Henry Sewall, The beginnings of physiological research in America," *Science*, 1923, *58* 187-95; (3) Walter J Meek, "The beginnings of American physiology," *Ann Med Hist* , 1928, *10* 111-25, (4) Wm H. Howell, "The American Physiological Society during its first twenty-five years," in: *History of the American Physiological Society, Semi-Centennial 1887-1937* (Baltimore 1938) (5) Richard H Shryock, *American Medical Research Past and Present* (New York Commonwealth Fund, 1947); (6) Morris C Leikind, "The evolution of medical research in the United States," *Internat. Rec Med.*, 1958, *171* 455-468, (7) Carl J Wiggers, "The evolution of experimental physiology at the University of Michigan and Western Reserve University, *Bull Cleveland Med. Lib.*, 1963, *10* 5-18; (8) Richard M Jellison, "The American physician as scientist in the nineteenth century: an interpretation," *21st International Congress of the History of Medicine* (Siena, 1968), pp 1397-1409 (9) Wyndham D Miles, "Introductory lectures in nineteenth century American chemistry courses," *University of Pennsylvania Library Chronicle*, 1971-72, *37-38*. 143-50, (10) Karl E Rothschuh, *History of Physiology*, transl Guenter B Risse (Huntington, N Y.. Krieger, 1973), 379 pp The American scene in the 19th century is discussed, pp 187-90, (11) Harry Bloch, "Medical research and education in nineteenth-century America," *New York State J Med.*, 1974, *74* 1071-74; (12) Clark A Elliott, "The American scientist in Antebellum society a quantitative view," *Social Studies of Science*, 1975, *5*· 93-108

Austin's bibliography, at least 42 presented original work, most of which was done at Pennsylvania before 1807.[8]

Among the studies using experimentation, interest was high in how drugs worked, in whether or not the skin had an absorptive function, and in how the process of digestion worked. There were also experiments dealing with respiration and carbon dioxide, with suspended animation and resuscitation, and healing of wounds.[9]

The earliest thesis which included original studies was that of Adam Seybart (1773-1825) in 1793. Seybart, a student of Caspar Wistar and later member of Congress, wanted to find out whether the blood of sick animals was putrid as claimed by Hermann Boerhaave (1668-1738) and others, or not as believed by John Brown (1735-88) and his followers. Finding that the subject "had never been put to the test of fair experiment; but, that speculative reasoning (too often delusive at best) and a few indecisive facts, were the chief supports on both sides," Seybart performed 23 experiments on a bitch, injecting various materials intravenously and concluding against the doctrine of the putrefaction of the blood of living animals. All went well until what turned out to be the final trial in which a "drachm of pus, diluted with a small quantity of clear pump water, was injected into one of the femoral veins." The pus had come from a skin lesion two days earlier. The bitch had two violent fits, looked moribund but was afebrile, and died several hours later.[10]

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[8] Dissertations were required for graduation at most American medical schools, including country schools, until late in the 19th century Prior to 1789 they were done in Latin and occasionally for a few years after this date. The essay had to be in the student's own handwriting and at some schools read and defended publicly Rush Medical College gave it up in the 1870s, Dartmouth, University College of New York, Jefferson, and Harvard abandoned the thesis requirement in the 1880s or early 1890s Pennsylvania required a thesis as late as 1893. Yale still requires one.

In the early years most were published by the authors The 322 theses published before 1821 make up 15% of all Austin items, and 26% of publications between 1788 and 1807. These published theses came mostly from the University of Pennsylvania (217), but also from Columbia (34), Harvard (12), Dartmouth (2), Queens (6), Maryland (32), College of Physicians and Surgeons (NY) (19), and Transylvania (1) After 1806, the number of Pennsylvania theses actually published declined sharply, probably because of the number of students became so great. From 1800 to 1806 there were from 10 to 24 Pennsylvania graduates each year and essentially all dissertations were published. After 1808 there were over 60 graduates each year but only 20 Pennsylvania dissertations were published between 1807 and 1820.

Students at the University of Pennsylvania did the most experimental work, with the University of Maryland, Columbia, Harvard, and the College of Physicians and Surgeons of New York all poor seconds in this respect. Among 1881 Transylvania theses there were few, if any, with experimental work according to Professor Eric H. Christianson of the History Department and Miss Roemol Henry, Curator of Special Collections Mrs. Mary H Hughes in charge of Special Collections at Bowdoin College has a similar impression of the 40 bound volumes of medical theses done at Bowdoin (1821-70)

Mrs Lois Hely examined catalogue cards of theses done at the University of Pennsylvania medical school between 1807 and 1860, few of which were published. Card titles for theses done between 1830 and 1860 are so brief as to be unreliable in judging whether or not experimental work might be described. Among a total of approximately 1000 theses done in the 1807-30 period six titles contained the words "experimental" or "research" or some variant Each of these theses was examined as were five others which reported experimentation and which were discovered in a random search Only one of these eleven theses described experiments on animals; most reported effects of drugs or treatments on the author and fellow students. This survey, though limited in scope, suggests that experimental investigations by medical students at the University of Pennsylvania declined sharply after 1807 when the number of students increased greatly.

[9] Of 322 published theses listed in Austin's bibliography, one-third dealt with disease or abnormalities, including various fevers (25), dysentery (13), croup (6), consumption (6), cholera (3), dropsy (6), apoplexy (5), angina pectoris (2), and epilepsy (2). Twenty-two were of an obstetrical or pediatric nature, including 6 on puerperal sepsis, but only one on conception. Eighteen were on surgical subjects and four on eye problems. Another large group (63) was made up of studies of therapeutic modalities, including botanical (35), elemental (9), and molecular (5). At least a quarter of these were experimental Opium, digitalis, and mercury were the most commonly studied drugs. The third major category might be labeled physiological and included studies of absorption from the gastrointestinal tract or integument of various substances including medicaments, of digestion; of respiration; of perspiration, of biliary function; and of neurological subjects, including sleep and

The following year Edward Cutbush (1772-1843), who became the pioneer of American naval medicine and later professor of chemistry and dean of the Geneva Medical Institution, did a study in which he reported autopsy examination of the brains of six insane people.[11] That same year William Bache, grandson of Benjamin Franklin, studied the effect of carbon dioxide on healthy insects, frogs, birds, and dogs:

> When first the animal is immersed in fixed air [$CO_2$], he makes forcible exertions to relieve himself, but almost immediately becomes apparently stupid, and so much under the effects of debility, that he lays down as if asleep, or rather in a apoplectic fit; all voluntary motion is then greatly diminished or disordered, and sensation is so far impaired, that the animal will patiently suffer to be pinched without appearing to feel any uneasiness and continues for a short time.

The experiment terminates with labored respiration, convulsions, incontinence, strong crural pulses and death.[12]

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animation Studies of the blood were unusual (James P. Morris, in his dissertation, *Blood, bleeding and blood transfusions in the mid-nineteenth century American medicine*, Tulane, 1973, lists only three theses on blood prior to 1820 After 1840 they became quite common ) At least a quarter of this physiological group were experimental One other identifiable group included 17 theses which might be called psychological or psychosomatic, included were such subjects as the effect of the mind on the stomach, effects of grief and fear, effects of the passions on therapy, the effects of habits on disease

[10] Adam Seybart, *An inaugural dissertation being an attempt to disprove the doctrine of the putrefaction of the blood of living animals*, Philadelphia, 1793, 78 pp Also published in Charles Caldwell, *Medical Theses selected from among the inaugural dissertations, published and defended by the graduates in medicine, of the University of Pennsylvania, and of other medical schools in the United States* (Philadelphia, 1805) Caldwell intended this to be an annual publication Though only two volumes appeared (1805 and 1806) it emphasizes the importance that one leading physician put on these student efforts. Caldwell saw it as a public forum for new work The selection emphasized experimental studies Wrote Caldwell, "On subjects where experimentation and observation can be had recourse to, as tests of truth, mere hypothesis will be admitted with caution" [p X].

[11] Edward Cutbush, *An inaugural dissertation on insanity*, Philadelphia, 1794, 53 pp

[12] William Bache, *An inaugural experimental dissertation* , Philadelphia, 1794, 72 pp. The atmospheric gases carbon dioxide, hydrogen, nitrogen, and oxygen were each identified between 1757 and 1775 and by the 1780s Lavoisier had determined that oxygen and carbon dioxide were interchanged in the lungs. It was

Several students undertook studies of digestion.[13] In 1796, John Wilson experimented on himself and on a man in the House of Employment. On several occasions, each ate a meal, took tartar emetic three or four hours later, and analyzed and distilled the vomitus obtained. Wilson found acid but was unsure whether it came from the food or the stomach. Finding no spirits, he concluded that fermentation was not part of the digestive process. Four years later, Joseph Glover (1778-1840), a Pennsylvania student from Charleston, studied the effects of saliva, gastric fluid, gastric mucous, bile and pancreatic juice on various victuals.[14]

John R. Young (1782-1804) of Maryland went a step farther in 1803, replacing *in vitro* studies with those done *in vivo* and identifying with litmus the acid nature of his own vomitus and the gastric juice of a frog. Young noted that the esophagus of the common bull-frog was so wide that food could "be examined at pleasure," much as Beaumont later did through a gastric fistula. He observed that linen bags of peas and beans and wheat remained unchanged in a frog's stomach after 30 hours and would, in fact, sprout after twelve days. On the other hand, if the grain was mashed before being put into the frog's stomach, or if the bread were introduced, digestion took place. All of which made Young wonder if some "living principle," such as John Hunter had used to explain the stomach's resistance to self-digestion, did not protect the whole seed. This conviction was strengthened by finding that a half-ingested smaller frog, lassooed by a thread which allowed its retrieval after 1 1/2 days, was intact if alive but caudally digested if dead.

In other experiments Young introduced human bladder stones into frog stomachs and, on finding that dissolution occurred, proposed that gastric juice be injected into the bladder of patients afflicted with the stone. Though Young mistakenly thought gastric acid to be phosphoric, he did recognize its

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not until 1837, however, that Gustav Magnus explained that the body tissues were the source of carbon dioxide and the consumer of oxygen in the process of respiration. Garrison felt that the theory of respiration was the most important contribution to 18th century physiology. Henry Hill Hickman, an English surgeon, reported the use of $CO_2$ as a surgical anesthetic for animals in 1824

[13] Until the middle of the 18th century the stomach had, in the words of William Hunter, been thought of as a mill, a fermenting vat, or a stew pan In 1752 Réaumur isolated and demonstrated the solvent effect of gastric juice Thirty years later, Spallanzani demonstrated that gastric juice was not only solvent but also antiseptic, a fact which made it unlikely that digestion was by "concoction, putrefaction, trituration, or fermentation" and that it was probably a matter of chemical solution The acid nature of gastric juice had not yet been recognized.

[14] John Wilson, *An inaugural experimental dissertation on digestion*, Philadelphia, 1796 Joseph Glover, *An inaugural experimental inquiry being an attempt to prove that digestion in man, depends on the united causes of solution and fermentation*, Philadelphia, 1800, also in Caldwell, *op cit.* (n. 10 above), 1806 A later more elegant study was done by Luke Douglas at the College of Physicians and Surgeons in New York (Luke Douglas, *An experimental inquiry into the function of the liver* , New York, 1816, 59 pp ) Douglas, who spoke of himself as a physiologist and had studied in Dublin, observed bile flowing into the opened duodenum

acid nature and determined that saliva and gastric juice are secreted simultaneously.[15] His work had attracted favorable attention of the editor of the *Medical Repository* who called his experiments "well-devised" and felt that they confirmed the presence of a vital spirit in animals which resisted gastric fluid.[16]

Closely allied to studies of digestion were those of gastrointestinal absorption. In 1798, Pennsylvania student John Hahn had studied the use of enemata to introduce medicines into the body as well as to hydrate patients. Work in absorption, however, was limited by the state of chemical knowledge and the lack of sensitive methods of assay. Many of the substances which could be identified in blood or tissues were toxic to animals or tissues, at least in the doses used. For example, Benjamin Hodge gave dogs variously ferrous sulphate, lead acetate, mercury, or the astringent nutgall and found it necessary to ligate esophagus or bowel to prevent vomiting or expulsion of enemas. When he was unable to detect the substance given in the chyle of the sacrificed animal he concluded that absorption had not occurred.[17]

Studies of skin function were also popular. Since the time of Hippocrates it was believed that various substances could be absorbed by the skin. Between 1800 and 1805, several medical students at the University of Pennsylvania tried to settle the matter experimentally. John Baptiste Clement Rousseau was credited by later workers as being "the first among us to institute a systemic examination of the question" and concluded that the skin did not possess the capacity to absorb. Subsequent workers including Hodge, Walmsley, Klapp, and Daingerfield all supported this conclusion by what a later investigator called "ingenious and interesting" but, as it turned out, misleading experiments, and their erroneous conclusions "gained great strength in this country."[18]

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of a dog, cannulated the common duct, weighed the bile, and reasoned that most of the bile must be reabsorbed since it weighed many times more than normal fecal output.

[15] John Richardson Young, *An experimental inquiry, into the principles of nutrition, and the digestive process* , Philadelphia, 1803 Also in Caldwell, *op. cit* , 1805. That gastric acid is hydrochloric was reported in 1824 by William Prout

[16] *Medical Respository,* 1804, *7*: 101. Young's thesis and the subject of digestion is considered more thoroughly in Donald G Bates, "The background to John Young's thesis on digestion," *Bull Hist Med.*, 1962, *36*, 341-61

[17] John Hahn, *Observations and experiments on the use of enemata and the external application of medicines to the human body*, Philadelphia, 1798 Benjamin Hodge, *Experiments and observations on the absorption of active medicines into the circulation*, Philadelphia, 1801; also in Caldwell, *op. cit* (n. 10 above), 1806

[18] John Baptiste Clément Rousseau, *An inaugural dissertation on absorption*, Philadelphia, 1800, Benjamin Hodge, *op cit,*; Thomas Walmsley, *An essay on glandular appetency, or the absorption of medicines*, Philadelphia, 1803, Joseph Klapp, *A chemico-physiological essay, disproving the existence of an aeriform function in the skin, and pointing out, by experiment, the impropriety of ascribing absorption to the external surface of the human body*, Philadelphia, 1805, Henry Powen Daingerfield, *An experimental essay on*

Joseph Klapp, a student from Albany, New York, exposed the skin of dogs and human subjects to a variety of substances including turpentine, garlic, camphor, and asparagus and concluded that since the appropriate characteristic odor did not appear in the urine or on the breath absorption had not occurred. When dogs inhaled turpentine vapors the distinctive odor of violets appeared in the urine but when only the skin was exposed the odor was absent. A typical experiment used a tracheotomized dog with ligated urethra. When the animal was sacrificed seven hours after exposure of its skin to turpentine, the vapor of which was excluded from the inspired air but which was blown repeatedly into the upper airways, the urine lacked any hint of violets. Only when turpentine vapors reached the lungs was the urine violaceous. This and similar experiments led Klapp and the others to conclude against the doctrine of the skin being an absorbent organ.[19]

In 1809, Reuben D. Mussey, a University of Pennsylvania medical student from Massachusetts and later professor of anatomy and surgery, first at Dartmouth and later for many years at the Medical College of Ohio, reversed this conclusion. Mussey soaked his bare feet in an infusion of madder for ten hours, found madder in his urine, and concluded that the skin must be absorbent. Three years later, inspired by what he called these "bold and original experiments," a Harvard student, Thomas Sewall, who later became professor of anatomy and physiology at Columbian University in Washington, D.C., extended Mussey's studies to other materials and other parts of the body.[20]

That same year, Jacob Bigelow, recent medical graduate of Pennsylvania (1810) and later long-time member of the Harvard medical faculty, won the Boylston Prize for his study of the treatment of injuries caused by fire. Bigelow studied the healing of scalded rabbit ears on which different forms of therapy were used, thereby introducing controls into his procedure. Another example of experimental control, using human subjects, was provided by James Tongue of Maryland. Attempting to show the mode by which mercury cured syphilis, he inoculated Matthew Wootton, a fellow medical student at the University of Pennsylvania, on each arm with material from a fresh chancre. The inoculation on one side had been mixed with calomel and

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*cutaneous absorption*, Philadelphia, 1805. The essays of Klapp and of Daingerfield may also be found in Caldwell, *op. cit.* (n. 10 above), 1806. The quotations are of Thomas Sewall, *New Eng. J. Med. & Surg* , 1813, *2*. 30-32

[19] Joseph Klapp, *op. cit*

[20] Reuben D. Mussey, *Experiments and observations on cutaneous absorption*, Philadelphia, 1809 Thomas Sewall, *op. cit.* (n 18 above)

produced no chancre, but on the other a chancre appeared. What happened to Wootton is anyone's guess; he did not graduate.[21]

Student investigators encountered not only problems in experimental design, technical limitations, and incurred perhaps unrecognized risks, they also had to contend with authority. When Enoch Hale, then (1813) a medical student at Harvard and later an attending physician at the Massachusetts General Hospital, did some experiments "on animal heat as connected with respiration," studies which were similar to those of the British physician Sir Benjamin Brodie, he incurred the displeasure of Mr. Brodie who complained that the experiments "were not exact repetitions of his, and therefore not entitled to much consideration in estimating the causes of animal heat." To this, Hale replied that, while "it could never be doubted that experiments performed . . . by a gentleman of such distinguished character, were perfectly correct in all their details," he nevertheless felt that repetition of experiments by others and with slight variations could only "lead to additional information."[22]

Though the editor of one prominent medical journal expressed the opinion that the purpose of experimentation by students was simply to make them better physicians rather than to expand knowledge or to train investigators, the students, judging from their comments, felt otherwise. Wrote one, "the man, who would attempt to teach, or write, on subjects connected with physiology, without the aid of facts and experiments, had better relinquish the matter altogether." Others wanted "to contribute [their] mite towards . . . the advancement of science," or "cast [their] portion into the scale of science." Still another hoped that his work would stimulate a whole series of experiments since, once involved, he found his subject more extensive than he had hitherto appreciated.[23]

The number of Pennsylvania medical students who undertook experiments around the turn of the 19th century is impressive, especially in view of the impediments. Students recognized that "the most trivial circumstances

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[21] James Tongue, *An inaugural dissertation. .* , Philadelphia, 1801; also in Charles Caldwell, *op cit* (n 10 above), 1806 p 354. Tongue also inoculated himself but only with inactivated material Various types of student co-operation were common One student, upon discovering that a class-mate was already studying the "sedative power" of digitalis on the pulse-rate, gave the report of four experiments he had performed on the same subject to his friend and chose a new topic for his own dissertation (cf John Moore, *On digitalis purpurea, or fox-glove* Philadelphia, 1800)

[22] Enoch Hale, *Experiments on the production of animal heat by respiration*, Boston, 1813

[23] *Medical Repository*, 1804, 7 101; Benjamin Hodge, *op cit* (n 17 above), Adam Seybart, *op. cit* (n 10 above), Joseph Glover, *op cit* (n 14 above), Thomas Horsfield, *An experimental dissertation* , Philadelphia, 1798.

[could] influence the result of experiments'' and many were restrained by fear of failure, especially in view of the great importance of the dissertation in the eyes of the faculty. Furthermore, the difficulty of obtaining subjects to say nothing of the dismay at inflicting suffering and death on so many animals was another matter which concerned them.[24]

Those who proceeded had resolved their guilt. ''The disagreeable necessity of inflicting death on so many animals, at first rendered the subject extremely irksome; but when I reflected that these immolations were absolutely necessary to a thorough investigation, and that myriads were daily slaughtered,'' not only by nature's appetite but also to satisfy the appetites of pampered epicures as well, the goal seemed to justify the means. This very difficulty in experimenting, wrote another, was ''the reason why physiology has not kept pace with the other branches of medical science.''[25]

By the 1820s the faculty itself had become more involved in research. Nathaniel Chapman, professor of physic at the University of Pennsylvania, had been studying the nourishment of the foetus with experiments on pregnant bitches.[26] William E. Horner, professor of anatomy at the same institution, performed ten experiments with kittens and rabbits which he thought proved that the medulla oblongata was essential to voluntary respiration, and that many bodily functions including circulation were dependent on the spinal cord. In his report he reviewed the experiments, said how they were related, and told how they bore on his hypothesis.[27]

In 1821, what may have been the first sponsored experimental research was undertaken by three young Philadelphia physicians, Benjamin Coates, Richard Harlan and Jason Lawrance, who investigated the absorptive functions of veins, lymphatics, and lacteals. The team, working as a committee of the Philadelphia Academy of Medicine, reported findings of 66 experiments on 43 animals with conclusions which differed from those of John Hunter and supported the work of François Magendie and other French investigators. Another important feature was the fact that the investigators reported all their data, even when they did not seem compatible.[28] Later, Dr. Harlan[29] dropped out of the team, but the other two continued their investigations, now with the sponsorship and financial support ($100.00) of Nathaniel Chapman, who had long been interested in the subject. Ninety-six new experiments were performed in the presence of a group which included senior members of the medical school faculty. Critical observers of the day considered the procedure model, the investigations thorough, and the conclusions reasonable.[30]

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[24] Joseph Glover, *op. cit.*, William Bache, *op. cit.* (n. 12 above).

[25] William Bache, *op. cit.*, Joseph Glover, *op. cit.*

[26] Nathaniel Chapman, On the nourishment of the foetus, *Phila. J. Med. & Phys. Sci.* 1820, *I*:1-17.

[27] William E. Horner, ''Observations and experiments on certain parts of the nervous system,'' *Phila. J. Med. & Phys. Sci.*, 1820, *I*:285-99. The faculty also discussed in their lectures the experimental work of colleagues. A student notebook reporting a lecture given by David Hosack, professor of medicine, on Nov. 7, 1823, at the College of Physicians and Surgeons in New York, cites Benjamin Rush's experiments regarding fermentation of food in which he, on three successive days, vomited half-digested food after taking an emetic, and demonstrated acid in the vomitus, Alexander Stevens' studies of food which passed through the gastrointestinal tract in perforated metal balls, and J. F. Blumenbach's studies of oxygen and carbon dioxide in the lungs. (Henry Rose Notebook, 1823, Edward G. Miner Library, University of Rochester.)

Electrophysiology was in its infancy.[31] In New York, in 1824, several professors from the College of Physicians and Surgeons, Alexander H. Stevens, Valentine Mott, and William J. Macneven, performed ten experiments on the body of a man recently hanged for murder, using a galvanic battery constructed by combining 328 pairs of plates belonging to four physicians. The first experiment consisted of wrapping or "arming" the Achilles tendon with tinfoil and placing a ball in the mouth. When connected, the jaws shut and the whole body convulsed. Said an observer, "they were not like the shakings of an ague, not the tremblings of fear, but rather like a succession of startings from surprise; the sight made the spectators shudder." The cadaver could be made to kick, shake hands, and move his lips as in life. Phrenic stimulation produced slight respiratory movements and the auricles could be made to contract and the ventricles to make "tremulous motions." The observers concluded that "this single [last] experiment may be of immense benefit" to patients where "the primary object is to excite the heart to action." Though they recognized that simultaneous artificial respiration would be necessary, they thought "the heart might be made again to beat, and the whole machinery of the body set in motion. . . ."[32]

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[28] *Phila J. Med. & Phys Sci.*, 1821, *3* 273-302 The committee consisted of Benjamin H Coates (1797-1881, M D Penn , 1818), a resident at Philadelphia Almshouse; Richard Harlan (1796-1843, M D Penn , 1818), who was in charge of Dr Parrish's private dissecting room; and Jason O'Brien Lawrance (1791-1823,M D Penn , 1815) who organized the later famous Philadelphia School of Anatomy, where generations of Philadelphia surgeons learned dissection and where "students of medicine engaged in experimental inquiries in Physiology . free of expense" during the summer recess *Phila J Med & Phys Sci* , 1825, *10* 219

[29] For a collection of Dr Harlan's research, most of which was zoological and paleontological, see his *Medical & Physical Researches or original memoirs* (Philadelphia, 1835)

[30] *Phila J. Med & Phys Sci.*, 1823, *7*· 376-98

[31] John C Dalton (1825-1889), professor of physiology at the College of Physicians and Surgeons in New York (1855-69), recounts the beginnings of electrophysiology in his book *The Experimental Method in Medical Science* (New York, 1882 Second course of Cartwright lectures) Luigi Galvani (1793-98), professor of anatomy at Bologna, discovered in 1789 that an electrical charge could make a frog's leg contract, and later observed that the two-metal arc or battery produced what we know as a galvanic current. Alessandro Volta (1745-1827), professor of physics at Pavia, elaborated this idea into the Voltaic pile or battery which was used in the experiments cited here. The recognition of separate sensory and motor nerves was made by Sir Charles Bell in 1822 Marshall Hall described the reflex arc in 1837 Most of this early work was done on frogs, and electrophysiologic studies in a living person had not yet been attempted

By the late 1830s investigators were using the new acoustical technique of stethoscopy. At Blockley Hospital, attending physician Caspar Pennock, and one of his residents, Edward Mott Moore, studied the relationship between the sounds and mechanical function of the heart in sheep and calves. The animal was rendered unconscious by a blow on the head and respired by means of a bellows inserted in a tracheotomy. The chest was opened and a stethoscope placed directly on the heart in order to correlate what was seen and heard. After 16 experiments the investigators were able to draw conclusions which confirmed the findings of English physiologists on important and still-debated questions. As crude as such studies seem to us today, they employed the most advanced technology of their time and they remained the major American contribution on the subject for many years.[33]

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[32] *The Medical Recorder,* 1824, *7*: 639-40 Reports of experiments on cadavers are few In 1833, William E. Horner (1793-1853), professor of anatomy at the University of Pennsylvania, studied the vascular connection between mother and foetus by injecting the aorta of a recently dead 24-year-old pregnant woman with one gallon of saturated potassium prussiate followed by one gallon of iron sulfate. None of the resulting Prussian blue was found in the umbilical cord. (*Am J Med Sci* , 1833, *12*· 353-4). Washington L Atlee (1808-78), surgeon and gynecologist of Lancaster, Pennsylvania, and later professor of medical chemistry at Pennsylvania College in Philadelphia, reported "A series of experiments made by the medical faculty of Lancaster upon the body of Henry Cobler Moselmann, executed in the jail yard of Lancaster County, PA on the 20th of December, 1839" (*Am J. Med. Sci* , 1840, *26*: 13-34). Published a year later were "Experiments on the body of Morris, executed January 15th, 1841 at Philadelphia, with the post-mortem examination" (*Med. Examiner* [Phil ] 1841, *4* 58-61). Because of the often hideous spectacle which cadaver experimentation provided, use of executed convicts was forbidden in Prussia after 1804 (F H Garrison, *An Introduction to the History of Medicine* [Philadelphia,, Saunders 1929], p. 328)

[33] David Williams, physician to the North Dispensary in Liverpool wrote "On the sounds produced by the action of the heart"; his observations were based on a correlation of acoustical data with anatomic changes found at autopsy (*Edinb Med & Surg J.*, 1829, *32* 297-305) The earliest vivisection experiments comparing acoustics with function were done by James Hope with Henry James Johnson in 1830, using rabbits Hope and Charles J B. Williams both later worked with asses which had been poisoned with woorara and were artificially respired with bellows through tracheotomies. These heart-lung preparations lasted about an hour (See James Hope, "Experiments illustrative of the causes of the sounds of the heart," *London Med. Gaz* , 1835, *16*. 774-7; Charles J.B Williams, "Remarks on Dr. Hope's conclusions from certain experiments on the sounds of the heart," *ibid.*, pp. 818-20.) Hope's book, *A treatise on the diseases of the heart* . . (2nd American edition edited by C.W. Pennock, Philadelphia, 1846) contains a whole chapter on these experimental researches (pp 33-82) as well as a republication of the report of work done by Pennock and Moore (pp. 59-65). The latter was available originally in pamphlet form. Caspar W Pennock and Edward Mott Moore, *Report of Experiments on the Action of the Heart* (Philadelphia, 1839). Later claims to priority by C J B Williams appear in *Brit. Med. J.*, 1876, *2* 421, and 1877, *1* 127-30, 768. An historical survey of discoveries regarding cardiovascular sound may be found in the first chapter of Victor A McKusick's book *Cardiovascular Sound in Health and Disease* (Baltimore· Williams & Wilkins 1958). The work of James Hope is mentioned especially on pages 18 and 19

The number of professional experimentalists increased in the 1840s. In addition to the workers at the University of Pennsylvania there was considerable activity elsewhere, especially in the South, and often by Pennsylvania graduates. In Louisville, in the early 1840s before anesthesia, the surgeon Samuel D. Gross studied healing of experimentally-inflicted intestinal wounds in over 100 dogs.[34] Bennett Dowler (1797-1879) in New Orleans did studies on the nervous system of crocodiles.[35] Joseph Johnson Allison (c. 1815-1840), a recent graduate from Pennsylvania (M.D. 1837), of only moderate means, was able to continue investigations in comparative physiology and microscopy and published "an interesting series of observations on the venous circulation."[36] Henry Fraser Campbell (1824-91), who graduated from the Medical College of Georgia in 1842 and continued on the faculty, proposed the idea, prior to the work of Bernard, that sympathetic nerves play a role in reflex phenomena.[37] Joseph Jones (1833-96) who was schooled at Princeton and the University of Pennsylvania (M.D. 1856) spent a lifetime in research and public health, mostly in New Orleans.[38]

In the predominantly Calvinist and commercial north there seemed to be less enthusiasm for experiment although John William Draper in New York and John F. Dalton, Jr., in Buffalo were notable exceptions. Waldo J. Burnett (1828-53), who graduated from Harvard Medical School in 1849, did enough original experimental research, especially in cellular pathology, between then and his death from tuberculosis five years later to warrant mention in both Samuel D. Gross' and Austin Flint's centennial surveys of American Medicine.[39] That he was an exception on the Boston scene is clear from the Massachusetts Medical Society's obituary notice of him. No one, it said, since James Jackson, Jr. (1810-34), who had died prematurely twenty years earlier, "had made such extensive acquirements, had thought so independently, and pushed out so far into new ground, or promised to do so much for the advancement of physiology" as Burnett.[40]

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[34] Samuel D Gross, *Autobiography of Samuel D. Gross, M D.*, 2 vols. (Philadelphia, 1887), vol 1, pp 96-98 Gross recalled that his results were acknowledged in Europe but largely ignored at home Thomas Smith had reported similar work in 1805 in his medical school dissertation *An essay on wounds of the intestines* (Philadelphia, 1805).

[35] Walter J Meek, "Bennett Dowler, a forgotten physiologist," *Ann. Med Hist.*, 1938, n s *10*. 237-46

[36] See Frederick H. Pratt, *A Forgotten Pioneer Biologist of Philadelphia and a Problem of Ex-Libris* (Philadelphia, 1934) Allison died prematurely but his teacher, Samuel Jackson, predicted a brilliant career for him had he lived and held him up as a example for other students to emulate Samuel Jackson, *Introductory Lecture to the Course in the Institutes of Medicine at the University of Pennsylvania* (Philadelphia, 1840), p 16

[37] Samuel D Gross, *Then and Now. a discourse introductory to the forty-third course of lectures in the Jefferson Medical College of Philadelphia* (Philadelphia, 1867) Campbell's claim to priority over Bernard was announced by him on March 2, 1857 The ensuing dispute between the two men was adjudicated by the British physician Dr. Marshall Hall as follows. "The idea and designation of excito-secretory action belonged to Dr. Campbell but his details are limited to pathology and observation. The elaborate experimental demonstration of reflex excito-secretory action is the result of the experimental labours of M. Claude Bernard . " (quoted in the article on Henry Fraser Campbell by Edgar Erskine Hume, *Dictionary of American Biography*, New York, 1958, *3* 453

[38] James O. Breeden, *Joseph Jones, M D , Scientist of the Old South* (Lexington. Univ. of Kentucky Press, 1975)

Though the professional investigator or experimentalist was still not common by the 1850s there was growing interest in research. By that time there was seldom an issue of the nation's leading medical journal, the *American Journal of the Medical Sciences*, which did not report experimental work—often in the lead article—and there was hardly an American physiologist who was not numbered among the authors of these studies.[41] Studies dealing with renal and pulmonary function and with digestion predominated. Further evidence of the growing interest in research is the fact that at least three of the essays awarded prizes in the 1850s by the American Medical Association dealt with experimental physiology.[42]

When the young American physiologist, Silas Weir Mitchell (1829-1914) reviewed the literature of American physiology in 1858, he found 138 papers, some dating back to 1800, which he felt made significant contributions. That few were the work of professional investigators is suggested by the fact that only 20 of the 78 authors listed had two or more citations. Among that twenty were nine from the South, four from Philadelphia, two from Boston and one each from Chicago and Cincinnati.[43]

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[39] See Waldo J Burnett, "The cell. its physiology, pathology and philosophy, as deduced from original investigations, to which is added its history and criticism," *Transact, A.M.A* , 1853 *6*: 647-832 Samuel D Gross, *Then and Now*, Austin Flint, Sr , *Boston Med & Surg J* , 1877, *95*: 321. That tissues were composed of cells had been established by Schleiden and by Schwann in the late 1830s. This recognition was fundamental to the work of Virchow and was the basis of modern pathology

[40] *Med Communic Mass Med Soc.*, 1860, ix 54-56

[41] Between 1850 and 1860 experimental work was published in the *Amer. J Med Sci* by Samuel Jackson, John W Draper, John C. Dalton, Jr., Waldo J. Burnett, William A. Hammond, S Weir Mitchell, Joseph Jones, J J Woodward, Austin Flint, Jr , Henry H Schmidt, and John Bacon, Jr

[42] Waldo J Burnett (1853), Henry Fraser Campbell (1857), William A Hammond (1857)

[43] S Weir Mitchell, *N. Amer Med & Chir Rev* , 1858, *2*: 105-28 From Philadelphia were J K Mitchell, J J Allison, C. W. Pennock, and F G Smith; from New York were John Draper, John Dalton, and Clay Wallace, from Boston were Waldo Burnett, and Edward Brown-Séquard, from Chicago was Nathan S Davis, and from Cincinnati was Reuben Mussey From the South were Bennett Dowler, M Michel, H. F. Campbell, John Harrison, Joseph Jones, J. L. Riddell, H A Ramsay, W. A. Hammond, S A. Cartwright Other surveys include S D Gross, *Then and Now* (Philadelphia, 1876); John C Dalton, *Vivisection What it is and what it has accomplished* (New York, 1867), Austin Flint, Sr , *Bost Med & Surg J* , 1876, *95* 318-21

The technical difficulties with which these early experimental physiologists had to contend were substantial. Before 1846 there was no anesthesia. The kymograph, essential to recording changing physiologic events, was not perfected by Ludwig until the late 1840s. Devices for artifical respiration and electrophysiology were crude. Chemical analysis was difficult as demonstrated by the fact that two leading American chemists—Benjamin Silliman and Franklin Bache—were unable to analyze gastric juice for Beaumont and one of them advised him to contact Berzelius in Sweden.[44] The American physiologist, Robley Dunglison (1789-1869), who collaborated with Beaumont in some of his experiments did identify hydrochloric acid in the gastric juice as did the Boston chemist, Charles T. Jackson.[45]

On top of this were other trials and risks. In 1815, a student expressed the hope that his research was of enough significance to justify the destruction of so many aminals,[46] and Samuel Gross wrote of his remorse as he saw the pleading look in the eyes of the dogs on whom he was inflicting injury. Gross also incurred the wrath of his colleagues because his dog colony, which was housed in the basement of the medical college, became infested with fleas, and the professor of chemistry found it necessary to lecture with pants tucked into boots for protection.[47] These problems were compounded by frequent premature deaths of young investigators, usually from tuberculosis or autopsy-incurred infected wounds.[48]

There were also conceptual impediments, one of which was the belief that animal life, in addition to its chemical components, was invested with a vital spirit which was beyond the comprehension of man and inaccessible to experimental proof. Indeed, such an idea had not prevented men like Harvey or Hunter from making important discoveries but it did give the theologian ultimate and perpetual authority over the scientist. The dissipation of this idea by men like Liebig and Virchow, who showed that life followed the same physical and chemical laws as inanimate matter, could only be a stimulus to investigation.

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[44] See Myer, *op cit* (n 3 above), pp. 180-87. See also, letter from Thomas Sewall, *Boston Med & Surg J*, 1833, *9* 95-6

[45] Myer, *op cit*, pp 160-61, 224-25, for a fuller account of Dunglison's contributions, see Jerome J. Bylebyl, "William Beaumont, Robley Dunglison, and the 'Philadelphia Physiologists,' " *J. Hist Med*, 1970, *25*: 3-21

[46] *New England J. Med. & Surg*, 1815, *4*· 4-20

[47] Gross, *Autobiography*, vol 1, pp 96-98.

[48] In Philadelphia, medical student John R. Young (1782-1804), who did some of the best early experimental studies of digestion, died of tuberculosis the year after graduation. Jason V. O'Brien Lawrance (1791-1830), one of the leading investigators of his time, died at 31 John D Godman (1794-1830), professor of anatomy and the man who changed the teaching of anatomy from a passive to an active process, died of tuberculosis at the age of 36 Another promising student investigator, John Joseph Allison (c. 1815-40) died at 25 as a result of a dissecting wound In Boston, the most promising medical scientists of two generations each died prematurely. James Jackson, Jr. (1810-34) of typhoid and Waldo Burnett (1828-54) of tuberculosis

How one physician's thinking changed in this matter may be traced in successive lectures to his medical students. Samuel Jackson, professor of the Institutes of Medicine at Pennsylvania, delivered sixteen introductory addresses to medical students between 1827 and 1859. In several of these he spoke of the vital spirit. In 1835, he called man "a combination of physical, chemical, and vital phenomena so blended, that it is impossible clearly to discriminate and separate them. Vitality probably, may prove to be the transcendentalism of Physics and Chemistry."[49] Almost ten years later, in 1844 this concept persisted in his thinking when he listed as one of the fundamental laws of organic life "a vital or organizing force or power, its nature unknown."[50] But thereafter his ideas of it began to change. The following year he spoke of "the complicated machinery of life, and its workings—the most perfect work, and the ever present miracle of God"—but added, with a new note of confidence, that "Nature had been interrogated and compelled to yield up her secrets."[51] In 1846, he spoke with even greater assurance that "the laws of organic life are being discovered" and added that "this is the first stage that medicine must complete before it can pass on to its terminal perfection as a positive science." He predicted that the science of organic life would be understood in a generation or two.[52]

By 1847, only three years after speaking of a vital force of unknown nature but essential to life, he had made a clear reversal. He stated that the phenomena of all organic nature are "for the most part entirely physical," that all the functions of organic life "are purely chemical" and compared the animal organic mechanism to that of a locomotive engine.[53] In one of his last speeches, entitled "the fundamental laws of organic molecular actions of the animal organisms, identical with those of rational mechanics," he spoke of physiology as dealing with the general and special laws of the living

[49] Samuel Jackson, *A Lecture Introductory to the Course of Institutes of Medicine* . (Philadelphia, 1835), p 10.

[50] Samuel Jackson, *Lecture Introductory to a Course on the Institutes of Medicine* (Philadelphia, 1844), p.8

[51] Samuel Jackson, *Lecture Introductory to a Course on the Institutes of Medicine* . (Philadelphia, 1845), p 14.

[52] Samuel Jackson, *An Introductory Address, Delivered before the Medical Class of the University of Pennsylvania* (Philadelphia, 1846), p.8

[53] Samuel Jackson, *Lecture Introductory to a Course on the Institutes of Medicine* (Philadelphia, 1847), pp. 15, 17.

organism. "These laws," he said, "must form the basis of medicine as a science."[54] At last, understanding of the processes of animal life were believed theoretically accessible through experimental method.

Though an important impediment was now removed, serious economic barriers remained. Not only did investigators like Gross have to buy their own animals and pay for a keeper, they also had to practice medicine for a living. William Beaumont personally supported Alexis St. Martin and his family during a considerable part of the time he was performing experiments on him.[55]

The greatest hindrance of all was the absence of any means of support other than through practice. Samuel Jackson, who had complained in 1840 that no one was doing any significant experimental research in America, warned Weir Mitchell that "every experiment in the laboratory would lose [him] a patient." James Jackson, the professor of medicine at Harvard, replied to Pierre Louis' plea that Jackson's son spend several years as a resident investigating disease at the Massachusetts General Hospital, that such a course "would have been so singular, as in a measure to separate him from other men. We are a business doing people. We are new . . . there is a vast deal to be done; and he who will not be doing, must be set down as a drone. If he is a drone in appearance only and not in fact, it will require a long time to prove it so, when his character had once been fixed in the public mind . . . he would have acquired the habit of dependence on others."[56]

Said William W. Gerhard of Philadelphia, "The United States will probably never present the spectacle, so frequent in France, of men of mature age, and high scientific endowments, resisting the temptations of private practice to devote themselves to to the patient, laborious, and in a pecuniary way, profitless pursuits of medical science."[57]

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[54] Samuel Jackson, *On the Fundamental Laws of the Organic Molecular Action of the Animal Organism, Identical with Those of Rational Mechanics. An introductory lecture* (Philadelphia, 1856), p. 7.

[55] See letter from Thomas Sewall in *Bost. Med. & Surg. J.*, 1833, *9*: 95-6.

[56] Samuel Jackson, *Address to the Medical Graduates of the University of Pennsylvania* (Philadelphia, 1840), p. 15. Anna R. Burr, *Weir Mitchell, His Life and Letters* (New York, 1929), p. 72. James Jackson, *Memoir of James Jackson, Jr., M.D.* (Boston: Duffield, 1836), p. 80.

[57] Quoted in Lawrason Brown, *The Story of Clinical Pulmonary Tuberculosis* (Baltimore: Williams & Wilkins, 1941), p. 177. This theme is elaborated more fully in an editorial on "American Medicine" in the *Philadelphia Journal of the Medical and Physical Sciences* (1824, *9*: 404-5) which says that the condition of European society is different from American and that it "offers the greatest inducements and rewards to those who are ambitious of professional distinction, and necessarily enables many to attain the highest standing, who otherwise, with all their talent, would have been obliged to expend their time in search of mere subsistence. The reverse of this condition has exerted its full influence here, where there are very few offices of emolument, no governmental patronage, and alas! no opportunities of distinction afforded by private

Oliver Wendell Holmes told medical students who wished to devote their lives "to the lofty pursuits of science for its own sake . . . take down your sign, or never put it up." We admire the achievements of men like Louis Agassiz, Jeffries Wyman, Charles T. Jackson and other "distinguished doctors who do not practise. But we say of their work and of all pure science . . . it is very splendid but it is not a practising doctor's business."[58] Members of the American medical profession had not forgotten the lesson of William Harvey who, considered crack-brained as a result of his work on the circulation, lost a large part of his income from practice.[59]

Without subsidy, the man who wanted to do experimental work had to have private income or support his activities through practice. Though one basic science chair, that in pathological anatomy at Harvard, was endowed before 1870, its incumbent (J. B. S. Jackson) was an observer and classifier, not an experimentalist.[60] Weir Mitchell, on the other hand, had to do his experimental work in his spare time. "It was my habit," he wrote, "to get through work at three or four o'clock; to leave my servant at home with orders to come for me if I was wanted, and then to remain in the laboratory all the evening, sometimes up to one in the morning, a slight meal being brought me from a neighboring inn."[61] Only after the Civil War did private endowment come to the support of research.

It cannot be said that public opinion led the way. Though popular interest in matters of health had started to increase in the 1840s, there had been a concurrent decline in the status of the medical profession. Popular books

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munificence While the physicians of our country are engaged in the labors of professional life for the support of their families, little leisure can be allowed them for the cultivation of particular branches of study, and still less for the more general acquisition of ornamental learning A man, fatigued with the details of practice, and whose time is never at his own disposal, can rarely do more than keep himself acquainted with the existing condition of medical science, and hence, instead of engaging in the composition of original works, he is forced to be content to read such as are proffered by those whose business is that of publication " The editor goes on to decry European "criticisms on the slightest [American] attempts to throw off a bondage produced more by our necessities than their talents "

[58] Oliver Wendell Holmes, *Medical Essays, 1842-1882* (Boston, 1899), p 297-8.

[59] Mentioned in C.E Brown-Séquard, *Advice to Students An address delivered at the opening of the medical lectures of Harvard University, Nov 7, 1866* (Cambridge, 1867), p 10. The source of this claim may be the statement by Harvey's friend John Aubry. "I have heard him say that after his Booke of the Circulation of the Blood came-out, that he fell mightily in his Practize and that 'twas beleeved by the vulgar that he was crack-brained; and all the physitians were against his Opinion and envyed him (Geoffrey Keynes, *The Life of William Harvey* [Oxford Clarendon, 1966], p 435)

[60] Austin Flint, Abstract of the address on medicine delivered before the International Medical Congress at Philadelphia, 1876 *Boston Med. & Surg J* , 1876, *95*·318-21, p 319

[61] Anna R. Burr, *op cit* (n 56 above), p 73

and public lectures on anatomy and physiology,[62] appealing regimens such as the masochistic steaming and puking of Thomsonianism for the poor, or the Water-Cure's douches and drinks of cold sulphur water for the rich, or the more benign homeopathy, each with its simple veneer of philosophical principle, pleased the public. Even the fact that two of the three hundred thousand Union soldiers who died in the War did so from disease unrelated to wounds (in the South it was 150,000 out of 200,000) did not get action.[63] After all, half of those born died before the age of five.

The War did one thing, however; it thrust the Surgeon-General and the Army Medical Corps into prominence. With money and organization in the post-war years, it was able to act as an institutional sponsor for its own research activities before the universities got going. Men like William Hammond, George M. Sternberg, Walter Reed, and John S. Billings, were leaders in American medicine. And had it not been the Army that had provided the salary and Joseph Lovell, then Surgeon-General, the encouragement which had made Beaumont's studies possible?

But it was the development of much larger amounts of disposable wealth after the Civil War which made endowment for research possible. With the mystery of vital force dispelled, man's evolutionary background established, the literality of the Bible challenged, and faith shaken in a God which could permit the carnage of the Civil War, the power of the church in society started its long decline. Gradually the scientist replaced the priest as the man thought to have the answers.[64]

This change in allegiance was reflected among philanthropists. In 1880, 142 theological schools had endowments of $8 1/2 million while medical schools had less than $200,000 in capital funds. By 1913, however, annual gifts to medical schools surpassed those to seminaries for the first time and by 1926, seventeen medical schools each had more than $1,000,000 in endowment and fourteen state schools had annual appropriations of at least $50,000.[65] Perhaps George Eastman, producer of the Kodak camera and flexible dry film, best described why this change took place. Speaking of dental dispensaries, he said, that by supporting them, he "could get more results for [his] money . . . than in any other philanthropic scheme [he] had investigated."[66] He later gave even more money to found and endow a medical school. It was such philanthropy which made possible the building and operation of laboratories at medical schools in which salaried investigators might work and medical students participate in experimental activities.

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[62] Reynolds (1833), Alcott (1834), Combe (1834), Hayward (1834), Comstock (1836), Lee (1838), Griscom (1839), Coates (1840), Cutter (1845), Jarvis (1847), Lambert (1850), *Bost. Med & Surg J*, 1840, *22* 289-90

[63] U S Army, Surgeon-General's office, *The Medical and Surgical History of the War of the Rebellion*, part II, vol. I, Medical History (Washington, 1888), p.1

[64] The belief that science might solve the problems of society was already being expressed by the French philosopher Auguste Comte (1798-1857) in the 1840s Samuel Jackson hailed the ideas of Comte in his 1856 Introductory address.

[65] See E.C. Atwater, "A modest but good institution and besides there is Mr Eastman," in *Too Each His Farthest Star, University of Rochester Medical Center, 1925-1975* (n.p , 1975), pp. 27-8

Scientific activity by physicians has a long history. The clinical observations of Hippocrates, or Heberden, or Sydenham were systematic attempts to collect and analyze data. This observational activity reached a peak in medicine in the mid-19th century with the efforts of medical societies to promote the identification and classification of presumably regional diseases and the flora which might offer remedies for them; with the routine dissection of cadavers by medical students who thereby became active participants for the first time; and with the stethoscopic study of bodily acoustics which, when correlated with morphological changes found at autopsy, made it possible to predict accurately abnormal conditions in the living patient. This was Baconianism carried to the nth degree. It was systematic, thorough, orderly, but it was not experimental. The observer did not participate with hypothesis or experimental design. Nature designed the experiment. Another generation of teachers and students would be more inclined to hypothesize and experiment.

The senior James Jackson, almost 60 when his student Oliver Wendell Holmes returned from learning stethoscopy in Paris, found his ears too old and trained too late for stethoscopy and often depended on Holmes to do his listening for him. But when the next generation came—those who had studied abroad with experimentalists like Bernard, or Ludwig, or Liebig, or Virchow—Holmes was now of the older generation and he felt uneasy with the newer experimental techniques. In 1862, still professor of anatomy and physiology at Harvard, he wrote to the young experimentalist Weir Mitchell that he "liked to follow the workings of another mind through these minute teasing investigations, to see a relentless observer get hold of Nature and squeeze her, until the sweat broke out all over her and Sphincters loosened—but I could not bring my own mind to it."[67]

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[66] Harvey J. Burkhart, "Centennial history of dentistry in Rochester," *Roch Hist Soc Publ Fund Series*, 1934, *13*·281-320, p.295

[67] Anna R. Burr, *op. cit.* (n 56 above), pp. 82-83

Samuel Jackson voiced similar if less colorful sentiments. In his first lecture as professor of the Institutes in 1835, he told his students:

> I entertain a distrust of my abilities to do justice to the important questions constantly arising for investigation and determination in this department of medicine, but I feel, that the occupations of a laborious practice, and a feeble constitution, do not enable me to engage in the extensive researches and experimental observations essential to their elucidation. The leisure is also wanted for that calm and deliberate observation and reflection on phenomena, so necessary to the establishment of great truths. . . .[68]

Others simply shared the view of Henry H. Smith, professor of surgery at Pennsylvania, who chided Weir Mitchell over his experiments. "What nonsense to bother yourself about snake poisons."[69]

Contrast the words of Holmes or Samuel Jackson with the complaints of still another generation. Richard Cresson Stiles (1830-73), complained in 1861 that the student "has not at command the instruments of research; and, what is worse, the taste for accurate investigation has not been acquired. . . . If physiological laboratories were founded, and a course of instruction in them demanded as a condition of graduating throngs of trained observers would leave our schools. . . ."[70]

Charles-Edward Brown-Séquard, who taught and worked briefly at Harvard in the early 1860s, perhaps hoping to introduce experimental physiology to Boston in spite of old Professor Holmes, could not "understand how anyone can have a clear idea of the rhythmical movements of the heart, or of the peristaltic movements of the bowels . . . if he has not studied these important phenomena with help of several of his senses, and especially his sight. . . . I would urge you, therefore to make good use of those low creatures, endowed with so little sensitivity,—the frogs, the fishes, and the turtles; to which I might add the rabbits. . . ." He added that "it is not necessary to be endowed with a peculiar mental disposition to become a discoverer. . . . Train your mind to a careful observation of facts . . . train your mind to draw conclusions. . . ."[71]

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[68] Samuel Jackson, *op. cit* (n 49 above), pp 5-6.

[69] Mitchell published ten articles between 1859 and 1894 in which he reported studies of venom and vegetable poisons such as curare The most famous were those done with Edward T Reichert *Researches upon the Venom of the Rattlesnake* (Washington, 1860), and *Researches upon the Venoms of Poisonous Serpents* (Washington, 1886) For a complete list see· S Weir Mitchell, *A Catalogue of the Scientific and Literary Work of S. Weir Mitchell* (n p., n d. [ca 1894]).

[70] R Cresson Stiles, *An Introductory Lecture to a Course of Demonstrative Instruction in Histology and Pathological Anatomy* (New York, 1866), pp 3-4 As early as 1827, René LaRoche, prominent Philadelphia physician who later wrote on yellow fever, addressed the Philadelphia Academy of Medicine on the importance of applying physiology to the science of disease and urged that it should have more emphasis in medical schools (*Phila. J Med Phys Sci* , 1827, *14*·203) This same opinion was expressed by Augustus A Gould a quarter of a century later to the Massachusetts Medical Society Dr. Gould felt that advances in medicine, as in all sciences, required diligent observation of nature Disease is not a distinct entity, but a deviation from health The student, therefore, should spend the first half of his pupilage studying the structure and function of the healthy body (A A Gould, *The Annual Discourse before the Massachusetts Medical Society at Springfield, June 27, 1855* (Boston, 1855), pp 7-9

Henry Pickering Bowditch, a medical student at Harvard when Brown-Séquard gave this advice, later studied abroad with Bernard and Ludwig and returned to organize a physiological laboratory at Harvard in which students might work. Another student of Bernard, John C. Dalton, had already started his own career as an experimental physiologist at the University of Buffalo where he used vivisection demonstrations in his lectures. John C. Draper, who had become professor of physiology (1850) at the University of New York, illustrated his classic 1856 textbook of physiology with engravings of microscopic photographs, a technique which he pioneered.

By the 1870s men interested in experimental work held most of the chairs of physiology. Dalton and Austin Flint, Jr., had both joined Draper in New York. Bowditch was in Boston, Francis Gurney Smith and the chairless Weir Mitchell in Philadelphia, Henry Donaldson and Newell Martin in Baltimore. At Yale and Michigan, Russell H. Chittenden and Victor C. Vaughan held chairs in physiological chemistry. Draper and Martin were products of the dissenting University of London. Bowditch, Dalton, Mitchell and Donaldson had all worked with Bernard or Ludwig.

By the time of the Civil War there had already been a considerable amount of experimental physiology done in the United States. The idea that answers could be known through conventional scientific methods had gained acceptance; Americans were becoming less believing and more questioning. New teachers trained a generation of experimentalists. Technological innovations provided new methods and there was talk of all students having laboratory experience in physiology as they did already in gross anatomy. Philanthropists, in the hope that science could solve some of the problems of society, began to turn their support from theology and the world to come toward medicine and this world. Americans turned their attention more and more to the Church Expectant and away from the Church Triumphant. Experimental physiology became a sustained activity.

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[71] C. E. Brown-Séquard, *op cit.*, 1867, p 18

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# THE FORENSIC ROLE OF PHYSICIANS IN PTOLEMAIC AND ROMAN EGYPT

DARREL W AMUNDSEN AND GARY B. FERNGREN

In the past few decades several articles on the history of forensic medicine have appeared.[1] These usually comment on the apparent absence of a forensic role for physicians in classical Greece and proceed to a discussion of Roman forensic medicine, thus bypassing Ptolemaic and Roman Egypt for which documents exist that illustrate the employment of physicians in a forensic capacity.

It has long been common to speak of "Greco-Roman Egypt" as if it were a culture distinct unto itself which maintained a societal continuity in virtually every area, save the higher administrative stratum, and was substantially unaffected by the change from Ptolemaic to Roman rule. In the light of recent examinations and re-examinations of papyrological material, this common view has been forcefully questioned by (among others) the eminent papyrologist, Naphtali Lewis.[2] Lewis asserts that "wherever we look at Egypt in the early years of Roman rule—at the governmental structure, administration, army, taxation, liturgy, land tenure—we find an *essential*[3] change from the Ptolemaic pattern or practice."[4] He concludes that "the more our studies bring to the fore the Roman elements in the organization of Roman Egypt, the less unique Egypt appears and the more it resembles other eastern provinces of the Roman Empire."[5] He recognizes, however, that "Die Sonderstellung Aegyptens' remains a fact, but not so absolute a fact as we used to think."[6] Tempered by such considerations, we have felt it best to divide our study into two chronological areas, Ptolemaic Egypt and Roman Egypt, bearing in mind that Ptolemaic Egypt was in many respects

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[1] E g , Erwin H Ackerknecht, "Early history of legal medicine," *Ciba Symposia*, 1950-51, *11*: 128 ff.; Sydney Smith, "The history of development of forensic medicine," *British Med J.*, 1951, *1*· 4707 ff., Douglas J. A. Kerr, *Forensic Medicine*, 6th ed (London· Adam and Charles Black, 1956), 340 ff., J. Arthur Gorsky, "The history of forensic medicine," *Charing Cross Hosp. Gaz* , 1960, *58* 31 ff., Lee B Greene, "Frontiers of medical jurisprudence. historical background," *Connecticut Med.*, 1962, *26*· 1 ff

[2] Naphtali Lewis, " 'Greco-Roman Egypt'· fact or fiction?" in *Proceedings of the Twelfth International Congress of Papyrology* (American Studies in Papyrology 7, Toronto Hakkert, 1970), 3 ff

[3] *Ibid.* Lewis' italics

[4] *Ibid* , 5.

[5] *Ibid* , 14.

[6] *Loc cit.*

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distinct from the rest of the Hellenistic world and that Roman Egypt was not in every facet of its culture and institutional structure a provincial microcosm of the Roman Empire.

The student of Ptolemaic and Roman Egypt has at his disposal a type of primary source material that is both a blessing and a curse: thousands of fragments of papyri that have been preserved by the favorable climate of Egypt.[7] These consist of literary fragments, public and legal documents, and personal records, documents and correspondence. The non-literary papyri provide information for the historian that is not available in such quantity and diversity from any other area of the ancient world. But the survival of the papyri has been extremely fortuitous. E. G. Turner writes: "Almost anything may turn up, yet what is expected often does not seem to. From such finds one may argue positively but not negatively: the argument from silence is especially dangerous."[8] Additionally, since many of the papyri are fragmentary with portions of the text completely missing and other parts nearly illegible, editors sometimes attempt to restore the text. When the wording of a papyrus is formulaic, restoration can be done with some degree of certainty; in many cases restoration is based on conjecture which is always hazardous and sometimes is predicated upon erroneous preconceptions.[9] It is therefore highly desirable to be particularly circumspect when attempting to draw broad conclusions from the papyri at our disposal.

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[7] We shall use standard abbreviations for papyrological collections, a convenient table of which can be found in E G. Turner, *Greek Papyri An Introduction* (London: Oxford University Press, 1968), 157 ff

[8] *Ibid* , 50

[9] Although not directly relevant to the subject of this paper, of interest to medical historians is a rather spectacular case of dubious restoration based on a faulty reading that has provided the *locus classicus* for a misconception that has gained great currency The fragmentary text of P. Oxy 40 involves a physician (c A D 142) who petitions the prefect to grant him immunity from a liturgy As restored and translated by the editors of the first volume of the *Oxyrhynchus Papyri* (in 1898), the text has the physician say "I am a doctor by profession and I have treated these very persons who have assigned me a public burden " The prefect replies: "Perhaps your treatment was wrong. *If you are a doctor officially practising mummification, tell me the solvent, and you shall have the immunity which you claim* " The restoration of the Greek text, of which the words we have italicized are a translation, has been convincingly revised by Herbert C Youtie ("A reconsideration of P. Oxy. I 40," in *Studien zur Papyrologie und antiken Wirtschaftsgeschichte [Friedrich Oertel zum achtzigsten Geburtstag gewidmet*], ed H Braunert [Bonn Habelt, 1964], 20 ff ). After giving examples of scholars who have been led by this restored papyrus to link the practice of medicine and embalming in Egypt as an established fact (*ibid* , nn 5 and 8), Youtie proposes a new reading based on his examination of the actual papyrus The Greek text of our italicized lines as restored by Youtie yields the following translation: "Notify the strategus if you are a physician with a public practice and suitable qualifications" (*ibid* , 28 f ) Youtie also cites scholars who have construed the prefect's apparent attempt to apply "scientific proof" (i.e , by asking the question concerning the solvent) that the petitioner was in fact a physician as evidence for some type of medical licensure (*ibid*., 23) See further the comments by Naphtali Lewis ("Exemption of physicians from liturgy," *Bull Amer Soc Papyrologists*, 1965, *2*. 87 ff.)

From Ptolemaic Egypt we have evidence of a variety of physicians who enjoyed some form of official recognition by the government. An inscription from the second century B.C. attests the existence of a man designated as *exegetes* of Alexandria, who is described as chief of physicians and president of the Museum of Alexandria.[10] We have no way of knowing what his functions were as "chief of physicians" but Rostovtzeff believes that "it is safe to assume that he was head of the medical section of the Museum, which very likely consisted of members who were at the same time court physicians" and infers that he was "responsible for the royal medical service in the city of Alexandria, including the garrison."[11] Alexandria was regarded as quite distinct from the rest of Egypt, called the *chora*. We have the testimony of Diodoros Siculos, using as his source Hekataios of Abdera, that there were in the *chora* public medical officials who gave their treatment free.[12] Each *nome* (administrative division) of the *chora* seems to have had a *basilikos iatros* (royal physician), an officer of health,[13] although this title may, in fact, have applied to all members of the public health service in the *chora*.[14] Diodoros' statement that the public physicians of the *chora* were supported by tax monies is probably a reference to the *iatrikon*, or "physicians' tax"; for this we have evidence from the papyri, evidence which, unfortunately, does not cause confusion about this tax to abate. Ofelia Nanetti has devoted an article to the examination of the papyrological documents pertaining to the *iatrikon*.[15] This "physicians' tax," attested in Egypt only from the third and second centuries B.C., is considered by Nanetti to be a tax that was originally Greek and affected only the Greek cleruchs.[16] Other scholars have held that the *iatrikon* was a general tax in kind[17] and some that it was a general tax levied under the jurisdiction of the cleruchs.[18] Irrespective of its precise nature, it is probable that the *iatrikon* was used to support the medical officials of the *chora*. Whether the existence of the public medical service precluded practice by private physicians[19] or whether those who paid the tax (whether cleruchs, all Greeks, or perhaps native Egyptians as well) were provided with free medical care are questions that, given the present state of our evidence, must remain unanswered.

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[10] See M Rostovtzeff, *The Social and Economic History of the Hellenistic World* (Oxford. Clarendon Press, 1941), Vol II, 1091; P M Fraser, *Ptolemaic Alexandria* (Oxford. Clarendon Press, 1972), Vol I, 97

[11] Rostovtzeff, *op. cit* , Vol II, 1091

[12] Diodoros I, 82, 2 See Rostovtzeff, *op cit* , Vol II, 1092, Fraser, *op. cit.*, Vol I, 373 and Vol. II, 548, n 311, Louis Cohn-Haft, *The Public Physicians of Ancient Greece* (Smith College Studies in History, 42, Northampton Dept of History of Smith College, 1956), 32, n. 4, and p. 69; A G Woodhead, "The State Health Service in Ancient Greece," *The Cambridge Historical Journal*, 1952, *10* 241

[13] Fraser, *op cit.*, Vol I, 373 and Vol. II, 549, n 313

[14] Rostovtzeff, *op cit.*, Vol. I, 1093

[15] "*Τὸ ἰατρικόν*," *Aegyptus*, 1944, *24* 119 ff

[16] The cleruchs were Greek mercenaries in Egypt who had been assigned plots of land in the *chora* as a means of subsistence See Rostovtzeff, *op. cit* , Vol. I, 284.

[17] E g , Ulrich Wilcken, *Griechische Ostraka aus Aegypten und Nubien* (Leipzig and Berlin· Giesecke and Devrient, 1899), Vol I, 375 f; Rostovtzeff, *op cit* , Vol II, 1092

Did the members of the public medical service have any official functions other than providing medical care? A papyrus of the first century B.C. preserves a letter from an official, styled *archiatros*, who apparently exercised authority over the embalmers and priests of the Fayûm, which was written to the embalmers ordering them to release the body of his assistant who had died there.[20] Rostovtzeff infers from this that this medical official was responsible for ensuring sanitary disposal of the bodies of those who died in Alexandria or the *chora*.[21] This is a considerable conclusion to draw from a papyrus that, at the most, indicates that the *archiatros* in question had (or presumed himself to have) the authority to demand the release of the body. Can we also assume for the physicians of the Ptolemaic medical service a forensic role? One *a priori* might argue that, given the existence of a public medical service supported by tax monies, and given the numerous instances when expert medical testimony would have proved valuable in trials involving death or injury, the Ptolemaic administration probably availed itself of the talents of its medical officials in a forensic capacity. On the other hand, since the thousands of papyri dating from the Ptolemaic period yield no instances of physicians as expert witnesses,[22] an argument from silence could be advanced that physicians simply were not so employed. Neither of these, of course, is satisfactory; both are too facile.

It is a commonplace to speak of Ptolemaic Egypt as a highly stratified society, burdened by an immense bureaucracy, typified by "a system of professional government officials."[23] It would not be surprising to find a variety of professional experts employed by the state who, as government officials, could apply their expertise in investigatory and certificatory capacities, particularly in the legal sphere. But our expectation of finding them in Ptolemaic Egypt, based as it is on modern legal and administrative practice, is not consonant with the practice of that day. During the Ptolemaic period the only experts mentioned by our sources who were employed by the state specifically in a forensic role are the *geometrai*, the land-surveyors. The earliest reference to these officials in Ptolemaic Egypt appears in 299 B.C.[24] But why were land-surveyors employed by the state as experts while others were not, although their evidentiary talents were, from our vantage point, just as crucial to judicial procedures?[25] There is evidence that official land-surveyors existed in Pharaonic Egypt and in other ancient near-eastern cultures.[26] The possession and use of land by individuals, the taxation of land by the state and the sovereign ownership of nearly all the land of Egypt by the pharaohs and later by the Ptolemies perhaps combined to create at a very early date an interest in expert certification in this area. Although the interests of individual land-holders may sometimes have provided the incentive for boundary disputes, the settlement of the issue was of very direct concern to the state.[27]

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[18] E.g , Claire Preaux, *L'Économie royale des Lagides* (Brussels Édition de la Fondation Égyptologique, 1939), 132 ff

[19] As Fraser, *op cit.*, Vol. I, 373, supposes.

[20] S B 5216.

[21] *Op cit.*, Vol II, 1092 f ; see also Fraser, *op. cit* , Vol II, 549, n 313

[22] The only papyrus from the Ptolemaic period in which a physician appears as a witness is P Col Zen. 54 (250 B.C ), where a physician, a scribe and several others witness a deed of the lease of an estate

[23] Leiv Amundsen, "The Classical Greek background of Ptolemaic law and administration of justice," in *Proceedings of the Second International Congress of Classical Studies* (Copenhagen Munksgaard, 1958), Vol. I, 264.

If it is proper to view the absence of officially appointed experts as a jurisprudential deficiency, the roots of this absence should be sought in the conceptual basis upon which the evidentiary and probatory principles of the legal system are predicated. The law of Ptolemaic Egypt governing the Hellenic inhabitants was predominately Greek, personal rather than territorial law which was imported from their native *poleis* by the Greek immigrants.[28]

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[24] See Henryk Kupiszewski, "Surveyorship in the law of Greco-Roman Egypt," *Journal of Juristic Papyrology*, 1952, *1* 257 ff.

[25] Mention can be made of the *moschosthragistai* or *hieromoschosthragistai* who were employed as religious officials to conduct the examination that preceded the sacrifice of a bull to certify that the animal was not an *apis*, a consecrated bull. See Kupiszewski, *op. cit.*, 259 f. This was an office that can be traced to pre-Ptolemaic times (see Herodotos II, 38). The forensic role of these officials was only potential, placing them in a class quite distinct from the *geometrai*.

[26] See chapter two of O. A. W. Dilke, *The Roman Land Surveyors* (New York: Barnes and Noble, 1971).

[27] Although less than in the case of the *geometrai*, the interests of the state can also be seen in the functions of the *moschosthragistai* or *hieromoschosthragistai*.

[28] An important qualification should be added here. We are using the terms "personal and territorial law" in a sense considerably more limited than that in which they are typically applied, e.g., to early medieval Germanic law. The element of personality in Ptolemaic law was much more conceptual than functional. As Hans Julius Wolff writes, "certain matters—for instance, enforcement, registers and archives, documentation of contracts—were regulated by royal decree, and therefore naturally exempt from the principle of personality" ("Law in Ptolemaic Egypt," in *Essays in Honor of C. Bradford Welles* [American Studies in Papyrology I, New Haven: The American Society of Papyrologists, 1966], 70). See also Wolff's "Plurality of laws in Ptolemaic Egypt," *Archives d'histoires du droit oriental*, 1960, *7* 191 ff., especially 217 ff.

It was composed of various elements, including Ionic, Attic, Doric, Aeolic.[29] The knowledge of the law of the Greek *polis* that we possess is primarily of Athens. In Athens the expert witness did not exist as a distinct category,[30] i.e., the law did not specifically recognize certain professionals as experts and assign to them a forensic role that made their evidentiary contributions procedurally mandatory. In the case of physicians as expert witnesses in Athenian law, we have, in a previous paper,[31] attempted to demonstrate that, although the initiative rested not on the state but solely on the individual to call a physician to witness in a professional capacity, physicians *qua* physicians were in fact used as expert witnesses and their testimony was assumed by those who called them to be given greater credence by virtue of their knowledge in matters of a medical nature.

In the creation of officials such as the *geometrai* who functioned in a legally designated forensic role, the Greek law of Ptolemaic Egypt went beyond native Greek law.[32] There were, in addition, experts in Ptolemaic Egypt with no legally recognized forensic capacity who were called upon to give expert testimony in situations where their professional knowledge was considered of evidentiary value by either party in a suit. We have reference in a papyrus from the end of the third century B.C. to an expert in hides being summoned by an official to make a professional assessment of value.[33] Somewhat similar is a papyrus from the mid-third century B.C. in which two weavers, in offering their skill to an individual, propose that either party may call in experts to estimate their work,[34] perhaps with a view towards a potential contract. It is reasonable to infer that during the Ptolemaic period physicians too were summoned on individual initiative to provide expert evidence. We should expect this because we have evidence that physicians were called to give expert testimony in Athens, a practice that, like so many other aspects of Greek legal procedure, is likely to have been brought to Egypt; and because the value of expert testimony was, as we have seen, recognized in other instances in Ptolemaic Egypt. The expert witness of a physician in legal procedures will have been simply *martyria*, testimony, albeit testimony to which greater credence is likely to have been given owing to the recognition of their expertise in the area involved in the case.

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[29] See Rafael Taubenschlag, "The Ancient Greek City Laws in Ptolemaic Egypt," in *Opera Minora* (Warsaw· Naukowe, 1959), Vol I, 601 ff; Rostovtzeff, *op. cit* , Vol II, 1047 and 1067 ff The Greek *politeuma* of Alexandria had its own code of laws, modelled substantially on those of Athens. See A H M Jones, *The Greek City from Alexander to Justinian* (Oxford. Clarendon Press, 1940), 3; William Tarn and G T Griffith. *Hellenistic Civilization*, 3rd ed. (London: Arnold, 1953), 196 f

[30] See e.g., A. R W Harrison, *The Law of Athens*, Vol II *Procedure* (Oxford. Clarendon Press, 1971), 134

[31] Darrel W Amundsen and Gary B Ferngren, "The physician as an expert witness in Athenian law,' *Bull Hist. Med.*, 1977, *51*: 202 ff

[32] Walter Hellebrand, *Das Prozeßzeugnis im Rechte der grako-agyptischen Papyri I. Die Funktion des Zeugen im ptolemaischen Verfahrenschrecht* (Munich· Beck, 1934), 108 "Solche Sachverstandigentatigkeit (sc , e g., that of the *geometrai*) geht über die eines sachverstandigen Zeugen wesentlich hinaus. Damit führt die ptolemäische Auffassung weit uber die heimatgriechische hinaus, die den Begriff eine 'sachverstandigen Zeugen' nicht uberwand "

[33] P Petr 32, 1, on which see Hellebrand, *op cit* , 107

[34] P S. I 341. See Kupiszewski, *op. cit* , 267.

In the Roman period we continue to see experts whose talents are not inherently favorable to forensic employment providing expert attestation. There is an agreement preserved, dating from 13 B.C., in which it is stipulated that after the son of one of the parties to the contract has been instructed in the playing of the flute, three experts will evaluate his skill and determine whether the instruction had been adequate.[35] A papyrus of A.D. 296 records that it was required by law that written agreements pertaining to dowries be evaluated by a goldsmith and a tailor.[36] But in addition to such craftsmen who lacked a legally mandated forensic status (although, in the latter case, did not lack a legally mandated forensic role), there developed during the Roman period a variety of categories of officials whose *raison d'être* was, from the state's point of view, largely forensic. Although the *geometrai* continued to exist under Roman rule, a type came into existence known as *horiodeiktai* who acted as boundary inspectors in both private and administrative cases.[37] *Architektones, tektones* and *oikodomai*[38] (building inspectors) and *logothetai*[39] (auditors of accounts, *e.g.*, those of estate managers, and documents connected with hypothecary loans) were employed as experts in a forensic capacity.

Early in the Roman period physicians began to exercise a legal role quite distinct from that which we surmise they performed under the Ptolemies. As part of the burgeoning investigatory and certificatory bureaucracy of Roman Egypt, physicians appear to have been assigned specific forensic functions in cases involving violent death or injury. Several papyri have survived that pertain to forensic medical investigation or examination, dating from the late first century A.D. to the late fourth century.[40]

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[35] B G U 1125 See Kupiszewski, *op cit.*, 268

[36] This papyrus is printed in *Études de papyrologie*, 1939, *5*: 85 ff (P Cairo Boak 21). See Kupiszewski, *op cit*, 267

[37] First attested in the late second century A D See Kupiszewski, *op cit*, 260 ff

[38] *Ibid*, 266 f

[39] *Ibid.*, 265 f

[40] These documents have previously been discussed by several scholars in varying detail; see Karl Sudhoff, *Arztliches aus griechischen Papyrus-Urkunden* (Studien zur Geschichte der Medizin, herausgegeben von der Puschmann-Stiftung an der Universitat Leipzig, Heft 5/6, Leipzig· Barth, 1909), 239 ff ; Ofelia Nanetti,

These documents are of four different types: (1) A request to an official for an examination or investigation, (2) An order from an official to his assistant to take with him a physician and submit a written report, (3) A *prosphonesis* (medical report), (4) A record of a *prosphonesis* used as evidence in a trial.[41]

We shall discuss each of the four types of documents.

1. Requests for an examination or investigation are generally addressed by private individuals to the *strategos*[42] through the mid-third century A.D. From the late third century, the officer to whom requests were made was the *logistes*.[43] The petitions that resulted in a medical investigation or examination fall into three categories.

a. Some request the official to send an assistant who is to bring with him a physician. An example is P. Oxy. 1556 (A.D. 247). This is a petition, addressed perhaps to a *strategos*, requesting that a *hyperetes*[44] and a *demosios iatros* (public physician) be sent to certify the petitioner's condition. The document, which does not indicate the nature of the medical problem, requests that a copy of the petition be filed for future legal action.[45]

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"Ricerche sui medici e sulla medicina nei papiri," *Aegyptus*, 1941, *21*, 301 ff ; E Boswinkel, "La médecine et les médecins dans les papyrus grecs," *Eos*, 1956, *18*, 181 ff , and Kupiszewski, *op cit* , 263 f Incidental references appear in a variety of works, e g , Leopold Wenger, "Der Eid in den griechischen Papyrusurkunden," *Zeitschrift der Savigny-Stiftung fur Rechtsgeschichte Romanistische Abtheilung*, 1902, *23*, 218 f , Raphael Taubenschlag, *The Law of Greco-Roman Egypt in the Light of the Papyri*, 2nd ed (Warsaw Naukowe, 1955), 633 f , A R Hands, *Charities and Social Aid in Greece and Rome* (London Thames and Hudson, 1968), 139; Cohn-Haft, *op. cit.*, 69 ff , Woodhead, *op cit.*, 241 Occasionally the editors of the collections of papyri in which these documents appear provide useful commentary, especially Eitrem and Amundsen on P Osl 95 (Vol III, 100 ff )

[41] It should be noted that there is not one individual case for which all the documents from these four procedural steps survive

[42] The *strategos* was, under the Ptolemies, a captain of the civil guard, however, during the Roman period, his authority, delegated by the imperial prefect, apparently involved a degree of jurisdiction in penal cases until the beginning of the second century A D Thereafter he seems to have become an administrative official having responsibility primarily in preliminary proceedings for the courts and therefore requests of investigation or examination logically were addressed to him

[43] The *logistes* was called in Latin the *curator civitatis*, the chief official of a city Little is known of his functions, see B R Rees, "*Curator civitatis*," *Journal of Juristic Papyrology*, 1954, *7-8*· 83 ff The latest document (P Lips 42, late fourth century A D ) is addressed to an official whose title is fragmentary in the papyrus, the editor of the papyrus restoring it to read *enarchos nuktostrategos* P. Cairo Preis 7, also from the fourth century, gives *ekdikos* as the official

[44] The *hyperetes* was an assistant to the *strategos* whom he represented in investigations We shall discuss his role in these cases in greater detail below

[45] Cf P Flor 59 (either A D 225 or 241), a petition upon the nature of which the editors of the papyrus did not choose to speculate Ulrich Wilcken ("Papyrus Urkunden," *Archiv fur Papyrusforschung*, 1906, *3*, 536) convincingly restores the lacunae in lines 8 through 10, and demonstrates that this is a petition probably requesting that both a *hyperetes* and a *demosios iatros* be sent to examine the petitioner who had apparently been assaulted Wilcken's restoration of the lacunae in lines 8 through 10 are paralleled by the formulaic wording in related documents and make the restoration of a *demosios iatros* very likely

b. In some cases the petition does not specifically request a physician but the official nevertheless deems it desirable for a physician to make an examination and certification. An example is P. Oxy. 475 (A.D. 182) which contains the text of a petition to a *strategos* to send a *hyperetes* to certify the death of a slave boy who had died after falling out a window of the petitioner's house while leaning forward to catch a better view of some dancing girls. Although no request was made for a physician, the *strategos* ordered a *demosios iatros* to accompany the *hyperetes* and to submit a written report.

c. Some petitions may have specifically requested a public physician to make an examination without mentioning a *hyperetes*. Although no such documents are extant, there are some *prosphoneseis* in which the public physicians state that they have been sent by an official in consequence of a petition having been filed. It cannot be determined whether the petitions simply requested an investigation without specifying the investigatory official or asked specifically for a public physician. The absence of any reference in these to the *hyperetes* makes them somewhat anomalous.[46]

The requests for examination or investigation involved either death or injury caused by violence or accident. Several notifications of death survive where there was no medical certification regarding cause. Notification of death may not have been mandatory since the persons writing extant notifications seem to have been concerned that the deceased be removed from the tax rolls.[47] In cases where the death was in any way unusual, the individual requested an investigation or examination with a view towards possible future legal action (either anticipated against a third party or fearing that action might be taken against himself). It also appears that, in the case of violent or accidental death, or in any case where foul play could be suspected, an investigation was required before the body could be laid out for burial.[48] In the case of injury caused by accident or violence, the motivation was probably the same as in an instance of homicide,[49] except that the individual who had been injured could, if his condition permitted, initiate the request himself.

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[46] See, e.g., P. Oxy. 52 (A.D. 325), 896 (A.D. 316), 983 [= S.B. 6003] (A.D. 316), P. Rein. 92 (A.D. 392). See n. 50, below. Additionally, documents from individuals describing injuries sustained that do not request investigation or certification by any official may have resulted in the *strategos* (or later the *logistes*) sending a physician, especially since medical certification in cases of accidental or violent death or injury seems to have been required. Such documents (e.g., P. Princ. 29 and B.G.U. 45) are properly called petitions only insofar as their senders request that the document be kept on file.

[47] E.g., P.S.I. 1064.

[48] E.g., P. Oxy. 475. Embalmers appear to have been entrusted with the duty of examining a cadaver in certain circumstances. Since the body of a person who had died under suspicious conditions could not be prepared for burial until an investigation had been made, in a case where suspicion was not aroused until after the body was already in the possession of the embalmers, the embalmers may then have been commissioned to conduct an examination. This is, of course, conjecture, but it seems unlikely that, in cases where the body

2. Although some *prosphoneseis* allude both to the petition and to the order from the *strategos* to conduct the examination, the only papyrus that contains the actual text of the official's order is P. Oxy. 475, referred to above. This document, addressed by the *strategos* to the *hyperetes*, instructs him to take with him a public physician and view the corpse and, after delivering it for burial, to write a report.

3. The majority of the documents relevant to this discussion are in category 3, the *prosphoneseis*. These are the medical reports written by the examining physician, the *hyperetes* or by a scribe. Each *prosphonesis* (except where the ending is missing) is signed by the physician(s) who conducted the examination.[50]

The *prosphoneseis*, as was indicated above, either certify death or the extent of injury. Although several of the petitions or trial records mention an examination of a corpse, only two *prosphoneseis* are extant that are themselves reports of such an examination.

P. Oxy. 51 (A.D. 173) was written by a *demosios iatros* who had been ordered to inspect the body of a man who had been found hanged and to send his report to the *strategos*. The physician accordingly examined the body

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had not yet been officially released for burial, embalmers would have been summoned to make examinations in lieu of a physician. P. Oxy. 476 (second century A.D.) records an apparent instance of embalmers being ordered to investigate a death. The editors of this papyrus have restored the papyrus to read "[We therefore inspected] the said bo[dy at] his [house]. . ."

[49] E.g , P. Oxy. 52 and 1556 The writer of the latter is concerned with the eventuality of charging with assault a man who had injured him. P Osl 95 involves the injury of a slave girl who had been beaten The owner likely was anticipating suing the culprit for damages to her property, i e , her slave.

[50] All the *prosphoneseis* whose endings are extant that were written before A.D. 316 were signed by the *hyperetes* in addition to the physician(s). There are three *prosphoneseis* (P Oxy 52, 896 and 983 [= S B 6003], two from the year 316 and one from 325) that do not include a reference to a *hyperetes* Perhaps his attendance by this time had ceased to be required These three papyri from the early fourth century all seem to involve two or more physicians (although P. Oxy 983 is very open to question), and this has led some scholars (e.g., Eitrem and Amundsen, *op cit.*, 102, Nanetti, "Ricerche . . ," *op cit* , 303) to feel that when two or more physicians made the certification, the presence of the *hyperetes* was not needed P Rein 92 (A.D. 392), however, does not mention a *hyperetes*, but only one physician conducts the examination and writes the *prosphonesis* In P Lips 42 and P Cairo Preis 7 (both from the late fourth century), the *hyperetes* is present and only one physician is involved, although in P Lips 42 it seems that the physician, acting on his own initiative, brought along the *hyperetes* Boswinkel (*op. cit.*, 184) thinks that the significance of the *hyperetes*' role was greatly reduced in the period when the procedure came under the jurisdiction of the *logistes*, but the question is one that cannot be resolved on the basis of the present state of the evidence The importance of the *hyperetes* vis-à-vis the physician in the earlier documents has been stressed by some scholars; we shall state our views regarding the *hyperetes*' role below

and simply reported that he had found it hanged by a noose without giving further details.

P. Rein. 92 (A.D. 392) is a medical report submitted to the *logistes* by a public physician who had been ordered, in response to a petition filed by a police official, to examine the body of a village official and to submit a written report. He records that he had examined the corpse, which showed no signs of contusions or lacerations, and states that the deceased had succumbed to an acute disease.

Much more plentiful are the *prosphoneseis* concerning injury, which we summarize.

P. Osl. 95 (A.D. 96) is a report of the examination of a slave girl. The physician's examination revealed that she had suffered an injury to her middle finger (it is not specified on which hand), which he treated, and that she had a determination of blood on the breast (also no specification whether right or left) and was vomiting and feverish.

B. G. U. 647 (A.D. 130) is the *prosphonesis* of a physician who, having been called to investigate an injury, found, above the man's left temple, a deeply penetrating wound containing small splinters of stones.[51]

P. S. I. 455 (A.D. 178) is the *prosphonesis* of a public physician who was ordered through a *hyperetes* to make a medical examination and submit a report on an injured person's condition. He records that he made the examination in the presence of the *hyperetes* and found that the injured party had sustained three wounds to his head and a contusion on his left shoulder-blade and shoulder.

P. Osl. 96 (A.D. 272) is a very fragmentary *prosphonesis* probably by a public physician. We learn from it only that wounds to the head had been sustained by a man who apparently had fallen.

B. G. U. 928 (A.D. 288) is the *prosphonesis* of a *demosios iatros* to the *logistes* in response to a petition filed by an official who had suffered injuries. The physician, accompanied by an official, who is presumably a *hyperetes*, examined the petitioner who had sustained bruises and swellings on his right arm and shoulder and elsewhere and whose clothes were torn.

P. S. A. Athen. 34 (third or fourth century A.D.) is the report of a public physician made in response to a petition filed by a councillor who had complained that his herdsmen had been beaten. Accompanied by a *hyperetes*, the physician made an examination of the herdsmen. The portion of the papyrus in which the wounds are described is very fragmentary, but enough survives to show that several had received wounds to the head.

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[51] The form of B G. U 647 is quite different from that of the other *prosphoneseis* In this papyrus, after the formulaic address to the *strategos* and a short description of the situation that precipitated the examination, the physician and two elders of the village swear an oath by the Genius of the emperor The description of the injuries is then given in the first person by the physician and this is followed by a scribe's attesting the witness of the two elders who were illiterate Then there follows a nearly verbatim repetition of certain elements already given together with the description of the injuries This is signed by the physician and the *hyperetes*

P. Oxy. 983, the very fragmentary text of which is found in S. B. 6003 (A.D. 316), is the *prosphonesis* by a public physician (or perhaps two) of a man confined to bed owing to injuries. The extant portions of the report include "two wounds on the top of the head . . . and on the right side of the head and along the left side of the forehead . . ., a swelling and a bruise [along] the left auricle and . . . down the right shoulder blade, a wound and swelling along the thumb of the right hand . . ., a swelling and a bruise on the [left?] hand and a wound on the left side . . . and two (?) wounds on the right side."

P. Oxy. 52 (A.D. 325) is the *prosphonesis* by two (or more) public physicians based on their examination of a girl who, as the result of the collapse of her father's house, was found to have sustained "several cuts in her hip and wounds near the shoulder and on the right knee."

P. Cairo Preis. 7 (fourth century A.D.) is a *prosphonesis*, addressed to the *ekdikos* by a public physician, describing an examination made in response to a petition from someone who had been attacked and wounded. The text is very fragmentary and breaks off before the description of the wounds is given.

P. Lips. 42 (late fourth century A.D.), the *prosphonesis* of a *demosios iatros* ordered to examine an injured man, includes the following description: "a swelling and a bruise on the left brow, and a swelling and a bruise on the left thigh (?), an abrasion on the left side, and a swelling of the scrotum or of the muscles by which the testicles are suspended."

There is one medical report, P. Oxy. 896 (A.D. 316), that perhaps does not properly fit into the categories of certification of death or of injury. We shall discuss this *prosphonesis* below (pp. 349-50).

Judging from the few *prosphoneseis* that have survived, it appears that the medical certificates signed by physicians in Roman Egypt were, from a modern pathological point of view, relatively unsophisticated. Yet they seem to have been quite sufficient to fulfil two functions. In the case of violent or accidental death, they simply certified the cause of death. We can reasonably assume that the physician who filed P. Oxy. 51 did not, by saying that he observed the body in question hanged by a noose, imply that he merely saw that the corpse was indeed suspended in that fashion (which any reliable witness could have certified) but that the cause of death was hanging. In the case of injury the concern was to give as accurate a description of the injuries sustained as possible. The most obvious use to which a detailed description of injuries could have been applied was in the assessment of damages against an assailant in an instance of assault or, e.g., perhaps against a builder in the case of the injuries sustained by the girl in the collapse of her father's house in P. Oxy. 52.

4. Our category 4, the use of *prosphoneseis* in a trial, is illustrated by only two extant papyri. P. Oxy. 2111 (c. A.D. 135) is a very fragmentary papyrus that contains the record of several cases heard by the prefect. The third case (lines 20 ff.) involves a violent quarrel between a man and a woman over some property which had been left to the woman that culminated in the strangulation of the woman. There is mention of a physician and a *hyperetes* who apparently had inspected the corpse and whose *prosphonesis* is introduced as evidence. P. Oxy. 1502 (c. A.D. 260), a very fragmentary report of a trial apparently for assault, contains a reference to a public physician whose written *prosphonesis* is introduced as evidence together with the petition to the *strategos* which evidently had occasioned the *prosphonesis*.

Nanetti, in her study of these medical reports, identifies four separate functions of the physicians who appear in the documents under discussion: (1) Certification of death, (2) Report of medical visit and treatment, (3) Written testimony for criminal proceedings or civil suits, (4) Certification of the illness of an employee.[52]

We believe, however, that the evidence supports only one of the four functions that Nanetti has assigned to these physicians. Death certificates apparently did not require a physician's signature. Only in cases where violence or accident was involved does it appear to have been necessary to request an official examination; this concern seems to have been prompted by the possibility of legal action. The individual who requested that a *hyperetes* be sent so that the slave boy who had died from a fall could be buried (P. Oxy. 475) did not ask for a physician to accompany the *hyperetes* and, judging from other extant notifications of death, would not have requested a *hyperetes* had not this procedure been required in instances of violent or accidental death. The *strategos*, however, sent a physician along with the *hyperetes*, not to verify that the boy was dead, for the *hyperetes* could just as easily have done that without the aid of the physician. The legally recognized expertise of the physician was employed to investigate the cause of death, since it occured under suspicious circumstances, to aid in determining whether legal action should be taken. Consequently, Nanetti's first category is not supported by the evidence; a physician's certification of death should be made part of her third category, written testimony for possible legal proceedings.

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[52] "Ricerche . . ," *op cit.*, 314.

Nanetti's second category, report of medical visit and treatment, also lacks evidence. In only one instance do we find treatment mentioned: P. Osl. 95, where only one of the several complaints of a slave girl is mentioned as having been treated and this was only incidental to the forensic role of the physician in making his examination. Nanetti cites in support of her second category P. Princ. 29 (A.D. 258). In her short description of this document[53] she was misled by the interpretation of the editor of the papyrus who believed that this papyrus contained a request for medical treatment made on behalf of the petitioner's brother who had been injured, apparently in a fall. As Cohn-Haft rightly points out,[54] there is in this petition no mention of a physician or even of a *hyperetes*, rather only a request that the document be kept on file in case the brother should die. Cohn-Haft chastizes Nanetti for including P. Princ. 29 in her discussion and observes that this papyrus "is exactly paralleled by B. G. U. 45, of 203 A.D., where, in a similar petition to the *strategos*, a father describes an attack made by certain persons upon his son, the son's present condition, and concludes with the request that the statement be placed on file."[55] It is not unlikely that both documents were followed by an order from the *strategos* to the *hyperetes* to take a physician, make an investigation, and to submit a report. But in the absence of any extant documents to support this hypothesis, it must remain merely an argument from analogy.

Cohn-Haft recognizes that only one of Nanetti's categories is valid: "From [the papyri] only one function emerges: the *demosioi iatroi* did, upon request, make an examination followed by a written report, properly witnessed, attesting to the facts in cases where a potential legal action is envisaged, most often cases of injury or death resulting from violence, whether allegedly accidental or deliberate."[56] He, however, only questioned Nanetti's second category. We have already discussed her first. The fourth remains to be examined: certification of the illness of an employee. The one document she advances in support of this function is P. Oxy. 896 (A.D. 316). This is a

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[53] *Ibid*, 313

[54] *Op. cit.*, 70

[55] *Loc cit.*

[56] *Ibid.*, 71.

*prosphonesis* made by two *demosioi iatroi* to the *logistes*. They relate that they had been sent to examine an official who requested medical certification of his condition. They state that they found him lying in bed suffering from a fever. Here there is a small lacuna in the papyrus where the editors thought there was once probably an adjective modifying the word for fever. It is indeed possible that where there is now a lacuna there was originally a word descriptive of some injury or complaint not directly related to the fever. The editors conjecture that the official involved may have been suspected of malingering and their interpretation has been frequently followed, not only by Nanetti but also by other scholars who touch on the functions of the *demosioi iatroi* in Roman Egypt, thus conveying the impression that they had, as a distinct functional role, the investigation of cases of suspected malingering.[57] We have no way of knowing the official's motivation for requesting this examination. It may indeed be true that he was simply suffering from a fever and used his position as an official to procure the certification of his illness by public physicians (which we cannot assume that the average laborer who was ill could have done), but his motivation would have been the fear of legal action against himself for dereliction. It is equally plausible that where there is now a lacuna the papyrus described a more serious illness or injury and that the official was suffering from more than just a fever and was envisaging some form of legal action of his own. In any event, this papyrus alone, fragmentary as it is, does not provide sufficient evidence to support Nanetti's fourth category as a distinct functional role for public physicians.

In our discussion of the *prosphoneseis* we have used either the designation physician or *demosios iatros* (or its English equivalent, public physician). In all the relevant documents, with the exception of the three earliest (P. Osl. 95 [A.D. 96]; B. G. U. 647 [A.D. 130]; and P. Oxy. 2111 [A.D. 135]), the physicians are called *demosioi iatroi*. In B. G. U. 647 the physician describes himself as "having an *iatrion* in the village," and in P. Oxy. 2111 he is simply referred to as *iatros*. In P. Osl. 95 he merely says that he is "a physician in Oxyrhynchos." Eitrem and Amundsen, by analogy with the later documents, assume that the physician in P. Osl. 95 is, in fact, a *demosios iatros*.[58] This title does not seem to have occurred in Egypt before the mid-second century A.D. and it does not seem to be attested outside Egypt.[59] Boswinkel[60] has advanced the theory that this title originated as a response to the decree of Antoninus Pius (A.D 161) limiting the number of physicians eligible for exemptions from liturgies (*munera*) to ten for cities, seven for towns and five for villages.[61] Boswinkel's interpretation is not capable of proof; nor can it easily be disproved. Given the fact that prior to the decree of Antoninus Pius all physicians were (in theory) exempt from liturgies[62] and that some physicians were used in the early Roman period in Egypt in an official, forensic role, the imposition of a numerical limitation on an existing practice does not necessarily imply any terminological or functional change as a direct consequence. If we concede that the designation *demosios iatros* was perhaps directly or indirectly due to the necessity of appointing a fixed number of physicians to enjoy immunities, does it follow that there was a concomitant functional change? We can assume that the *iatroi* who appear in a forensic capacity before Antoninus Pius' decree were not limited to that function. But what of the *demosioi iatroi*? A. H. M. Jones remarks that "the papyri . . . suggest that [physicians'] principal activity was signing medical certificates for the use of the courts and the administration" but he suspects that this is no doubt an unjust impression.[63] If Boswinkel is correct in asserting that only those *iatroi* chosen for immunities were designated *demosioi*, and if they functioned only in an official, forensic capacity, it follows that they were indeed members of the bureaucracy and as such were limited to a forensic role. If, however, they also engaged in the regular treatment of patients but were termed *demosioi* only when performing an official function, the term appears as a designation of function rather than, necessarily, status. That physicians who were not granted immunities were called to perform a forensic role and while acting in that capacity were called *demosioi* to indicate their official function is also plausible. It should be obvious that the evidence does not offer a positive solution to the problem.

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[57] See, e.g., Hands, *op. cit.*, 139

[58] *Op. cit.*, 101

[59] Cohn-Haft says that, to the best of his knowledge, "this particular location is not met anywhere in the Hellenistic period, nor anywhere outside of Egypt in the Roman period" (*op. cit.*, 69) In this he is probably correct Hands (*op. cit.*, 174, n. 169) misreads Strabo, 4, 1, 5, 181, as implying the existence of *demosioi iatroi*

Boswinkel gives particular attention to the role of the physician (*iatros* or *demosios iatros*) vis-à-vis the *hyperetes* in the *prosphoneseis* and stresses the apparent secondary importance of the physician in some papyri.[64] Ei-

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in the West and incorrectly cites *An Economic Survey of Ancient Rome*, ed. by Tenney Frank (Baltimore Johns Hopkins Press, 1933-40), Vol III, 417 and Vol IV, 807 as supporting this interpretation

[60] *Op. cit* , 184 f

[61] Corpus iuris civilis, Digesta, 27, 1, 6

[62] P Fay 106 (c A D 140) and P Oxy (c A D 142) demonstrate that they sometimes had to fight to maintain this privilege

[63] A. M H Jones, *The Later Roman Empire, 284 - 602. A Social, Economic and Administrative Survey* (Oxford Blackwell, 1964), 1012 f

[64] *Op cit.*, 182 ff

trem and Amundsen believe that the administration did not consider the physician's certification sufficient but "requiring still greater warranty" thought it necessary to have the *hyperetes*' verification.[65] Although it is true that, until the early fourth century, the *hyperetes* was always present at the medical investigations, the concern of Boswinkel, Eitrem and Amundsen over the comparative "importance" of the *hyperetes* and the physicians is ill-founded. The two undoubtedly had distinctly different functions. As the assistant to the official responsible for preliminary investigations in cases that had the potential for legal action, the *hyperetes* surely acted *ex officio* in a role that complemented rather than duplicated the investigatory capacity of the physician. While the physician was responsible for verifying the extent of injuries or the cause of death, the *hyperetes*, as the representative of the official who would decide on legal implications, acted in an advisory capacity to his superior regarding potential court action. His presence at the medical examination was accordingly a desirable step in the total investigatory procedure leading up to possible legal action. The *demosioi iatroi*, although acting in an official capacity, were not police officials upon whom the onus of broader legal determinations was incumbant. It is as misguided to contrast the importance of the physician and the *hyperetes* in these cases as it would be to ruminate upon the comparative roles of a coroner and a district attorney in modern legal proceedings.

In this paper we have attempted to demonstrate that in Ptolemaic Egypt physicians apparently had no mandatory forensic role. We can reasonably assume, however, that physicians were called on individual initiative to provide expert witness in cases where a medical determination was central to a legal judgment. We have based this interpretation on two factors. Since the law governing the Greek population of Ptolemaic Egypt was drawn from the law of various Greek *poleis*, including Athens, where physicians appeared as expert witnesses summoned on individual initiative, a similar situation can plausibly be assumed for Ptolemaic Egypt. Secondly, since various craftsmen, e.g., experts in hides and weavers, were called to give expert testimony in cases where their professional competence was considered helpful in legal determinations, by analogy, a similar function will have likely existed for physicians. The forensic role of such experts was not, however, procedurally mandatory. There is evidence for the continued forensic employment of craftsmen in Egypt during the Roman period where, as we have seen, goldsmiths and tailors had a legally mandated forensic role in certifying the value of dowries, although they lacked a legally mandated forensic status. But a variety of experts, e.g., building inspectors and auditors, were assigned an officially designated forensic status, and physicians were as a matter of procedure called to certify the cause of death or the extent of injury in cases involving violence or accident. Physicians acting in this capacity had no official title before the mid-second century A.D., when they begin to appear consistently as *demosioi iatroi,* although no functional change in their forensic role is evident. Whether the *demosioi iatroi* were limited to investigatory and certificatory duties or whether the title was applied to physicians only when acting in a forensic capacity must remain unanswered. What clearly emerges is that physicians were recognized as experts whose talents were used by the state when medical attestation served a legal end and their certification was not duplicated but rather was complemented by other investigatory officials.

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[65] *Op cit*, 101 f

A final question involves the origins of such legally designated forensic experts.[66] Since they came into existence under Roman domination it is possible that their creation resulted from the influence of Roman law on the procedures in Roman Egypt. Any attempt to answer such a question is entirely speculative at this point but will be added as a retrospective to our forthcoming paper on the physician as an expert witness in Roman law.

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[66] It has been suggested that physicians were employed as forensic experts in Pharaonic Egypt (See Roberto Bozzoni, *I medici ed il diritto romano* (Naples: Pierro e Figlio, 1904), 133, citing Revillout, *Precis du droit égyptien.*) If this is true it is indeed possible that such practice continued among the native Egyptians during the Ptolemaic period. This would involve quite a separate study requiring the examination of the demotic papyri. Even if such forensic employment of physicians could be proved for the native Egyptians, given the absence of comparable procedures by Greek-Egyptian courts under the Ptolemies, we would hesitate to assign it a causal role in the formulation of the forensic medical practices that prevailed in Roman Egypt

# THE HISTORICAL BACKGROUND OF INTERNAL FIXATION OF FRACTURES IN NORTH AMERICA

ROBERT A. ROBINSON

By this paper the author wishes to pay respect to all of the surgeons who have, since about 1800, developed open reduction and internal fixation of fractures. I wish to bring attention to the work of a few doctors in North America who, with the help of European colleagues, have contributed to the concepts, implant materials, and operative techniques for internal fixation of long bone fractures.

I particularly want to mention Dr. Clay Ray Murray (1890-1947) (Fig. 1) who in 1942 not only taught me what he knew about the treatment of open fractures but also the treatment of originally closed fractures by open reduction and internal fixation.[1] He was Chief of the Fracture Service at the Columbia-Presbyterian Medical Center and Professor of the Columbia University's College of Physicians and Surgeons in New York City.

The "road" to our present position with open reduction and internal fixation has been a difficult one, often involving contention with the peers of those who advanced the subject.[2] In other words, surgeons who have developed the principles of internal fixation of fractures had to have an ideal to which they could cling and I think their idealism is summed up in Clay Ray

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[1] F. W. Bancroft and C. R. Murray, *Surgical Treatment of the Motor Skeletal System*, Part 2, Section 7 (Philadelphia: J. B. Lippincott, 1945), pp. 615-688; C. R. Murray, "The treatment of fractures," *Internat. Surg. Digest*, 1931, *12*: 131-140; *Idem*, "Physical therapy in the treatment of fractures," *Arch. Phys. Ther., X-ray, Radium*, 1933, *14*: 325-329; *Idem*, "Healing of fractures. Its influence on the choice of methods of treatment," *Arch. Surg.*, 1934, *29*: 446-464; *Idem*, "Immediate and early physical therapy in fracture management," *Arch. Phys. Therapy*, 1938, *19*: 74-78; *Idem*, "The timing of the fracture healing process: its influence on the choice and application of treatment method," *J. Bone & Joint Surg.*, 1941, *23*: 598-605; *Idem*, "The detailed operative technique for open reduction and internal fixation of fractures of the long bones," *J. Bone & Joint Surg.*, 1944, *26*: 307-312.

[2] In 1775 a French physician, Icart, publicly accused two of his colleagues, Lapuyade and Sicre, of malpractice resulting in the death of a patient. The defendants had performed an open reduction and internal fixation of a fracture of the humerus. Brass or iron wire was used to fasten the fragments together. Inflammation and suppuration ensued, and death occurred two days later. Because of the speed of events this particular disaster was undoubtedly due to an overwhelming septicemia rather than the effects of implant corrosion as Icart charged. G. Schmeisser, "Progress in metallic surgical implants," in: *Membership Handbook ASTM Committee F4 on Surgical Implants* (Philadelphia: American Society for Testing and Materials, 1970), Appendix, pp. 31-53.

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Figure 1
Clay Ray Murray, 1890–1947
From G. J. Curry et al., *Profiles in Trauma*, 1969, p. 160. *Courtesy of Charles C Thomas, Publisher, Springfield, Ill.*

Murray's oft repeated ideal of fracture treatment: "To wish the fragments into place, to hold them there by moral suasion, and send the patient on about his business while the fracture heals."[3]

Dr. Harrison McLaughlin, who like myself (although for a much longer period), was a student and associate of Dr. Clay Ray Murray, interpreted this ideal:

(1) "To wish the fragments into place" means reduction without any additional tissue damage. IT CAN'T BE DONE! Nonetheless, the best reduction is that most closely approximating this ideal: that is, the earliest and gentlest reduction possible. For the same reason, that is, prevention of secondary tissue damage and adequate first aid care is essential to an optimal final result.[4] As pointed out by Murray an open reduction may be less traumatic

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[3] H. L. McLaughlin, *Trauma* (Philadelphia: W. B. Saunders, 1959).

[4] *Idem, Principles of Fracture Treatment in an Outline of the Treatment of Fractures by the Committee on Trauma*, 5th ed.(Chicago, American College of Surgeons, 1954); Murray, *Treatment of Fractures* and *Timing of the Fracture Healing Process* (n. 1 above).

to the soft tissues surrounding the fracture than a closed reduction, particularly if multiple, forceful, unproductive attempts at closed reduction become necessary.

(2) "To hold them there by moral suasion" means maintenance of reduction without interfering with continued function of the associated soft tissue structures. THIS IS IMPOSSIBLE! Nevertheless, the apparatus or method most closely approaching this ideal is the best, that is, that which provides adequate stabilization of the bone fragments coincident with minimal interference with local function throughout the healing process. A healed bone, even in perfect anatomic alignment, is of little use when the surrounding soft tissue has been compromised in adults by prolonged immobilization, or infection, or unnecessary disuse:[5] the soft tissues in general heal more quickly than does bone.

(3) "To send the patient on about his business while the fracture heals," means maintenance of all social, economic and other normal functions of the patient as a whole throughout fracture healing. THIS IS RARELY POSSIBLE. However, the method of choice of fracture treatment is that which most nearly approaches this ideal. As implant quality and availability, instrumentation and experience have been developed, an increasing number of fracture surgeons (particularly Orthopaedic Surgeons in the United States), have decided that the apposition of bone fragments and their rigid internal fixation may be best achieved in an increasing number of patients by open reduction: that plates and screws, intramedullary rods with adjunct circumferential wires and/or screws, are best for achieving the goals of ideal fracture treatment in more and more patients.

Since 1850, with the widespread use of anaesthesia and the other general improvements in the sciences that surround surgery (Fig. 2), open reduction and internal fixation of fractures has become safer for the patient and less frustrating for the surgeon. Just as these advances in medical sciences all came together about 1950 to make the anterior approaches to the spine more acceptable to patients and surgeons, so have they assisted the acceptance of the open reduction and internal fixation of fractures of long bones.

In the field of internal apposition of bone fragments and their stabilization one notices that advances in metallurgy have been followed by a wider application of implants, and a wider application of implants has led to improvement in these implants and the instrumentation associated with their

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[5] McLaughlin, *op. cit.* (n. 3 above); Murray, *Physical Therapy in the Treatment of Fractures* and *Immediate and Early Physical Therapy in Fracture Management* (n. 1 above).

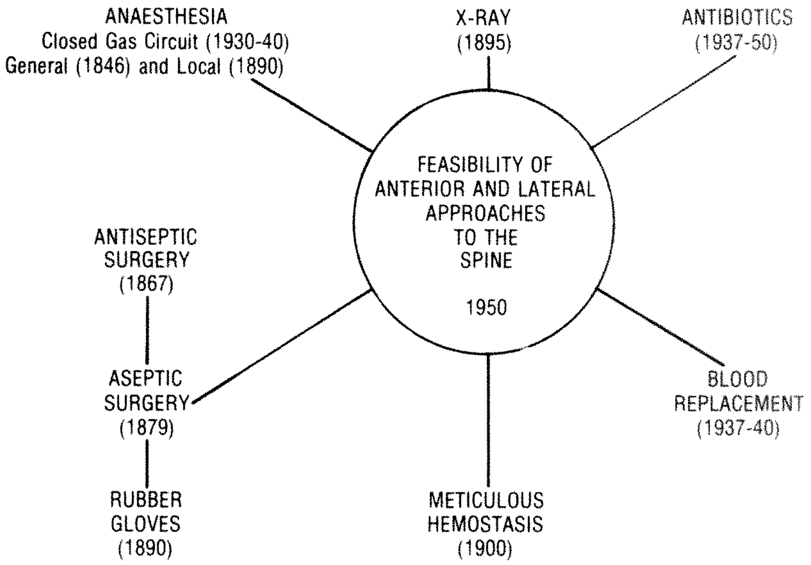

Figure 2

insertion and use. This in turn led to further improvement in metallurgy, mechanical design and concern about the biological compatibility of these implants with bone and soft tissue. This is a spiral and expanding type of evolution (Fig. 3). I would like to mention some of the outstanding points along this evolutionary path that can be attributed at least in part to the pioneers in internal fixation of fractures in the United States since about 1800.

The treatment of fresh fractures by open reduction and internal fixation really stems from the desire to treat disabling non-unions of long bones following fracture. Historically, the incidence of fracture non-unions, although widely denied or overlooked by practitioners of fracture treatment in gerneral, at least until the 19th century, nevertheless had been sufficiently frequent and disabling to give rise to a large number of published communications. Irving S. Cutter[6] reviewed them in 1929 and noted that "the numerous methods proposed in order to secure union are interesting as well as curious, some of which may be found in the writings of Celsus (during the

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[6] Irving S. Cutter,"Landmarks in surgical progress: wire fixation of an ununited humerus—J. Kearny Rodgers," *International Abstract of Surgery*, 1929, *49*: 189-192.

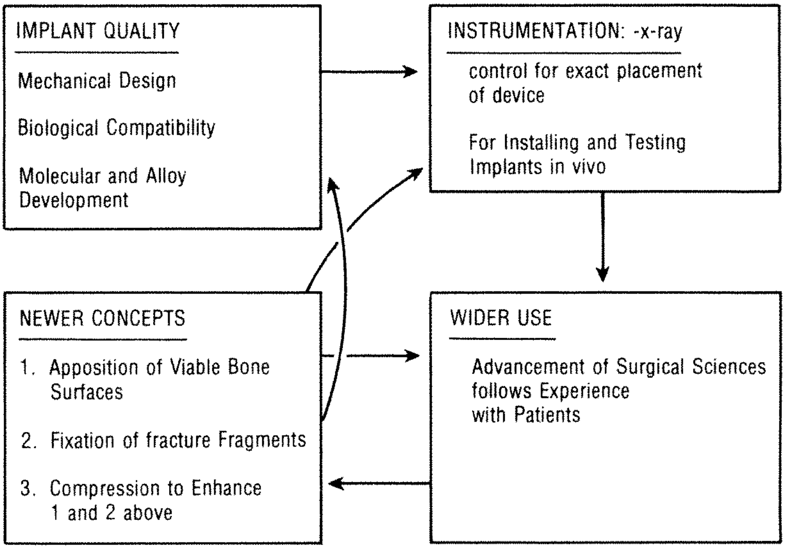
IMPLANT QUALITY
Mechanical Design
Biological Compatibility
Molecular and Alloy Development
INSTRUMENTATION: -x-ray
control for exact placement of device
For Installing and Testing Implants in vivo
NEWER CONCEPTS
1. Apposition of Viable Bone Surfaces
2. Fixation of fracture Fragments
3. Compression to Enhance 1 and 2 above
WIDER USE
Advancement of Surgical Sciences follows Experience with Patients

Figure 3

reign of Tiberius Caesar), Avicenna (980-1036 A.D.) and others. Celsus recommended friction. This consisted of vigorously rubbing together the ends of the ununited fracture fragments. Avicenna advocated scraping or rasping of the ununited ends of the bones. This was condemned by Guy de Chauliac, about 1300 A.D."

*An ideologic breakthrough came in 1759.* (Ideas are hard enough to come by, but in our litigious society they are becoming even more difficult to apply.) The idea of surgical resection, instead of rasping off the fibrous and cartilaginous tissue on the apposing ends of the fracture fragments at a non-union site, and the approximation of fresh bleeding bone to fresh bleeding bone for osseous consolidation of a longstanding bone discontinuity is generally credited to Charles White of Manchester, England.[7] He proposed the idea in 1759 for treatment of non-union of the humerus in a nine-year-old boy. Mr. James Burchall, a surgeon of Manchester, performed this operation with Mr. White in 1760. The operation was a success. They had youth going for them but not much else: no anesthesia, no antiseptic or aseptic surgery, no antibiotics, and no internal fixation device. (I wonder if they would have dared to apply this new idea in the present legally dominated environment where safety is the overriding concern in any new venture.)

[7] *Ibid.*

The earliest American case I could find in which the bone ends at a non-union site were freshened and then held together by an internal wire loop (an early form of internal fixation) to attain apposition of bone fragments was that reported by Dr. J. Kearney Rodgers of New York Hospital in 1827.[8] The patient was 15 years old. When the patient was about 12 years old he sustained a fracture of the humerus two inches proximal to the elbow joint. Twelve days after the fracture the splints were removed. Six months later a seton was passed through the non-union site and withdrawn at seven days. The elbow was splinted for four weeks. Non-union was still present when he arrived in New York City from Ohio a year later. A seton was again passed and left in place for six months while the arm was splinted: result—failure of bony union. He then consulted Dr. Rodgers at the New York Hospital when he was 15 years old. Dr. Rodgers stated, "It appeared to me that but one other mode remained which held any prospect of success—Mr. White's plan of sawing off the ends of the bone (at the non-union site), and reducing them to a state of a recent compound (i.e. open) fracture." Dr. Rodgers couldn't coapt the freshened bone ends at the time of surgery: "Apprehensive that I should be foiled if the bones were this far apart (1½ inches), I drilled the ends of the ununited fragments and brought them into apposition by means of a silver wire loop." He brought the ends of the silver wire loop out through a cannula that projected from the wound. That was on July 31, 1827. Fifteen days later, "the cannula fell away with the loop entire." Even though the wire had cut out of the bone, the humerus healed in good position by the 59th day. The patient had regained full elbow motion five months after operation. He had two inches of shortening of the humerus.

I should point out that Dr. Rodgers also used external immobilization: "I did not expect bony union for some time. I scarcely examined the state of the bones for a month. . . . For two months after the operation my patient was not allowed to move out of bed and during this time motion of the affected arm was prevented by splints. To the unyielding firmness with which my patient seconded my exertion, I attribute the success which crowned this operation." Dr. Rodgers presented the patient with a functioning arm to medical students at the New York Hospital on December 9, 1827.

Loops of wire, usually silver or platinum, and loops of silk through drill holes were used to appose bone fragments for many years. Halsted records "freshening" and drilling both ends of a mal-united femur and fastening of bone ends together by "strong silk" through the drill holes on May 7, 1890.[9] Lord Lister used the same general method as Rodgers had used on a boy's radius with a silver wire loop in 1873. He shortened the ulna to obtain radial apposition.[10]

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[8] J. Kearney Rodgers, "Article IV. Case of un-united fracture of the os brachii, successfully treated," *N.Y. Med. & Phys. J.*, 1827, *6*: 521-523.

Dupytren's[11] 1818 case of a fractured mandible treated by removal of a fracture fragment and application of a platinum wire loop passed around teeth for immobilization of the bone fragments of a fractured jaw was a somewhat different situation, since it really involved maintenance of bone apposition by a device external to the integument. However, the importance of bone apposition for fracture healing was obviously recognized by White in 1759, by Dupytren in 1818, by Dr. Rodgers in 1827, and by Lord Lister in 1873.

Norris,[12] in an exhaustive report written in 1842 on the occurrence and treatment of non-union, catalogued 22 suggested methods used, or "usable," for the cure of the non-union, including "blistering" over the site of non-union, application of iodine to the injured part, application of strong pressure applied to an external plate placed over the point of fracture, the application of electricity, and the intensive use of mercury internally to the point of extreme salivation, the application of caustic alkalis over the site of the fracture, the injection of stimulating substances such as copper sulphate, salt water and port wine poured into the limb between the ends of ununited fracture fragments. The seton, usually a silver wire, was probably introduced for treatment of fracture non-unions by Philip Syng Physick, of Philadelphia. It is known to have been used later by Mr. Charles Bell and Mr. Brodie in England.

Almost the last procedure mentioned by Norris in his 1842 list of 22 methods was the White method used and modified by necessity at the operating table by J. Kearny Rodgers, of New York, on the 15-year-old boy in 1827.

Norris concluded his list of 22 methods of treating non-unions with amputation which, as Somme well said, "could hardly be considered a method of cure."[13]

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[9] W. S. Halsted, "The treatment of wounds with especial reference to the value of the blood clot in the management of dead spaces. IX. Operations for ununited fractures," *Surgical Papers* (Baltimore: The Johns Hopkins Press, 1924), vol. 1, pp. 92-93.

[10] R. J. Godlee, *Lord Lister* (London: MacMillan, 1918).

[11] G. Dupuytren, *Leçons orales de clinique chirurgicale faites a l'Hôtel-Dieu de Paris*, 4 vols. (Paris, 1832-34), vol. 4, p. 669.

[12] G. W. Norris, "On the occurrence of non-union after fracture; its cause and treatment," *Amer. J. Med. Sci.*, 1842, n.s. *3*: 13-67.

[13] "The introduction of a wire between the ends of the fragments was advocated by C. L. S. Somme,

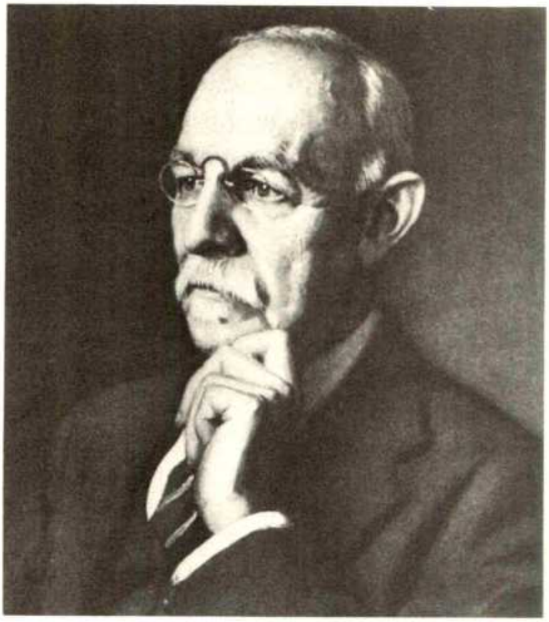

Figure 4
William S. Halsted, 1852–1922

Dr. William Halsted (Fig.4) who was the first Director of Surgery at the Johns Hopkins Hospital in Baltimore, was interested in internal fixation of fresh fractures by plates and screws: not rigid in present-day terms but more rigid than the wire loop.[14]

The following letter from Dr. L. A. Stimson in 1916 and Dr. Halsted's reply are of interest, particularly when one recalls that Mr. Arbuthnot

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surgeon of the Civil Hospital of Antwerp. His case report is frequently misinterpreted. By some authors Somme's case is cited as the first instance in which silver wire was used to bring the ununited fragments into apposition. Somme's article appeared under the title 'A Case of Ununited Fracture of the Thigh-Bone Cured by the Application of a Silver Wire between the Fractured Extremities.' After declaring the method of applying friction to the fragments too dangerous and too painful, Somme mentions White's operation of resection and the introduction of a seton which Roux saw performed in London. He then proceeds to describe the introduction of a thread of silver *around* [my italics] the site of the non-union by means of a trocar and cannula. The wire was imbedded in the wound by means of an incision and was tightened daily. Somme secured complete union in his case, withdrawing the silver wire after noting definite evidence of callus formation" (Cutter, *op. cit.* [n. 6 above], p. 190). This was apparently a circumferential compression and slow transection of the fibrochondrous callus rather than a compression of oblique fracture fragments of the humerus.

[14] S. J. Crow, *Halsted of Johns Hopkins: The Man and His Men* (Springfield, Ill.: Charles C Thomas, 1959).

Lane[15] is generally credited as being the first one to use buried screws for internal fixation of oblique fractures of the tibia in which the fibula was not broken.[16] The dates of Lane's first three operations were January 8, January 15, and January 18, 1894, when he was on service at Guy's Hospital. Halsted's reply to Dr. Stimson's letter showed that Halsted used *both* buried plates and screws as early as 1893. Dr. Stimson wrote as follows: "Dear Dr. Halsted: I remember that when you were at Roosevelt Hospital in New York City [i.e. prior to 1887] you *plated* a fracture of the humerus, and so far as I know, that was the first time it was done." Dr. Halsted replied:

> Dear Dr. Stimson: I wish I could recall the case of bone plating to which you refer. I believe that for many years we had the field of bone plating with buried screws to ourselves. The idea of burying the screws was original with me but the plates which I used were Hansmann's.
>
> I presume you recall Hansmann's little communication to the Deutsche Gesellschaft für Chirurgie in 1886.[17] I happened to be in Hamburg when Hansmann, Schede's first assistant, was perfecting this method with the aid of Dannenberg, a well-known instrument maker in Hamburg. This was, I think, in the spring of 1885. Hansmann gave me some of his plates and I purchased a good supply from Dannenberg. It is quite likely, therefore, that I used these plates, (and screws), in New York. I came to Baltimore in 1887 but had no occasion to employ the buried screws here until after the Johns Hopkins Hospital opened in 1889. The Hansmann plates contained, I think, nickel, copper and tin. They were therefore a kind of German silver. After experimenting with Bolton in Baltimore on the antiseptic properties of metal [1896] I used silver for both the plates and the screws.
>
> You will recall that Hansmann removed not only his long screws which projected beyond the skin into the open, but also the plates. His wounds in consequence were always more or less infected. [Langenbeck's device (discussion by Gurlt of Hansmann's paper) was employed only in pseudoarthrosis.] It was therefore only a step in advance to buried screws. The introduction by us of [rubber] gloves just at this period (1890) did much probably to insure the success of this method. . . . On looking over our records I find that I first used the Hansmann plates here [at Hopkins, in Baltimore] in April of 1890 but did not bury the screws in using these plates until 1893.

Although Dr. Halsted unquestionably used plates *with* buried screws in 1893, Lane only used buried screws, *without* plates, in 1894. In 1896 Halsted began using pure silver rather than German silver for both plates and screws because he and Bolton thought silver was antiseptic or self-sterilizing when implanted.

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[15] W. A. Lane, "(Report #XXXI) A method of treating simple oblique fractures of the tibia and fibula more efficient than those in common use (read April 13, 1894)," *Trans. Clinical Soc. of London*, 1894, *27*: 167-175.

[16] Crowe, *op. cit.* (n. 14 above).

[17] Hansmann, "Eine neue Methode der Fixirung der Fragmente bei complicirten Fracturen," *Verhandl. d. deutsch. Gesellsch. f. Chir.*, 1886, *15*: 134-137.

The ability to avoid infection after internal fixation of fractures was also facilitated by Dr. Halsted when he introduced rubber gloves into the operating room in 1890,[18] and they have allowed the modification or eventual elimination of Lane's time-consuming and clumsy "no-touch technique" in fracture surgery.

Once the idea of internal fixation had developed sufficiently the need for an advance in metallurgy and design of implants became obvious. Dr. William O'Neill Sherman noted in his paper of 1912:[19] "While Hansemann, some 25 years ago, introduced silver plates and screws for the repair of fractures, it remained with Mr. Arbuthnot Lane of London, to prove the practical superiority of steel to silver." Sherman then cited reports of 18 cases in which the Lane plates had broken. Therefore he carried out further investigations and published his paper of 1912 entitled, "Vanadium Steel Bone Plates and Screws." He considered the formulation of the steel then available; the manufacturing specifications for plates and self-tapping screws; and the comparative merits of silver, aluminum and steel for burying in the body. "Crucible steel" or "tool steel" was the brittle kind of steel employed in the manufacture of bone plates at that time. They were strong but they wouldn't bend. He changed that to the use of "well-blued," or annealed, Vanadium steel for plates and screws (Fig. 5). As Chief Surgeon of the United States Steel Company, Dr. Sherman had access to resources of metallurgy not available to other surgeons in the United States.

In 1912 Dr. Sherman made a statement which is interesting in retrospect because it probably showed a universal lack of perception at that time of the importance of biologic tolerance of implants:

> During the past two years, using Vanadium steel, we have removed 17 plates in a series of 55 cases. In several cases the plates were completely enveloped by hard bone. . . . If the plates are removed in from six weeks to eight months or later, the tissues immediately surrounding the plates will usually be found stained with iron. *This, however, is not objectionable*. Screws were found in position as at the time of surgery, it being necessary to thoroughly unscrew them before they could be removed. . . . In some cases the bone about the thread had softened sufficiently to cause the screw to become loose.

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[18] J. F Mitchell, "The introduction of rubber gloves for use in surgical operations," *Ann Surgery*, 1945, *122*· 902-904; Hunter Robb, "The use of rubber gloves and gauntlets as a means of preventing infections in surgery," *Cleveland Med. Gaz*, 1900-1901, *15*. 531

[19] W. O Sherman, "Vanadium steel bone plates and screws," *Surg , Gyn & Obst* , 1912, *14*· 629-634

| % Composition | Tool Steel | Vanadium Steel |
|---|---|---|
| Carbon | 1.0 | 0 45 – 50 |
| Manganese | 0 3 – 0 5 | 0 7 – 0 9 |
| Silicon | 0 15 or less | 0 03 or less |
| Phosphorus & Sulfur | 0 03 | ——— |
| Chrome | ——— | 0 09 – 1 1 |
| Vanadium | ——— | 0 18 |
| Iron | Balance~ (98%) | Balance~ (97%) |

*Carbon* and *chrome* were considered to be those elements which gave greater elastic limits, that is, toughness and bending properties.
*Vanadium* was considered to enhance both elasticity and ductility

Figure 5

The remark he subsequently made, that he didn't think there was *necrosis* of bone tissue when the screws *were loose*, is not only surprising in that he had just implied that although most of the screws were tight the bone around some had become soft, but then his statement about "no bone necrosis" was not supported by any histology.

Furthermore, in Sherman's illustrations of plate-screw application, the screws only penetrated one cortex of the bone. This implies that the full concept of rigid internal fixation had not fully developed in 1912 and was not uppermost then in the surgeon's mind; only approximation.

Between 1913 and 1918 Fassett[20] equalized legs of unequal length in seven patients using Lane plates and screws and supplemented in some patients by blocks of ivory or magnesium to stabilize the osteotomized femoral fragments in their elongated position. Illustrations of his x-rays, or "skiograms," were of poor quality, but I think he must have put the screws through both cortices of the femur in view of the type of surgery he was doing.

Obviously by 1914 plates and screws were being used for increasingly rigid internal fixation, but still in 1918 no concern was expressed by Fassett about metal corrosion in body fluids.[21] However, between 1918 and 1924 the biologic tolerance to metal implants concerned more surgeons.

In 1924, Zierold[22] operated upon a number of dogs in which he drilled holes in numerous bones in various parts of the skeleton and placed these metals in the holes: gold, silver, aluminum, zinc, lead, copper, nickel, high carbon steel, stellite, etc. Tissues about the pieces of copper showed much discoloration and marked overgrowth of the bone. About the gold, silver, and aluminum there was excessive periosteal growth of bone. Lead caused indifferent bone reaction. Nickel produced marked irritation and some stimulation of new bone. Iron and steel caused discoloration and soft tissue reaction. Gold and stellite were readily tolerated by bone and became encapsulated early. Silver and lead, which easily corroded, caused connective tissue reaction. Zinc was corroded readily and interfered with bone regeneration. Zierold concluded that steel and iron definitely inhibit bone regeneration and that "stellite" caused the least reaction of all the metals he used. At that time he described "stellite" as an alloy containing 58% cobalt, 35% chromium, 4% tungsten plus small amounts of iron, carbon, etc. This was the inception of the chrome-cobalt alloys for human metal implants (Fig. 6).

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[20] F. J. Fassett, "An inquiry as to the practicability of equalizing unequal legs by operation," *Amer J. Orthop Surg.*, 1918, *16*: 277-287.

[21] In fact, the magnesium blocks in two patients produced *gas bubbles* in the tissue as noted in the x-ray by Fassett. This would concern me now, but it didn't seem to bother Fassett in 1918

[22] A A Zierold, "Reaction of bone to various metals," *Arch Surg* , 1924, 9 364-412

| Stellite | |
|---|---|
| Cobalt | 58% |
| Chromium | 35% |
| Tungsten | 4% |
| Iron, ) | |
| Carbon, ) | 3% |
| etc - ) | |

Contrasted to *Stainless Steel, Stellite* was an early alloy made principally of cobalt and chromium for human implants

Figure 6

In 1937, Venable, Stuck and Beach[23] reported on their experiments with screws made of various metals and metal combinations and metal alloys in the radii of dogs. They used the parameters of roentgenograms, histology, direct observation, cultures for bacteria and chemical analyses of tissues about the various screws and also analyses of the liver and kidney for abnormal mineral elements of some of their test animals. They were mostly interested in the electrolytic effects which occurred after these several screws had been applied in various combinations and left *in situ* for two, four, six, and eight weeks after surgery.

The reason for their study was that they suspected that in surgical fracture treatment and other orthopaedic work that involved metal implants electric couples were causing implant loosening, other adverse tissue reactions, and metal corrosion. They suspected that these problems were created by the use of plates in conjunction with screws, bands, wires, washers, lock-nuts, nails, and other fastening devices when two or more different types of metal were used in the same bone. They reasoned that under these conditions Ohm's law becomes effective. They stated:

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[23] C S. Venable, W G Stuck and A Beach, "The effects on bone of the presence of metals, based upon electrolysis," *Ann. Surg* , 1937, *105* 917-937

> An electric current is directly proportionate to electric conductivity and inversely proportionate to resistance. Conversely, metals that have nearly the same electric conductivity (E.M.F.) offer less resistance to a current between them. . . The usual Lane plate furnished by instrument dealers is nickel plated or chrome plated steel in which the iron is exposed at the holes into which screws of plain steel, chromium plated steel, rustless steel, or galvanized iron are to be placed. What happens? Immediately an electrolytic reaction is started at the point of contact between each screw with a metal plate and the current is carried by the contacting plate to each other screw There is then produced a progressive, proliferative reaction in the tissue that is protective (reactive) against the current. *A necrosis and destruction of bone cells also takes place* and an accumulation of discolored exudate, which contains elements of the metals in solution, forms about the metals and enters the circulation. In two weeks the screws, which were placed in the bone with forcible resistance may be picked out with the fingers.

Venable et al.[24] in 1937 as one of their 14 conclusions stated:

> The alloy of least reaction that we have found is one called Vitallium which contains no iron and consists of cobalt, chromium and molybdenum. There was no tissue reaction or bone change at the site of any Vitallium screw though it was checked in many experiments with several different combinations of metals Unmodified Vitallium is 65% cobalt, 30% chromium, 5% molybdenum with traces of magnesium and silica and *no iron* [Fig. 7A].

Probably the final conclusion of this excellent study was facilitated by a contact between Dr. Venable and his dentist, Dr. Taylor. In 1929, eight years before this report of Venable et al., dental appliances were being made of cast Vitallium.

A number of cobalt alloys have followed (Figs. 7B, C, D), the last being Nitinol—"the metal with a memory" (Fig. 7E) which only contains 1% cobalt.

In 1939 in the first edition of *Operative Orthopaedics* by Dr. Willis C. Campbell[25] the internal fixation of fractures of the neck of the femur is discussed:

> Internal fixation of this fracture by nails, screws, and [bone and ivory] pegs is by no means a new procedure, as the first report of this method of treatment was made

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[24] *Ibid*

[25] W C Campbell, *Operative Orthopaedics* (St. Louis, C V Mosby, 1939), pp 520-521

by Langenbeck in 1850.[26] Successful results were reported by Nicolaysen [in 1897],[27] and others prior to the development of the roentgen ray. Thomas,[28] in 1921, and E. D. Martin and A. C. King,[29] in 1922, reported [a few isolated] excellent results following the use of steel wood-screws for internal fixation. At that time, internal fixation was not uniformly successful, either because of inaccurate reduction or because of lack of dexterity on the part of the surgeons in introducing the [fixation] material. As a consequence, this method fell into disuse except among a small number of surgeons, who probably were more expert in properly placing the fixation. Smith-Peterson is responsible for reviving and popularizing the procedure and originating a pin for the purpose.[30]

Watson-Jones[31] stated in 1944: "The history of internal fixation of the femur illustrates the interrelation of advances in various branches of science. Round nails, square nails, and screws had often been used with little success" for osteo-synthesis of fracture of the femoral neck prior to Smith-Petersen's triflanged nail. For instance, Nicholayson described a nailing technique in 1897,[32] and in 1920 A. Basset,[33] reporting the clinics of P. Delbet, reviewed internal fixation by means of a screw. There were four successful results in thirty-five operations. Further reports from the clinic of Prof. Delbet on this subject appeared in 1921[34] and 1927[35] but patients over 60 years continued to show a high percentage of failures with this method of treatment.

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[26] During discussions in the fourth session of the seventh congress of the German Surgical Society held on 13 April 1878, Langenbeck cited a case of unhealed fracture of the neck of the femur which he had treated in the 1850s, successfully accomplishing fixation by using a silver-plated screw In translation his description reads, "After the fragments had been aligned carefully, I made an incision of one centimeter towards the middle of the trochanter and from here I drilled with a fine drill into the neck of the femur so deeply that the small fragment must have been hit, and now I screwed a galvanically silver-plated screw especially constructed for this purpose through the drilled hole to the same depth. Indeed the bones were successfully united so that they were completely immobilized ." *Verhandl d deutsch Gesellsch. f Chir* , Berlin, 1878, *7*. 92-93

[27] J Nicolaysen, "Lidt om Diagnosen og Behandlinger af Fr colli femoris," *Nord med. Ark* , Stockholm, 1897, n.F. *8*· 1-21.

[28] T T Thomas, "Fixation by a wood screw without arthrotomy in certain fractures of the neck of femur," *Am J Surg* , 1921, *35*· 292-295

[29] E. D. Martin and A. C King, "Preliminary report of a new method of treating fractures of the neck of the femur," *New Orleans Med & Surg J.*, 1922-23, *75* 710-715

[30] M N. Smith-Petersen, E G Cave and G. W Van Gorder, "Intercapsular fractures, neck of femur," *Arch Surg.*, 1931, *23* 715-759

[31] R Watson-Jones, *Fractures and Joint Diseases*, 3rd ed (Baltimore: Williams & Wilkens, 1944), vol 2, p. 633

[32] Nicolaysen, *op cit* (n 27 above)

[33] A Basset, *Ann clin chir du Prof Pierre Delbet*, No. 7 (Paris Masson, 1920)

[34] P Delbet and A Basset, *Fractures du col de femur* (Paris· Masson, 1921)

[35] Leveuf et Girode, "Le traitement des fractures du col du femur," *Ann clin chir du Prof. Pierre Delbet* (Paris: Masson, 1927)

## Figure 7.

### *COBALT ALLOYS FOR SURGICAL IMPLANTS*

A Co-Cr-Mo

*Cast* Vitallium (Howmet Corp ), Zimalloy (Zimmer Co.), Stellite "21", Vinertia, Protasul 2.

| % by Weight | Co-Cr-Mo *Casted* Alloy | |
|---|---|---|
| | Min | Max |
| C | 0.20 | 0.35 |
| Cr | 27.0 | 30.0 |
| Fe | | 1.0 |
| Mn | | 1.0 |
| Mo | 5.0 | 7.0 |
| Ni | | 2.5 |
| Si | | 1.0 |
| Co | ~67.8 | ~57.15 |
| E—modulus, KP/mm² | 19000 | 21000 |
| Yield strength, 0,2,KP/mm² | 43 | 56 |
| Ultimate strength, KP/mm² | 63 | 77 |
| Fatigue strength, KP/mm² | 19 | 29 |
| Elongation, % | 8 | |
| HV—Hardness, KP/mm² | 288 | 384 |

B Co-Cr-Ni-W

*Wrought* Vitallium (Howmet Corp.); Neutrillium, Stellite "25", Haynes "25" Can be machined unlike the Co-Cr-Mo alloy

C Co-Cr-Ni-Mo "MP35N2"

Trademark of the Standard Pressed Steel Co

| Elements | % by Weight |
|---|---|
| Cr | 20 |
| Mo | 10 |
| Ni | 35 |
| Co | 35 |

Ref Escales, F , Galante, J., Rastoka, Wm. and Coogan, P. S · *J. of Biomed. Materials Research,* 9:303-313, 1975

D. Co-Cr-Ni-Mo-Ti MP35N

Manufactured by the Latrobe Steel Co. It is a wrought alloy treated by a repeated vacuum melt process. Also called "Protasul 10". Below are the practical variation limits allowed in manufacture of "MP35N2" (cf. Fig. 7 C)

| Elements | % by Weight | |
|---|---|---|
| | Min. | Max. |
| C | | 0.025 |
| Cr | 18.0 | 21.0 |
| Fe | | 1.0 |
| Mn | | 0.15 |
| Mo | 8.5 | 10.5 |
| Ni | 33.0 | 37.0 |
| Si | | 0.15 |
| Ti | 0.65 | 1.0 |
| Co | ~39.85 | ~29.175 |
| E—modulus, KP/mm$^2$ | 21000 | 23000 |
| Yield strength 0,2,KP/mm$^2$ | 62 | 97 |
| Ultimate strength, KP/mm$^2$ | 82 | 116 |
| Fatigue strength, KP/mm$^2$ | 52 | 58 |
| Elongation, % | 35 | 55 |
| HY—Hardness, KP/mm$^2$ | 288 | 384 |

E. Co-Ni-Ti ----Nitinol-----

(Nickel-titanium—Naval Ordance Laboratory—U.S.A.)

Now being tested in vivo, for biologic compatibility.

| Nominal Composition | |
|---|---|
| Elements | % by Weight |
| Co | 1 |
| Ni | 55 |
| Ti | 44 |

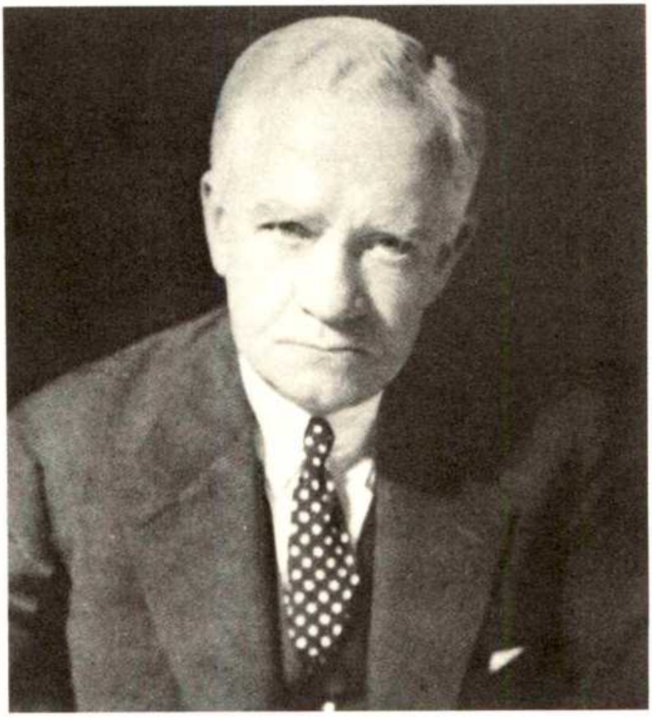

Figure 8
Marius N. Smith-Petersen, 1886–1953
From *J. Bone & Joint Surg.*, 1953, *35A:* 1043.
*Courtesy of The Journal of Bone and Joint Surgery*

It was not until Smith-Petersen[36] (Fig. 8) devised the triflange nail that one problem with internal fixation of femoral neck fractures was solved. All metals previously used, even annealed Vanadium steel developed by Sherman in 1912, caused reaction in the adjacent bone with decalcification and loosening of the nail or screw. 18-8SMO steel (Fig. 9) caused relatively little reaction. Vitallium, which is less resistant to strain, causes almost no reaction. Also triflange nails, because of their design, maintain their grip in the bone long after their introduction.

The story of the triflange nail and its relatively successful general application is a bit more complicated. As the story unfolds, it depended on the design and metal used for the internal fixation device; the ability to quite accurately

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[36] Smith-Petersen, *op. cit.* (n. 30 above).

Figure 9.

STAINLESS STEEL FOR SURGICAL IMPLANTS

| Elements | 18-8 SMO | AISI—316 L ** *** | |
|---|---|---|---|
| | % by Weight | Min. | Max. |
| C | 0.07 | | 0.03* |
| Cr | 18.0 | 17.0 | 20.0 |
| Cu | | | 0.1 |
| Mn | | | 2.0 |
| Mo | 2.75 | 2.5 | 4.0 |
| Ni | 8.0 | 12.0 | 16.0 |
| P | | | 0.03* |
| S | | | 0.03* |
| Si | | | 0.75 |
| Fe | ~70% | ~68.5% | ~57.1% |
| Elastic modulus, KP/mm² | | 19,000 | 20,000 |
| Yield strength, 0,2,KP/mm² | | 28 | 33 |
| Ultimate strength, KP/mm² | | 51 | 53 |
| Fatigue strength, KP/mm² | | 24 | 31 |
| Elongation % | | 63 | 74 |
| HV-Hardness, KP/mm² | | 135 | 149 |

* Corrosion facilitating factors.

** There are several families of stainless steel. Two of these families, the "ferritic" and the "martensitic" are not acceptable for internal use. Both are magnetic. 316L belongs to the third family called "austenitic". The "austenitic" family of the stainless steels is non-magnetic, and is more corrosion resistant and yet has necessary strength.

At one time one was able to clean out all the non-acceptable stainless steel devices that might be available in an operating room by running a magnet over them. This would, of course, get rid of the first two families: the feritic and the martensitic.

In 1964 the American Society for Testing Materials was organized in the U.S.A. An F4 Committee on Surgical Implants was set up to describe material standards. Present stainless steel for implants, accepted by the F4 Committee, is that stainless steel known as AISI 316 LVM. Spelled out, this means the *A*merican *I*ron and *S*teel *I*ndustries, arbitrary steel type number *316*, with *l*ow carbon content, prepared by a *v*acuum *m*elt process. Metals must not only be formulated correctly but they must be processed correctly and handled correctly to prevent failure *in situ*.

*** Manufacturer: Joslyn Stainless Steel, a Division of Joslyn Mfg. Co., Fort Wayne, Indiana. See G. Schmeisser, "Progress in metallic surgical implants," *Membership Handbook ASTM Committee F4 on Surgical Implants* (Philadelphia: American Society for Testing and Materials, Oct. 1970), Appendix 1, pp. 31-53.

reduce the skeletal deformity by manipulation under safe anaesthesia[37] and the stable retention of the repositioned fracture fragments during the internal fixation procedure without opening the hip joint;[38] the introduction of a

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[37] Royal Whitman, "The abduction method, considered as the exponent for all forms of fracture at the hip in accord with surgical principles," *Amer. J. Surg.*, 1933, *21*: 335-344.

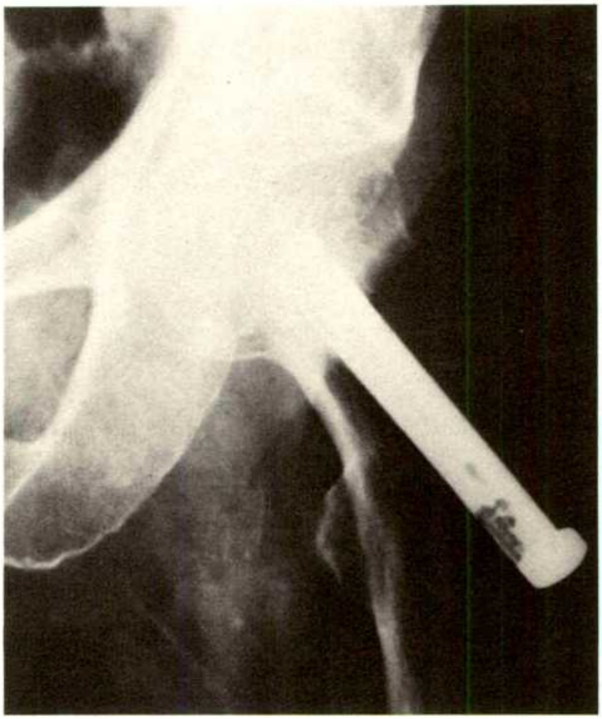

Figure 10. X-ray of a healed fractured hip. The pinning was done by Dr. Smith-Petersen and the nail was left in place despite the obvious erosion of the fins at the distal end of the triflange nail near the brass cap. This nail never gave any medical problem. Courtesy of J. Drennan Lowell, M.D.

guide pin for the nail under two-plane x-ray control;[39] and a cannulization of the nail or screw to slip over the guide pin.[40]

Drs. M. N. Smith-Petersen, Edwin F. Cave and George W. Van Gorder in 1931[41] published a landmark paper concerning the internal fixation of intercapsular fractures of the neck of the femur by a triflange nail. All of the

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[38] Nicolaysen, *op. cit.* (n. 27 above).

[39] Sven Johansson, "On the operative treatment of medial fractures of the neck of the femur," *Acta Orthopaedica Scandinavica*, 1932, *3*: 362-385; T. King, "Recent intracapsular fractures of the neck of the femur: a critical consideration of their treatment and a description of new technique," *Med. J. of Australia*, Jan. 5, 1934, *1*: 5-15; S. Ostrowski, "Zur Richtungbestimmung des Bohrkanals bei der Verschraubung von Schenkelhalsbrüchen," *Beitr. klin. Chir.*, 1926, *138*: 368-381.

[40] Max Richard, "Die Behandlung der Schenkelhalsfraktur," *Schweiz. med. Wschr.*, 1930, *60*: 615-616; *idem*, "Einfache Richtungsbestimmung zur Schraubung der Schenkelhalsfraktur," *Zentr. f. Chir.*, 1931, *58*: 837-839; *idem*, "Die langstreckige (femoro-pelvine) Verschraubung der Schenkelhalsfraktur," *Chirurg*, 1930, *2*: 1027-1032.

[41] Smith-Petersen, *op. cit.*(n. 30 above).

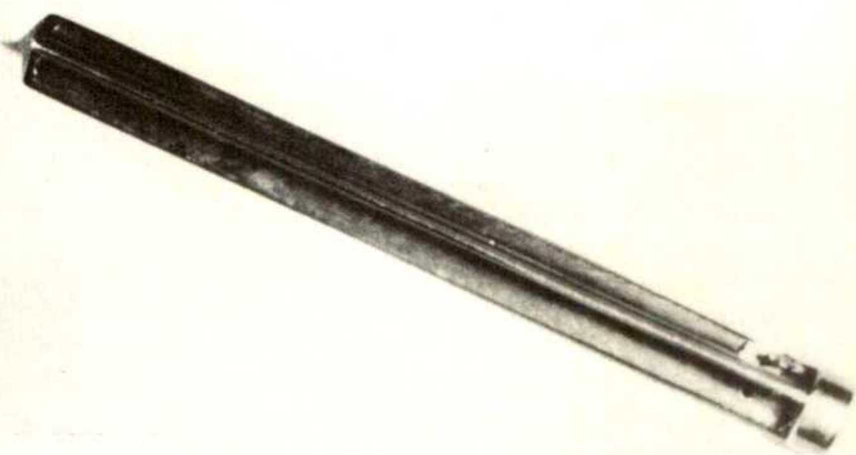

Figure 11. This is a photograph of an early Smith-Petersen nail. It is three-flanged and has a brass cap next to the eroded area in the fins. The three-flanged nail was not used before 1925 and the corrosion resistant stainless steel nail was first used just prior to 1930. This nail was inserted in Boston between 1925 and 1930 to fix a slipped capital epiphysis in an adolescent boy. It was removed approximately two years after its insertion by Dr. P. D. Wilson, Sr. in New York City. The patient from whom the nail was removed eventually became a patient of Dr. J. Drennan Lowell in Boston for a total hip replacement and he gave Dr. Lowell the nail. It is obvious that one of the fins is bent near the tip. Also the fins are not exactly straight, showing a slight irregular deviation along the length of each one. Dr. Smith-Petersen noted that the steel in these nails used between 1925 and 1930 was softer than the later ones which were made of 18-8 SMO stainless steel.

authors were members of the Fracture Service of the Massachusetts General Hospital. They only submitted the paper for publication after five years of experience with 24 consecutive patients and the paper was published after six years of experience with the triflange nail. Usually this is called the Smith-Petersen nail. The authors noted in 1931 when the paper was finally published, "The nail as now used is made of rustless steel and has three flanges. Previously nickel-steel was tried but was found to be too soft" (Figs. 10 and 11).

In a letter of April 7, 1976, addressed to the author it was stated,[42]

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[42] J. Drennan Lowell, Personal correspondence, 1976-77.

> Among my possessions is one of the early nails designed by Dr. Smith-Petersen. . . . These nails were made out of a nonstainless type of steel and had a brass cap. This particular nail was not inserted by Dr. Smith-Petersen but rather by Dr. Philip Wilson, Sr., before he left for New York, and was used to fix a slipped capital femoral epiphysis in an adolescent boy. This man eventually became a patient of mine and had a total hip replacement about two years ago. He gave me the nail which had been removed about two or three years after it had been inserted and, as you might expect, there is much corrosion of the steel portion near the brass cap [Figs. 12 and 13].

The triflange nail was apparently devised in the mid 1920s just before dependable stainless steel (18-8 SMO) had been developed (see Fig. 9), but by 1931 the authors stated that they were using stainless steel. This internal fixation device solved two problems: it had a maximum surface exposed to the bone on either side of the fracture site with a minimum mass of steel. This was probably the most important contribution: namely, adequate solid internal fixation by maximum contact with bone with a minimum encroachment on the bone tissue and its blood supply. The type of steel in the nail and the electrolysis between the nail and its brass cap after insertion was probably not of such great importance as the triflange design itself. Despite the corrosion that occurred in the region of the outer cortex of the femur (Fig. 13) where the brass cap and the steel nail provoked an electrolytic reaction in body fluids, the fractures healed in 75% of the patients. I suppose it was fortunate that the electrolytic reaction was confined to the region of the nail that was most distant from the fracture site and, secondly, that the corrosion didn't usually cause loosening until the fracture had healed. The authors do mention in their paper of 1931 that the original steel they used was too soft. Also the corrosion was probably disturbing. They had switched to stainless steel by 1930.

In 1931 Dr. Sven Johansson,[43] Surgeon-in-Chief of the Allmanna Och Sahlgrenska Sjukhuset in Göteborg, Sweden,

> chanced to see a preparation originating from post mortem examination of a case operated in Oslo according to (the method of) Smith-Petersen by Lindboe, and I observed then the firm fixation of the fragments still two months after the nail was put in and before the bone had united. I determined then to endeavor to turn the advantages of the method to account without exposing the fracture: in other words, to work out a method of extra-articular osteosynthesis with the Smith-Petersen nail. Only by this means would the operation be relatively slight—so slight that, even having regard to the advanced age at which most of these fractures occur, it [would] seem to me to be justified. The advantages one derives from osteosynthesis are: firm fixation, shor-

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[43] Johansson, *op. cit.* (n. 39 above).

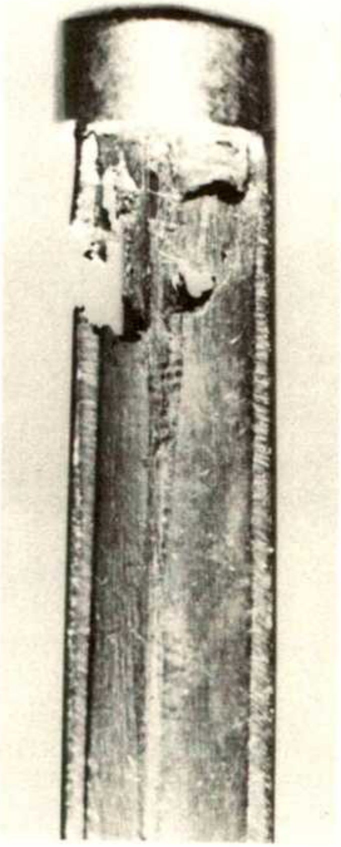

Figure 12. This shows detail of the eroded area near the brass cap end of the nail shown in Figure 11.

tened period of union and simplified after-treatment. My experience as yet is too small to enable me to decide in what manner the period of union can be reduced, but I believe I can already state that it is possible to satisfy the other two requirements.

Dr. Johansson then remembered an apparatus described in 1927 by Ostrowski[44] and modified it to fit on his fracture table. He included in this apparatus a drill which he had described in *Zentralblatt für Chirurgie* in 1923. To the fracture table was attached two x-ray tubes, one for obtaining true lateral (cross-table) roentgenograms and the other for AP views of the hip.

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[44] Ostrowski, *op. cit.* (n. 39 above).

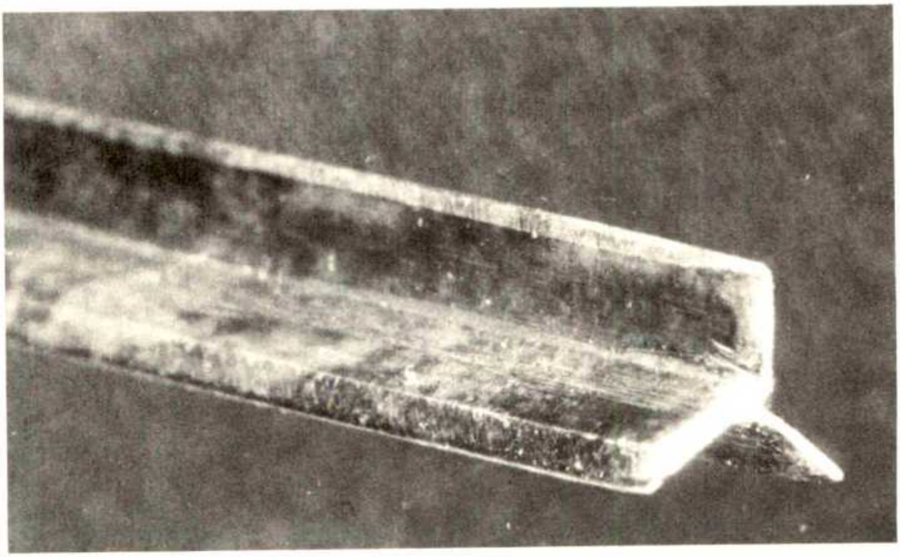

Figure 13. End-on view of the tri-fin nail illustrated in Figure 11. The bottom fin is bent slightly.

The fracture table permitted stabilization of the femoral neck fracture after reduction of deformity by the Whitman method[45] in a reduced position with the femur in 25° abduction and 25-30° internal rotation so that the neck of the femur should be parallel to the surface of the fracture table as Ostrowski had determined in his investigations.[46]

His only comment about the cannulization of the Smith-Petersen nail was: "which for this purpose has been furnished with a central canal." Apparently the idea of a cannulized nail or screw was developed by Max Richard and published in 1930 and 1931.[47]

His other comment about the type of metal follows: "In my first cases I used nickelplated steel nails, and since then nails of stainless steel. The question of the best metals for use in osteosynthesis has not yet been conclusively settled but so much may be taken to have been established, that nails of a single metal are better than for instance plated or chromed (ones), and also that the metal used ought to be rustless. Of the many works treating the question of the best, or perhaps rather the least troublesome, metal for osteosynthesis I would refer to Max Lange's (work).''[48]

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[45] Whitman, *op. cit.* (n. 37 above).

[46] Ostrowski, *op. cit.* (n. 39 above).

[47] Richard, see three references in n. 40 above.

In 1934 a paper by Thomas King of Australia[49] was published in which he described his independent development of the same general idea as Johansson had had of simplifying the surgical procedure for the internal fixation of fractures of the neck of the femur without opening the hip joint. He used ''stiff piano wire'' as a guide for his cannulated S-P nail. His only comments about the modified S-P nail are in the legend for Figure 8 of his paper, namely: 2 + the three flanged rustless nail, which has been modified, however, so that a 1.5 mm. (one sixteenth inch) steel wire may be passed along a perforation through its entire centre and so act as a guide. Also the head (of the nail) contains a thread for the extractor bolt.

''3 + The special perforated three flanged nail showing the construction in cross section.''

Dr. King also illustrated the nail extractor of his own design which he stated was, ''similar to a motor wheel extractor.'' He described in detail his technique of insertion of his modified S-P nail, but the method of positioning the patient on the fracture table presently used is more similar to that described by Johansson.

In 1934 Dr. Melvin S. Henderson[50] of the Mayo Clinic reviewed the various treatments of fresh and old fractures of the femoral neck and at the end of his paper noted that five patients had had internal fixation of femoral neck fractures with a cannulated S-P nail. Diagrams, but not details of the insertion of the cannulated nail, are shown. In 1937 he described in detail the method of ''pinning'' a fractured femoral neck using a cannulated lag screw. The canal in the lag screw accepted a large Kirschner wire (#47 drill) .062 of an inch in diameter.

Subsequently, the guide pin has increased to the diameter of a small Steinmann pin or 3/32 of an inch in diameter. Arthrotomy is rarely used now for the insertion of an internal fixation device for fractured neck of the femur. The Johansson-Ostrowski technique for positioning the patient and the femur on the fracture table once reduction is obtained (without the plaster boot) and inserting the guide wire (or pin) under biplane x-ray control are in general use today for "pinning" a fracture of the neck of the femur when such a procedure is indicated. The tri-fin nails are almost without exception cannulated now but are still called Smith-Petersen nails. The method of reducing the fracture deformity is now usually that of Leadbetter,[51] with the addition of McElvenny's maneuver[52] in some instances, rather than Whitman's method.[53]

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[48] Max Lange, ''Der Kruppstahldraht als Knochennahtmaterial,'' *Zeitschr. f. orthop. Chir.*, 1926, *47*: 519-546.

[49] King, *op. cit.* (n. 39 above).

[50] M. S. Henderson, ''Internal fixation of fractures of the neck of the femur,'' *Arch. Surg.*, 1937, *35*: 419-430.

Ferguson, Laing and Hodge in 1960[54] showed that inertness of metal is a relative matter and this is still true. Exchange of ions between all metallic inserts and tissue fluids and cells can be demonstrated if tests are sufficiently critical. Furthermore, metals which are relatively inert in themselves, but having dissimilar composition when used in the same bone, may cause electrolytic reaction of greater or lesser degree.

Surface treatment and careful operative handling and all other manipulations of metallic devices, especially those in the stainless steel series, is of great importance.[55]

Implants made of pure single metals are now being tested and some alloys new for implants are being developed (Fig. 7E). Titanium is very light; cost is reasonable; as a pure metal it has excellent biologic compatibility; and its strength is good but not as good as that of steel. It forms surgically useful alloys. Zirconium is also very light but the cost is high; biologic compatibility is excellent; and its strength is good.

18-8 SMO stainless steel was quite well tolerated biologically and about 1930 became available not only to Smith-Petersen, who had developed the triflange hip nail in Boston, but also to Clay Ray Murray in New York City, who was starting a study of over 1,000 open reductions of long bones with internal fixation—a study which he carried out between 1931 and 1943, the results of which even now appear excellent.[56]

With Dr. Clay Ray Murray in the U.S.A. the concept of close approximation and rigid internal fixation of long bone fractures by the use of plates and screws and an oblique screw to increase rigidity and apposition was emphasized as early as 1931. Clay Ray Murray in 1944,[57] after describing exactly how the plates and the screws holding the plate to the bone should be applied, continues:

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[51] G. W. Leadbetter, "A treatment for fractures of the neck of the femur," *J. Bone & Joint Surg.*, 1933, *15*: 931-940.

[52] R. T. McElvenny, "Reflections and opinions regarding the present status of fracture treatment," *Clin. Orthopaedics*, 1953, *2*: 20-26.

[53] Whitman, *op. cit.* (see n. 45 above).

[54] A. B. Ferguson, P. G. Laing and E. S. Hodge, "The ionization of metal implants in living tissues," *J. Bone & Joint Surg.*, 1960, *42-A*: 77-90.

[55] C. O. Bechtol, A. B. Ferguson and P. G. Laing, *Metals and Engineering in Bone and Joint Surgery* (Baltimore: Williams & Wilkins, 1959).

[56] F. W. Bancroft and C. R. Murray, *Surgical Treatment of the Motor Skeletal System* (Philadelphia: J. B. Lippincott, 1945), pt. 2, sect. 7, pp. 615-688; Murray, "Detailed operative technique" (n. 1 above).

> The clamps holding the plate are then removed: the ones holding the fracture are left in situ. A hole is then drilled on a plane at a right angle to the plane of the screws holding the plate, so as to go from one fragment, across the fracture line, and out through the other fragment. If the fracture line is oblique or spiral, this hole can be driven straight across the fracture line. If the fracture line is transverse, the drill hole has to be made obliquely across the fracture line from one fragment to the other, and counter sinking for the screw head is indicated to avoid shearing strain. A screw is then inserted through this drill hole. This screw serves to take up torsional strain and eliminate torsional mobility at the fracture site, and to take this strain from the plate and its screws, thus distributing the strain. A second similar screw can be applied if necessary. It should be here emphasized that the placing of this transfixion screw has proved, in our experience, to be a vital factor in securing rigidity of fixation and distribution of strain. With its use, the difficulties due to loosening of screws, breakage of materials, and reaction to the fixation metal have been reduced to a minimum, and visible absorption at the fracture site has been eliminated in clean cases. In our experience late absorption at the fracture site with a visible gap—so-called distraction—in the vast majority of cases results from only two causes, infection or movement of the fragments at the fracture site. The latter is frequently present when a plate alone is used, and may result in absorption. The cross fixation is our guarantee against this. It is a most valuable and, in our opinion, necessary feature of the fixation.
>
> The clamps are now removed and the fracture is tested for rigidity of fixation. If an additional transfixion screw is needed, it is applied.

This concept of rigidity as a factor in efficient fracture healing was later amplified by Jergesen[58] (also a resident of Clay Ray Murray), who first tried the routine use of two oblique screws and one long plate with screws, but then went on to two long plates and screws. Dr. Jergesen also recalled[59] some experimental work while he was a resident on specially designed stainless steel plates which Clay Ray Murray used in patients. These plates were much thicker and wider and were rectangular unlike Sherman plates and made of 18-8 SMO steel. They were much stronger than Sherman plates.

> Indeed the plates held in the vise with an extension of about one meter which was vibrated by a doorbell buzzer was used by Steve [Dr. Steven Hudack, a member of

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[57] Murray, "Detailed operative technique" (n. 1 above).

[58] F. H. Jergesen, "Healing of clinical fractures of the tibia. A. With rigid internal fixation," in: *The Healing of Osseous Tissue*, ed. R. A. Robinson (Washington, D.C.: National Academy of Sciences-National Research Council, 1967).

[59] Floyd H. Jergesen. Personal communication, 1976.

the Fracture Service faculty] to demonstrate how crystallization took place from cyclically applied bending moments: Clay Ray Murray had the idea that muscles created similar constant bending moments causing some plates to crystallize and break *in vivo*.

Dr. Jergesen states,

> To my recollection, Clay gave great credit to Albin Lambotte for his teaching. Lambotte's book was written in about 1907 and a few years ago the Belgian Orthopaedic Society issued a volume written by Edouard Vander Elst to commemorate his work. I happened to have been a guest of the Society that year and remember the exhibition of Lambotte's instruments [for application of internal fracture fixation devices].

E. Burke Evans in the obituary which he wrote in 1963 stated,

> In 1949 Dr. George Eggers [Fig. 14] received the Gold Medal Award of the American Academy of Orthopaedic Surgeons for his exhibit entitled: "The Influence of the Contact-Compression Factor on the Osteogenesis of Surgical Fractures."[60] In that same year he was the recipient of the Robert Danis Award for important work in the field of operative treatment of fractures. His firm insistence upon the principles of stability, contact, *and compression*, rather than (pure) rigidity, in the open treatment of fractures caused many surgeons to think, if not to believe. The slotted plate which he designed was widely known at the time of his death in 1963 as the Eggers contact splint.[61]

The concept of compression of living bone fragments to facilitate bone union in tuberculous knees which he was trying to fuse was recognized clinically by J. Albert Key in 1932;[62] the general principle was demonstrated in fracture healing experimentally by Krompecher in 1937;[63] clinically by Wenger in 1946;[64] and experimentally again by Eggers in 1949.[65]

Schenk and Willenegger in 1965[66] in their paper on "Morphological Findings in Primary Fracture Healing" presented on the basis of experiments in animals what appears to be the "ultimate" in cortical bone healing. They used both compression and rigid internal fixation.

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[60] G. W. Eggers, T. O. Shindler and C. M. Pomerat, "The influence of the contact compression factor on osteogenesis in surgical fractures," *J. Bone & Joint Surg.*, 1949, *31-A*: 693-716.

[61] E. Burke Evans, "An obituary for Dr. George Eggers," *J. Bone & Joint Surg.*, 1963, *45-A*: 1560-1562.

[62] J. A. Key, "Positive pressure in arthrodesis for tuberculosis of the knee joint, *Southern Med. J.*, 1932, *25*: 909-915.

[63] I. Krompecher, *Die Knockenbildung* (Jena: Gustav Fischer, 1937), esp. pp. 77-92.

[64] L. Wenger, "A new method of bone plating," *Surgery*, 1946, *20*: 541-547.

[65] G. W. N. Eggers, T. O. Shindler and C. M. Pomerat, "The influence of the contact-compression factor in bone surgery," *Arch. Surg.*, 1951, *62*: 467-474.

[66] R. Schenk and H. Willenegger, "Zur Histologie der primaren Knockenheilung," *Langenbeck Arch. klin. Chir.*, 1964, *308*: 440-452; *idem*, "Morphological findings in primary fracture healing," *Symp. Biol. Hung.*, 1965, *8*: 75.

Figure 14
George W. Nordholz Eggers, 1896–1963
From *J. Bone & Joint Surg.*, 1963, *45A*: 1560.
*Courtesy of the Journal of Bone and Joint Surgery*

The concept of compression in addition to rigid internal fixation has been found useful in fracture healing. Whether compression simply means increased stability by closest possible apposition of fragments and an increased coefficient of friction between the two bone fragments that prevents their motion under stress, or whether compression facilitates fracture healing by some roundabout route, such as by stimulating AMP activity in cells or by stimulating the neural system in bone, is not entirely clear.

If we were able to temporarily stimulate the local cellular response to a fracture so that the fracture would heal as though it had occurred in an infant, we probably would not be so interested in the internal fixation of fractures. However, in the adult we are faced with a very slow fracture healing process compared to that in the infant. Therefore for the present it appears that the development and interest in internal fixation methods will be with us for a long time.

## *Acknowledgments*

I wish to thank several friends and colleagues who helped me collect data for this paper. The first is Owsei Temkin, Professor Emeritus of the History of Medicine, The Johns Hopkins University; Frank Stinchfield and Floyd Jergesen, who were both residents under Dr. Clay Ray Murray in New York; Alan de Forest Smith, who preceded Dr. Stinchfield as Director of the New York Orthopaedic Hospital. Finally, Dr. Drennan Lowell of Boston, who supplied invaluable information about the history of hip-pinning by Smith-Petersen and his confreres.

Three items to which I have not specifically referred in the text were of great importance in supplying bibliography and in defining the trends of fracture treatment in the United States since 1800. They are: G. J. Curry, I. Cohn and F. V. Wade, *Profiles in Trauma* (Springfield, Ill.: Charles C Thomas, 1969); S. R. Gaston, "The role of leadership in the quality of fracture care," *Bull Amer. Coll. Surgeons,* Nov. 1975, *60:* 16-23; C. L. Scudder, "Oration on fractures," *Surg., Gynec. & Obst.,* Jan. 1930, *50:* 193-195.

# ON THE MUTUAL INDEBTEDNESS OF JAPANESE AND WESTERN MEDICINE*

ILZA VEITH

When I began my studies at Hopkins as a graduate student at Homewood, and worked on a thesis on the history of Japan, one of the sages of the History of Ideas Club—I believe it was Professor Arthur Lovejoy himself—said to me: "Regardless of what we do later on, we always seem to come back to our original scholarly interests."

He was so right, for I have always returned in my studies to various aspects of the history of Japan—if I have ever actually left them. Subsequently, when I joined the Institute of the History of Medicine, still deeply interested in everything Japanese, I was pleased to learn that there was a Hideyo Noguchi Lectureship and I hoped that, perhaps, one day I might be invited to present it. This day has now come, and I am deeply moved by the honor of this invitation.

What had puzzled me for a long time was how the Johns Hopkins University had come to establish this lectureship, since Noguchi—even though his name belongs to all science the world over—was never personally associated with Hopkins. But the answer came, thanks to Dr. Thomas Turner's *Heritage of Excellence*. Here we learn that:

> In 1929, Dr. Emanuel Libman, a prominent internist in New York City, made a gift of $10,000 to the School of Medicine to establish a lectureship in the Department of the History of Medicine to honor Hideyo Noguchi, a well-known scientist of the Rockefeller Institute for Medical Research, who had died of yellow fever in Africa. It was because of Libman's friendship with Welch and Osler that Hopkins was selected for this endowment.[1]

Doctor Libman had been Noguchi's personal physician in New York, and evidently a great admirer of his former patient, who had lost his life to yellow fever only one year prior to the generous bequest.

Because Noguchi has given so much to the world of medicine of East and West, I have chosen for the title of my lecture, "On the Mutual Indebtedness of Japanese and Western Medicine."

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* Presented as the Hideyo Noguchi Lecture at the Johns Hopkins University School of Medicine, Baltimore, Maryland, on October 20, 1977.

[1] Thomas Turner, *Heritage of Excellence* (Baltimore & London. The Johns Hopkins University Press, 1974), p. 256

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In the early Christian era, the disintegration of the Roman Empire and the ensuing great migrations shook the Western World to its very foundations. To this day, it seems that Europe and the Middle East never have recovered from the results of this upheaval.

At the very time when Western culture strove to regain some form of precarious balance, the ancient state of Japan evolved as a cohesive nation from a multiplicity of warring tribes and numerous competing clans, and has been in continuous existence ever since. This earliest Japan was under the rule of the ancestor of the present Imperial House and practiced *Shintō,* an animistic religion that was greatly preoccupied with ritual purity. Because of this preoccupation, which excluded all manifestations of illness and suffering from the ministrations of its priests, this religious system held little promise for the development of a healing art. However, from the beginning of their history the Japanese displayed a remarkable talent for the recognition and adaptation of superior foreign skills and knowledge, if they served their own benefit.[2]

At the outset, it was the complete system of Chinese medicine which was gradually adopted by the Japanese when they conquered Southern Korea in the 4th century A.D.[2] And Chinese medicine was to remain the predominant factor in the Japanese medical scene for at least one thousand years. And then, again, another foreign complex of medical knowledge supervened over what had hitherto been practiced. Western medical science found its way into Japan in the middle of the 16th century. For all these reasons, it would seem fair to say that any indebtedness in the structure of Japan's system of medicine was initially one-sided.

If one could weigh an intellectual debt on a regular scale, one might find that Japanese medicine learned more from the outside world than the world learned from the Japanese. But this was true before the 19th century only. At that turning point the Japanese scientists—though for the most part trained in Europe—made up for time lost and came through in grand style. The appearance of modern medicine on the world stage is inseparably linked to names such as Hideyo Noguchi, Sahachiro Hata, Kiyoshi Shiga, and above all, Shibasaburo Kitasato. These men, together with Robert Koch, Paul Ehrlich, Emil von Behring, and Louis Pasteur, to name but a few, became the innovators who made medicine into what it is today, and the indebtedness, thus established, became mutual.

Historically, however, it was the foreign world that brought its scientific and medical knowledge to Japan, and it is that story that I wish to tell you here, before I turn to the hero of today's lecture and his prodigious contemporaries.

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[2] G B Sansom, *Japan, a Short Cultural History* (New York· Appleton-Century, 1943), pp 27-29

The story of the introduction of foreign medicine into Japan began at the dawn of that country's existence, and, in fact, before the Japanese had evolved any form of documentation in writing. The first and most lasting wave of foreign medical knowledge was Chinese medicine which was introduced as a complete system during the 6th and 7th centuries A.D.

Chinese medicine and learning came to Japan not from China directly, but by way of Korea. It was thus also that Buddhism was introduced into Japan.[3] The main feature that distinguished Buddhism from Shintoism, the native religion of Japan, was that the exponents of the new religion, the Buddhist priests, combined compassion and medical care for the sick and dying, whereas the Shintō priests had shunned all such ritually defiling situations.[4] The Buddhist priests realized what later on the European traders and Portuguese Jesuits were to become aware of, and to exploit: this was the idea that a people is won most easily to new religions and political and trade alliances by a judicious admixture of health care to the principal intention and main objectives of international intercourse. At any rate, thanks to the Korean intercession and the challenge of the Buddhist priests, the Japanese adopted the Chinese method of writing in the 7th century A.D., and with it the system of medicine that was always entirely satisfactory, serving the Japanese more or less exclusively until the 19th century.

So much has been written by others and myself about Chinese medicine—in fact two of the earlier Noguchi lectures dealt with that subject—that I shall refrain from entering upon Chinese medicine for this occasion.[5] Suffice it to say, Chinese medicine reigned supreme in Japan until the middle of the 16th century when the Japanese were first confronted with the striking novelty of European medicine. And it is here that the central subject of our talk begins.

The first Westerners to come to Japan were the Portuguese. In 1542 or 1543—there is no certainty about the exact date—three Portuguese traders, traveling in a junk and bound for a Chinese port, were driven north in a storm to land on a small Japanese island off the coast of Tanegashima. The enthusiasm of their reception was heightened by the fact that they carried arquebuses, the first firearms ever seen by the Japanese. This new type of weapon could materially change the fortunes of the ongoing feudal wars, and the lord of the domain immediately ordered the firearms to be copied. The excitement about the arrival of the white men and their unusual possessions soon spread throughout the adjoining provinces. Upon the news of the discovery of Japan, the Portuguese trading settlements in China dispatched several expeditions.

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[3] Ilza Veith, "Medicine in Japan," *Ciba Symposia*, 1960, *11* (4), 1190-1193.

[4] *Ibid*., pp 1193-1202

[5] Edward H. Hume's *The Chinese Way in Medicine* (Baltimore Johns Hopkins Press, 1940) is based on a series of Noguchi Lectures, and Joseph Needham in 1949 presented as Noguchi Lecture an introductory survey of his multi-volume work, *Science and Civilization in China*, publication of which was begun in 1954 by the Cambridge University Press in Cambridge, England

The relations between the Japanese and these Western traders initially were excellent and mutually beneficial. The Portuguese found Japan to be a completely new and unexploited market, while the Japanese welcomed the importation of goods and skills from the West.

Western merchandise was soon followed by Western ideas: only a few years after the Portuguese traders arrived, the missionaries of the Jesuit order reached Japan. In 1549, Francis Xavier and two other Jesuit priests landed in Kagoshima. They were received most cordially, and were given unrestricted freedom by the local lord to convert his subjects to the Catholic faith. Conversion progressed rapidly, and even Buddhist priests, far from hampering the efforts of a competing religion, attended many of Francis Xavier's sermons.

The encouragement given to Xavier and his fellow missionaries by the lords of the various fiefs was not so much the result of their interest in Christianity itself, as it was their desire to attract the Portuguese traders. The missionaries overlooked the mercenary motive behind the tolerance shown to them and made full use of the vast possibilities for conversion which were thus offered, In their missionary zeal, however, they soon incurred disfavor when they began to describe the eternal damnation of those Japanese who had died without salvation by conversion. All Japanese—even the converted ones—were highly indignant at what they considered an insult to the object of the profoundest devotion: their ancestors. And Francis Xavier and his colleagues were ordered to leave Satsuma. During this period of disgrace, which lasted for several decades, the Portuguese, who were then called *Nambanjin*, or the "Barbarians of the South," devoted their time to giving medical help to the farmers and the peasants. Like the Buddhist missionaries of a thousand years earlier, the Jesuits saw that medical services rendered to the people in general—especially to the lower classes who could not afford to pay their own physicians—helped to increase the number of converts.

The Japanese were greatly impressed by the medical skill and compassion of the missionaries. Because the medical missionaries, as well as the traders, were regarded as ambassadors of good will for the Jesuits, the Portuguese crown (under Philip II of Spain and Portugal) sent a number of additional physicians to Japan. In 1568, the Shōgun confirmed his approval of the Portuguese by an astounding act: He presented to the Jesuits a temple which had formerly belonged to Buddhist monks. The Buddhist name of the temple was changed to *Nambanji*, or "Church of the Southern Barbarians"; it was to house priests as well as physicians and to serve for religious services, and as dispensary and hospital.

The ambulatory practice was used chiefly by the indigent Japanese, and most of the hospitalized patients were lepers.

Both of these groups—the indigent and the lepers—had hitherto been neglected by Japanese medicine. Early efforts to hospitalize lepers, which had been made soon after the introduction of the Buddhist faith into Japan, had ceased during the upheaval and strife of civil wars: and the lepers, cast out by their clans and villages, had been left to fend for themselves.

It was, however, not so much by these charitable undertakings as by their skill in surgery that the Portuguese physicians attained lasting fame. During the centuries of civil wars, the Japanese had accepted the need for surgery, but they had not been able to evolve a successful plan for the study of anatomy; nor had any special surgical methods been developed. The Portuguese were able to teach their art of surgery to the Japanese by means of diagrams and demonstrations. The study of internal medicine, on the other hand, required a mutual understanding of each other's language—something which was lacking almost entirely. In short, as a center of study, the church-hospital of *Nambanji* became increasingly important.

Since the herbs, which were the main components of the European medicaments, did not exist in Japan, the Portuguese physicians organized botanical expeditions to search for similar plants. Later on, they were granted a large area of land near Kyoto where, it is reported, they succeeded in cultivating three thousand medicinal trees and herbs from specimens imported from the West.

When, in 1585, the Shōgun Hideyoshi Toyotomi discovered a political conspiracy on the part of the Portuguese, with the aim of annexing Japan for their country, he proscribed the Christian religion and banned all Portuguese religious and medical missionaries.

The medicinal plants introduced by the Portuguese, however, had become part of the Japanese materia medica and remained indispensable. Nor was

Hideyoshi Toyotomi able to exile Portuguese surgery, for the *Namban-Ryu-Geka*, "The School of Surgery of the Southern Barbarians," had supplanted whatever had previously been achieved in this field in Japan. The practice of Portuguese surgery was continued by numerous Japanese students of the missionary-physicians who had established themselves in Sakai and Nagasaki and had there succeeded in attracting many followers. Two textbooks, *The Method of Wound Treatment* and *The Handbook of Surgery,*[6] were introduced by the *Namban-Ryu Geka* into Japanese medical literature.

The influence of Western medicine would have been short-lived, had it emanated exclusively from the Portuguese, whose stay in Japan was barely longer than fifty years. As a matter of fact, the Portuguese only prepared the ground for the next group of Europeans, the Dutch, who began to visit Japan early in the 17th century. Although the Portuguese resented the arrival of the Dutch as an act of trespass, the Japanese extended a hearty welcome to these new Europeans who were solely interested in trade and had no desire to win Japanese souls for a strange god and away from Japanese martial ideals.

At the turn of the 17th century, internal peace had returned to Japan. The Shogunate had fallen into the strong hands of Iyeyasu Tokugawa, the first of the Tokugawa dynasty whose uninterrupted and subsequently peaceful reign lasted for more than two hundred and fifty years—until Admiral Perry's opening of Japan. Iyeyasu Tokugawa, a great statesman, always showed a profound interest in foreigners and their beliefs and possessions. For nearly a decade it pleased him to have in his country both the Dutch and the Portuguese, whom he had permitted to return, so that he could study their political and religious hatred of each other. He used one nation to stimulate the competitive spirit in the other; and he expelled the Portuguese only when he had conclusive evidence that they strove to make political as well as religious converts.

The Dutch, like the Portuguese, had arrived in Japan by accident. In the year 1600, one of their ships bound for the Indies was driven off course and landed in Japan. The pilot-major in charge happened to be an Englishman—Will Adams by name—who was a man of great resources and superior intelligence. Adams became the Shōgun's personal protégé and taught him nautical sciences, mathematics, and trigonometry. Through Adams, the Dutch obtained generous trading privileges and established a trading post at Hirado.[7] This post was moved to the island of Deshima close to Nagasaki in the year 1641, when Japan embarked on her policy of rigid seclusion and expelled all other foreigners. Thus, for over two hundred years, the small Dutch trading settlement at Nagasaki was the only link connecting Japan with the rest of the world. In view of its small size, the amount of information that traveled east and west through this Dutch outpost is surprising. It may be more surprising to note, however, that the agents of this exchange of information were the physicians who were attached to the Dutch trade settlement.

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[6] Both these books were derived from selections of the writings of Ambroise Paré

The physicians had received with their professional training a thorough general education and were better equipped than the merchants to observe and record their findings on different social customs and natural phenomena. Moreover, in voluntarily seeking employment of this nature, they were following their inclinations to explore strange and exotic parts of the world. Among the small number of Dutch who were employed at the trading post in Nagasaki, there was rarely enough illness to keep the doctors fully occupied. Therefore, they enlarged upon their field of duty by giving medical aid to the Japanese who were connected with the trading post, and later to all the Japanese who sought their help.

The Portuguese who had attempted the study of the Japanese language only in order to preach the Gospel had never really succeeded in this endeavor. It is generally believed that they made individual converts by helping the sick and the poor and occasionally by substituting fervor for comprehensibility. Historians agree that wherever large numbers of converts were made at the same time, as for instance in Kyūshū, it was due to the orders of the Japanese barons who wished to attract trade, and not owing to the elocutionary powers of the missionaries. The Dutch merchants, however, were not anxious to learn Japanese, nor were they encouraged to do so by the authorities, who had ceased to trust Westerners after their political experiences with the Portuguese.

Instead, a number of Japanese were selected and ordered to learn the Dutch language and then appointed as interpreters. To the Japanese, these interpreters served a double purpose: they were also instructed to serve as spies and to keep their government informed about all the planning of the Westerners. These interpreters, however, appropriated another function which had not been foreseen by either the Japanese or the Dutch: The Japanese interpreters became the transmitters of Western medicine. This, in fact, occurred with such frequency that one is led to the conclusion that many of the Japanese who applied for positions as interpreters were primarily interested in the medical information which they might thereby obtain.

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[7] Ilza Veith, "Englishman or Samurai, the Story of Will Adams," *Far Eastern Quarterly*, November 1945

A part of the isolationist program of Japan was the rigid interdict upon the importation of Western books of any nature, including medical works. Moreover, all Japanese were, under the threat of capital punishment, forbidden to study Dutch. Even the interpreters were not allowed to acquire more than a slight conversational knowledge of the language. In spite of restrictions of this nature, physicians were the only foreigners in Japan who had some measure of freedom in mingling with the native population. Hence, like the Buddhist and Portuguese missionaries, the Dutch traders instructed their home government to keep the trading station supplied with good doctors, who might serve as ambassadors of good will. The physicians proved to be a useful link with the Japanese population, but they were even more valuable when they accompanied the biannual delegations which the Dutch settlement was obliged to dispatch to "Edo"—as Tokyo was then called—where the Tokugawa shoguns had established their official residence. At court, the Dutch doctors consulted with the Shōgun's personal physicians and thus formed a professional alliance transcending the restrictions of nationality.

Among the goods imported from Holland, medical supplies such as saffron, saltpeter, borax, corrosive sublimate, alum, gum, found a particularly ready market in Japan. These new medicines and the Dutch surgical methods were eagerly accepted by the Japanese medical profession and formed the core of the *Oranda-Ryū-Geka*, also known as *Rangakusha*, or the "Dutch Surgical School." It is noteworthy that despite its name, this school owed most of its fame to four men, of whom none was of Dutch nationality. At different times between 1640 and 1840, they took employment with the Dutch East India Company, mainly for the purpose of exploring and studying Japan. They were Caspar Schambergen, Engelbert Kaempfer, Carl Peter Thunberg, Philipp Franz von Siebold, and Erwin Baelz.

Very little is known of the early life of the first-named, Caspar Schambergen, beyond the fact that he arrived in Japan on a Dutch ship sometime around the year 1644. According to some Japanese sources he and the other members of the expedition were sent to Edo where Schambergen remained and gave medical instruction to the physicians of the court and the capital.

The impact of Caspar Schambergen's teachings on Japanese medicine is illustrated by the fact that there was founded in Japan the "Caspar School of Surgery," *Caspar-Ryū Geka*, which continued to teach his methods. After

Schambergen's success at court, several other Dutch physicians accompanied the delegation of the trade settlement to the capital. Of these, William ten Rhyne is outstanding, not so much for his contributions to the Japanese practice of European medicine, but rather because upon his return to Holland he tried in vain to introduce Japanese medical methods into Europe—especially acupuncture and moxibustion in the treatment of arthritis and gout.[8] In Japan, the many changes and innovations introduced by European medicine never completely displaced the ancient belief in the effectiveness of acupuncture and moxibustion.

The next Western physician to come to Japan, was Engelbert Kaempfer, another German, who served with the Dutch East India Company from 1690 to 1692; he gave graphic descriptions of these ancient methods. Chapters three and four of the appendix to his magnificent *History of Japan* deal with acupuncture and moxa predominantly and are headed as follows: "Of the cure of the COLICK by the ACUPUNCTURA or NEEDLE-PRICKING, as it is used by the Japanese." This form of colic, named *Senki,* was said to have resulted from immoderate consumption of Saké.

Yet in contrast to the Dutch physician William ten Rhyne, Kaempfer felt that the use of both these remedies, acupuncture and moxibustion, should be determined by climatic conditions and remain restricted to Japan.

Kaempfer's *History of Japan*[9] was, and still is, a standard work, not only on subjects of medico-historical interest, but also on Japan's customs and social life, art and architecture, flora, fauna, and minerals. Like Schambergen, Kaempfer was sought after as a teacher of medicine; but he was more eager to learn than he was to teach. While instructing the interpreters in medicine, he drew from them whatever information about their country they were able to give; and while conferring with the physicians in Edo, Kaempfer had an open eye and a ready pen for the court life around him.

The medical instruction given by Kaempfer and his Dutch colleagues dealt mainly with matters of surgery. Dutch internal medicine was less readily accepted by the Japanese, because the Japanese faith in the ancient Chinese textbooks on internal medicine was still unshaken.

The surgical demonstrations of Schambergen, Kaempfer, and the Dutch represented the first systematic introduction to the subject and were now taken seriously. Their instruction is reflected in a number of Japanese textbooks compiled from lecture notes taken by the interpreter-students. Towards the end of the 17th century, Japanese interest in surgical matters was so keen that several Japanese, disregarding the threat of the death penalty, attempted to translate excerpts from the works of Ambroise Paré, which had been brought to Japan in Dutch translation. The most famous of the Japanese adaptations was published in 1706 under the title of *Geka Sōden*.

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[8] Wilhelm ten Rhyne, M D , *Dissertatio De arthritide· Mantissa schematica De acupunctura et orationes tres.* (London R Chiswell, 1683)

[9] Engelbert Kaempfer, *The History of Japan 1690-92* Translated by J C. Scheuchzer (London: Printed for the Publisher by T. Woodward and Charles Davis, 1728)

Engelbert Kaempfer, who died in 1716 in Germany, was honored with a memorial in Nagasaki. There he shares a monument with Karl Peter Thunberg, a Swedish physician who served the Dutch East India Company nearly a century later. Like his predecessor, Thunberg was fascinated by Japan and made extensive observations about that country, its people and its customs—and its medical practices, especially when he too was permitted to join the Dutch East India Company's annual embassy to Edo. There, in the capital, Japanese nobles, merchants, and scientists called upon the European visitors. It was Thunberg's special pleasure to note that the general attention was concentrated upon himself, as he was considered to be the most knowledgeable in European sciences and technology. As Thunberg happened to have some surgical instruments with him, which he demonstrated, he attracted an ever-increasing number of Japanese visitors.

Thunberg was extremely well impressed by the personality, character, and appearance of the Japanese and compared them favorably with the European nations. Regarding public sanitation and personal hygiene of the 18th-century Europeans, the Japanese habit of daily bathing by all classes impressed him greatly, as did the use of disposable rice paper in the place of linen handkerchiefs. (This is as far as I know, the first mention of something resembling "Kleenex" in medico-historical literature.)

In his conversations with Japanese physicians, Thunberg became convinced that the fairly wide-spread venereal diseases had been brought to Japan by the Europeans. Because of the novelty of this illness, the Japanese had as yet not devised any treatment. Thunberg had brought some sublimate of mercury which he tried in vain to sell to the Japanese physicians as a remedy for syphilis. They refused to buy it, however, because they were afraid that the ensuing salivation might be harmful to their patients; yet they did accept small portions as presents which Thunberg gave to the Japanese physicians and their interpreters.

But even more than Thunberg's teachings, which were more or less confined to Deshima and Nagasaki, another event was responsible for the progress of medicine in Japan. In 1720, the eighth Tokugawa shōgun Tsunayoshi, also known as Yoshimune, relaxed the ban on the importation of European books, and shortly afterward when he was presented with a copy of a Dutch book he ordered that it be translated into Japanese. He designated two men, Bungo Aoki, a well-known scholar, and Genjō Noro, a court physician, to study the Dutch language. In 1745, the shōgun went so far as to permit Yoshio and Nishi, two interpreters stationed in Deshima, to read Dutch books. The physicians in Edo welcomed this change of policy, for until then they had been able to study Western medicine only from the none too accurate lecture notes taken by the Japanese students of the physicians at Deshima and from the surreptitious and faulty translation of Ambroise Paré's works.

One of the most famous physicians of this era, Riōtaku Mayeno, studied under the scholar Aoki and then took residence in Nagasaki to obtain additional instruction from the interpreters Yoshio and Nishi. Finding their vocabulary too limited for his ambitious plans, Mayeno continued his training in the Dutch language by studies of his own. In this endeavor he found a companion in his friend and colleague, Gempaku Sugita, whose interest in the Dutch language had been aroused when he came into possession of several Dutch medical books.

Of the various works which were studied by Mayeno and Sugita, the *Tafel anatomica* by Johann Adam Kulmus, made the deepest impression upon them, for its illustrations differed completely from the anatomical tables used in Sino-Japanese medical instruction. Their curiosity aroused, they asked for and obtained permission to witness a dissection at the execution grounds of the "Plain of Bones" (*Kotsugahara*) near Tokyo. This experience unsettled completely their belief in their own traditional medical knowledge. While the executioner in performing the dissection pointed out the location of the internal organs and bared them to sight, the physicians were able to compare them one by one with Kulmus's anatomical tables; however, of the descriptions contained in the Sino-Japanese textbooks they found none to be correct. Sugita and Mayeno therefore decided to make a systematic translation of Kulmus's work in order to make it available to all Japanese physicians.

In spite of all their previous efforts to study Dutch, their knowledge of the language was hardly more than rudimentary, and the translation of the complete work took fully four years. It had to be rewritten eleven times before Sugita, who had assumed the responsibility of authorship, felt that it was ready for publication. The translation finally came out in 1773 (two years before Thunberg's arrival in Nagasaki) under the title *Kaitai Shinsho*, or "New Work on Anatomy." The book was received with great interest and was soon followed by two revised editions. Later it was enlarged by Genshiu Udagawa and published in 1779 as *Ihan Teiko*, or "Outline of the Principles of Medicine." Gempaku Sugita was the author of numerous additional works, among which there were a handbook of surgery, a work on theriacs, a treatise on hygiene, and two books which characterize his self-searching and philosophical personality; they are entitled "Nocturnal Conversations with a Shadow" and "Soliloquy of an Octagenarian."

Sugita and Mayeno had labored on their task of translation without the benefit of any linguistic help, such as dictionaries or grammars. This lack was felt by one of their students, Otsuki Moshitsu, also known as Gentaku Otsuki (1757-1827), who created the first Japanese grammar of the Dutch language. With the help of this grammar and the experience gathered while working on it, he translated several Dutch books into Japanese: and, in 1789, published a completely revised edition of Sugita's translation of Adam Kulmus's anatomical tables, entitled *Kaitai Shinsho*.

Anatomical works published at the beginning of the 19th century show fewer literal translations and more original work, yet most of these were still based on Dutch precepts. The microscope had found its way into Japan and its function was discussed in 1802 in a monograph by Hoshu Katsugawara. European ophthalmology was the subject of a treatise published in 1815 by Ryukei Sugita, two years before the death of his famous father, Gempaku Sugita. Books on anatomy, containing copper plate illustrations, followed each other at short intervals. It is strange that during this period of progress the then reigning Tokugawa shōgun should order a sudden termination of the practice of Western medicine. This order was based on the presumed racial and constitutional differences between Oriental and Westerner; in the shōgun's opinion—and contrary to previous Japanese experience—these differences made it highly improbable that Western treatment and medicines should be effective in the cure of Japanese disease. The practice of Western surgery, on the other hand, had by the time of the prohibition achieved such a firm foothold that it could no longer be suppressed.

The shōgun's efforts to ban Western medicine were counteracted by the arrival of Philipp Franz von Siebold of Würzburg, Germany, the fourth of the famous non-Dutch physicians employed by the factory at Deshima. Siebold arrived in Nagasaki around 1822 and departed in 1830. His activities—or rather adventures—during these short years were unusually varied, considering the fact that all the other foreigners permitted to reside in Japan were restricted to Deshima and Nagasaki and could travel only when called to Edo on official business and with special permits.

In Europe, Siebold is known as the scientific discoverer of Japan. As mentioned above, Kaempfer and Thunberg share a simple monument inscribed only with their names and dates, on the shore of Nagasaki, while Siebold's monument in Suba Park in Nagasaki bears an inscription in Japanese which reads: "His name is immortal through his great deed of having recognized the nobility of our country and our people and of having conveyed his knowledge of it to the nations of Europe." Siebold was the third of the physicians who were the actual discoverers of Japan. After taking his M.D. degree in his native city of Würzburg in 1820, he sought an appointment in the colonial administration of Holland and was posted to the Dutch settlement of Deshima in Japan.

During the two and a half years which Siebold spent on the island of Deshima in the Bay of Nagasaki, he fulfilled the extremely sensitive task of supporting the Dutch trading post against the new British competition. For his sick calls, he received permission from the Japanese to move freely in Nagasaki and its surroundings. Soon an increasing number of students attached themselves to him. Even though he was in Japan as an employee of the Dutch, Siebold laid the foundation for the high reputation of German medicine in Japan. Among other feats, he became especially famous for the many cataract operations he performed and for his introduction of smallpox vaccination.

Through his extensive contact with patients of all strata of the population, Siebold succeeded in making and recording observations on the government, politics, and economics of Japan which would have been impossible for anyone else during this period of rigid isolation. For his medical services he received many botanical specimens as compensation—because he, as well as all foreigners, was forbidden to carry money—Dutch or Japanese.

In Deshima, Siebold founded a botanical garden with more than one thousand plants, many of which were medicinal as well as decorative plants. It was he who introduced the chrysanthemum and the peony from Japan to Europe, from where they were imported into this continent. Under great difficulties and danger he also succeeded in smuggling seeds of the teaplant into Holland.

Like Kaempfer and Thunberg before him, Siebold eventually was invited to join the Dutch delegation on its visit to the Japanese capital. The number and depth of observations made by Siebold during the two years of his stay in the capital are unequaled by anyone elses. Equally unusual was his freedom to move about and to exchange information with Japanese physicians. In December 1828, however, Siebold's work in Japan was abruptly terminated.

It seems that Siebold was accustomed to remunerate those who enriched his collections, and that this practice brought about his downfall. One of the men attached to the court procured for Siebold the chief map of the country, heretofore inaccessible even to most Japanese. Siebold's token of gratitude apparently did not meet the expectations of the official, whose complaints indirectly reached the ears of the shōgun. Aware at last of Siebold's secret activities, the shōgun had him thrown into prison and ordered him to commit suicide. Siebold, decidedly unwilling to end his life by his own hands, remained in prison for more than a year, during which time his friends and students worked for his release. This was finally achieved but under the condition of permanent banishment from Japan.

In spite of the haste of his forced departure Siebold was able to take away with him most of his art treasures and his zoological and botanical specimens, which he later presented to the museum at Leyden. Considering his active life in Japan, it is astonishing that Siebold found the time to write two books and to collect material for many more works during his residence in the country. The books written in Japan were the *De historiae naturalis in Japonica statu 1824* and the *Epitome linguae japonicae*. In 1832, only two years after his return to Leyden, he published his *Fauna japonica*, written with the assistance of the zoologists Hann, Schlegel and Temmik. His *Flora japonica*, which followed somewhat later, was written in collaboration with the botanist Zuccarini.[10]

During his stay in Japan Siebold exerted himself to have the practice of vaccination adopted by the Japanese government. But his attempts to demonstrate the operation were unsuccessful, for the lymph which had been imported from Java had lost its virulence during the long period of storage.

Vaccination was finally introduced from two sources and from two geographically distant parts. In 1848-49 the Dutch physician Monike brought lymph from Luzon and used it to vaccinate a child in Nagasaki. The resulting scab was sent to Kyoto where a physician, Hino Tōzai, used it to vaccinate

[10] See also Philipp Franz von Siebold's remarkable two-volume work, entitled *Nippon* in *Archiv zur Beschreibung von Japan* (Würzburg: Woerl, 1897, 2nd completed edition, 1897)

his grandchild and later many of his patients. Meanwhile in Yezo, in the north of Japan, vaccination had been introduced entirely without the help of the medical profession. According to Whitney's *Notes,*[11] this event occurred in the following manner: Goroji Nakagawa, a fisherman of Yezo, lost his bearing while fishing out on the high seas and landed on the coast of Siberia. This happened just at the time when the Russian Government had begun to introduce vaccination throughout the country. Nakagawa was impressed by the principle of vaccination and studied the method. When he returned to Yezo in 1824, he was adept in its application and began to vaccinate a number of persons, who escaped the ravages of the next smallpox epidemic. Thereupon his fame spread, many people came to be vaccinated by him, and finally Shōzen Sakurai, a physician who had studied in Kyoto, came to Nakagawa in order to learn from him the technique of the operation.

Vaccination was not always received in good favor. Many of the physicians of the Chinese school objected to it violently and dissuaded their families and patients from submitting to it. Such difficulties arose in Tokyo and were overcome only when one of the imperial princes took matters in his own hands. He sent one of his personal physicians to Nagasaki to study vaccination with the Dutch doctors and then ordered that his family and retainers be vaccinated. In 1858, five of the most prominent physicians of Edo, convinced of the efficacy of vaccination, formed an institute entirely devoted to this purpose.

Within one year eight more physicians joined the staff of this institute. During the first year of its existence the funds for its maintenance were collected from private contributions. In 1859, the shōgun, having become convinced of the need for medical research in general, and of vaccination in particular, instructed his government to help finance the institute. In the following year the government took over the entire financial responsibility and thus enabled the "Hall of Vaccination" to expand, and finally extend its activities to Western medicine in general. Under its new name the Institute of Western Medicine (*Sei-yō-i-gaku-sho*) confined itself to the treatment of patients and to laboratory research but within a few years medical instruction was added to its curriculum.

Quite apart from its medical importance the history of the institute was politically symbolic: its activities reflected the tremendous transformation that the country was undergoing, and the change of its name from "Hall of

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[11] Norton W Whitney, "Notes on the history of medical progress in Japan," *Trans Asiatic Society of Japan*, 1885, *12* 245-469

Vaccination" to "Institute of Western Medicine" acknowledged a great historical event: the opening of Japan to the West.

Although Commodore Perry's arrival in Japan antedated the founding of the institute by five years, it had begun its work as a purely Japanese organization; yet within a few years it assumed Western characteristics, and when in 1877, it finally developed into the medical department of the University of Tokyo, even the faculty was no longer entirely Japanese and it differed little from any other medical department of the Western world.

On his return to Holland, Siebold was received with great honors as the scientist who was most knowledgeable in Far Eastern matters. Instead of going back to his native Germany, Siebold settled in Leiden where he prepared his vast collection of scientific observations on Japan for publication.[12] The result was seven substantial books on the flora, fauna, and geography of Japan.

After Siebold had left Japan in 1830, a political movement evolved against the shogunate government for the restoration of the emperor: the result, in 1868, was that the last Tokugawa Shōgun resigned in favor of the Emperor who returned his court to Tokyo from Kyoto, where he had resided ever since the ascendency of the Tokugawa hierachy in 1600. With the restoration of the Meiji government, Japan was opened for all Western powers, and Siebold's banishment was lifted. He returned to the country he loved so much, and in 1859 was cordially welcomed by many of his old friends and supporters.

Siebold immediately resumed his naturalistic studies which in Japan had been interrupted for thirty years. He was also consulted as a physician by many Japanese patients. The xenophobia which had followed Admiral Perry's opening of Japan led to repeated attacks upon diplomats or representatives of Western countries. Only Doctor Siebold, well into his sixties, could make his sick calls without annoyance. Eventually, the Japanese government invited Siebold to serve as advisor to the court at Edo, or Tokyo. It is doubtless owing to Siebold that German medicine and science became the dominant influence in Japan, and that subsequent German physicians were warmly received in Japan.

With the opening of Japan and the treaties concluded between Japan and the various Western powers, the Dutch monopoly of foreign residence in the country was terminated. Nevertheless, for a few years afterward, Dutch physicians continued to be leaders of Western medical education in Japan. Dr. Pompe van Meerdervoort was invited to direct the newly founded foreign medical school in Nagasaki. During Meerdervoort's directorship, the first two Japanese students were selected to study in Europe.

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[12] Philipp Franz von Siebold, *op cit* (n. 10 above)

In 1870, twelve Japanese students were sent to Germany to study medicine. And in 1871 two German army physicians were invited to become professors at the medical college of the newly organized University of Tokyo. Even though during this period teachers of medicine came from various Western countries, especially from England, France, and America, the German influence remained predominant, and fluent command of German was required of every Japanese medical student. In the four years of premedical study, the curriculum contained courses in German and Latin, as well as in the natural sciences.

At the main gate to the University of Tokyo there is a bust of Dr. Erwin Bälz, another German physician who was the latest of the German doctors to receive an invitation for a full professorship in medicine at the Imperial Medical Academy in Tokyo. So favorable and honorific were the conditions of the appointment that Bälz accepted it without hesitation, and departed for Japan soon afterwards. Once arrived in his newly adopted country in the summer of 1876, Doctor Bälz labored mightily and successfully on the study and subsequent perfection of his knowledge of the Japanese language. Last year, the centennial of Dr. Bälz's arrival in Japan was celebrated in most Japanese medical schools. He married a well-born and beautiful Japanese girl, Hana Arai, and became the father of two children. His son, Toku Bälz, was the author of his father's extensive biography.[13]

Doctor Bälz not only taught at Tokyo Imperial University, but also became personal physician to the Crown Prince and many other members of the Japanese aristocracy. He further was associated with the leading Japanese scientists who were the ones chiefly responsible for the indebtedness of Western medicine to the medicine of Japan.

Most important among these Japanese medical scientists was Shibasaburo Kitasato (1853-1931) (Fig. 1), a graduate of the Imperial University of Tokyo, who was also a student of Bälz, and the most distinguished of the

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[13] Erwin Bälz, *Das Leben eines deutschen Arztes im erwachenden Japan.* Tagebücher, Briefe, Berichte. Hrsg. von Toku Bälz (Stuttgart: J. Engelhorne Nachf., 1931).

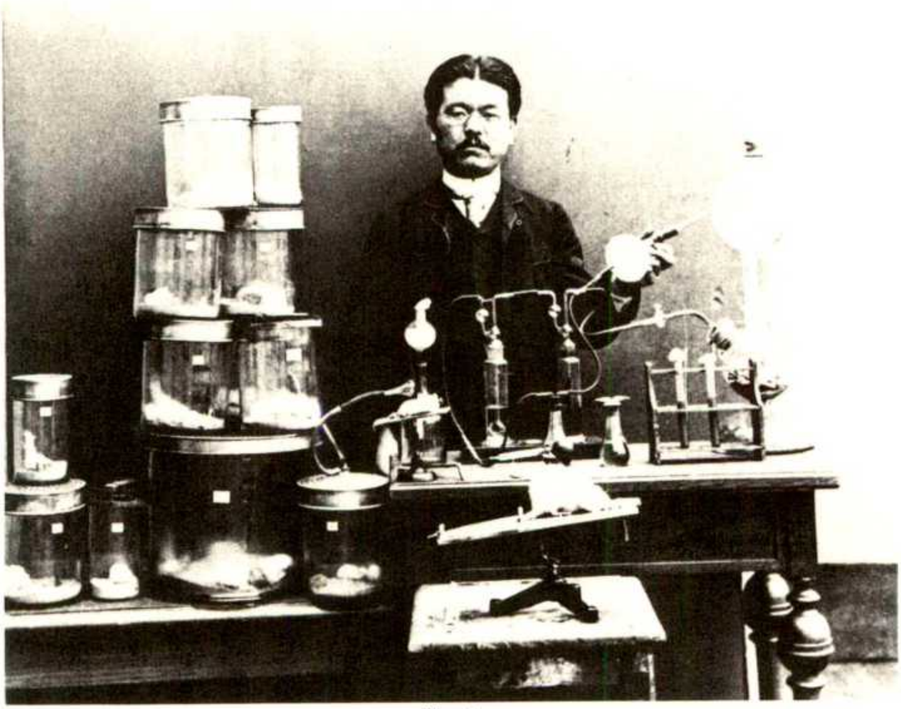

Fig. 1

Professor Shibasaburo Kitasato at the Institute for Infectious Diseases in Berlin, signed and dated by him as of March 14, 1892, which was the last year of his stay in Germany and also the year when he received the professorial title. Courtesy of Prof. Dr. Heinz Goerke, Director of the Museum for the History of Medicine at Ingolstadt, Germany.

Japanese medical scientists. After Kitasato's graduation in Tokyo, he took the customary scientific pilgrimage to Berlin and was so fortunate as to be invited to work with the world-famous bacteriologist, Professor Robert Koch, and to be given the appointment of Professor *Extraordinarius*. Kitasato is best known for his discovery of the plague bacillus in 1894, which coincided with the same discovery by the Swiss bacteriologist Alexandre Yersin. While still in Germany, Kitasato succeeded in demonstrating in pure culture the *Tetanus Bacillus* in 1889. Only one year later he shared, in collaboration with Emil von Behring, the fame of the discovery of the principle of serotherapy in their joint discovery of tetanus antitoxin in 1890.

Upon his return to Japan in 1892, Kitasato founded the first research institute for infectious diseases in that country. The institute was named after Kitasato; serum treatment was carried out there; researches on plague were conducted; and the treatment of tuberculosis patients and clinical experiments were pursued.

In 1914, without prior agreement with Kitasato, the Kitasato Institute was transferred to Tokyo Imperial University. Thereupon Kitasato resigned, because he objected to the transfer. Together with interested colleagues, he founded the privately operated Kitasato Research Institute for Infectious Diseases, where they carried out their researches and the production of serums.

Both, the government-sponsored and the privately-run Kitasato Institute, continued to exist side by side: and, in their opposition and competition, they gave strong impetus to the study of bacteriology and serology in Japan.

It is interesting that both Robert Koch and Shibasaburo Kitasato were exposed by their fellow-countrymen to similar chicanery that was born of suspicion and envy of their inspired scientific talents.[14]

In fact, so intimately was the study of, and research in serum therapy associated with Japan, that the chair of serology at the University of Tokyo was occupied until his recent retirement by Tomio Ogata, the descendant of a prominent medical hierarchy of four generations and a total of eight physicians, whose ancestor Shunsaku Ogata published in 1795 the Shuto *Hitsujunben*, which was the first Japanese book on variolation. His great-grandfather, Koan Ogata, founder of his own private medical school in Osaka, translated Hufeland's *Enchiridium medicum* into Japanese. The present Ogata, Tomio, emeritus professor of serology of the University of Tokyo, followed the traditional pattern of studying abroad, first in Germany and then in America, and is now deeply involved in the study of the history of medicine—not only of his own distinguished family, but of the evolution of Japanese medicine in general.

Long before the Kitasato Institute was transferred to the University of Tokyo, it had become a springboard for the world-wide advance in bacteriology. It was there, in Japan, that Kiyoshi Shiga, even before beginning his studies with Ehrlich in Germany, discovered the dysentery bacillus in 1898, in the excreta of Japanese dysentery patients.[15]

The discovery of this dysentery bacillus was of world-wide importance because it made possible the development of an antiserum which has saved the lives of innumerable travelers to exotic regions of the earth. Prior to Shiga's discovery, dysentery had posed a seemingly insolvable puzzle: there were various bacteriologic theories—one holding amoeba and the other holding bacteria as causative factors. The amoebic theory was upheld by Robert Koch because he had found amoeba in a series of dysentery patients in Egypt.

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[14] James R. Bartholomew, "The Acculturation of Science in Japan: Kitasato Shibasaburo and the Japanese Bacteriological Community 1885-1920," Ph.D. Dissertation, Stanford University, 1971.

[15] William Bulloch, *The History of Bacteriology*. University of London, Heath Clarke Lectures, 1936. Delivered at the London School of Tropical Medicine (London-New York: Oxford University Press, 1938).

At the time of his discovery, Shiga was working at the Kitasato Research Institute as an assistant to Kitasato. Today Shiga is remembered all over the world in the scientific name of the dysentery bacilli: *Shigella*.

Later in 1901, Shiga went to Europe. Under the direction of Paul Ehrlich, with whom he worked from 1900 to 1903, Shiga made a special study of the specific medication for Trypanosomiasis of Lethargica, indigenous to Africa. The specific medication he found in *Trypanrot*.

Another former member of the Kitasato Institute was Sahachiro Hata, without whose indefatigable assistance Professor Ehrlich might not have gone through all of his six hundred and six experiments, which resulted in the discovery of *Salvarsan* for the treatment of syphilis—popularly known as the "magic bullet." This discovery of *Salvarsan* initiated the field of chemotherapy.

We cannot leave the Kitasato Institute without mention of Hideyo Noguchi (Fig. 2), the man in whose honor this lecture is being given. It was as Kitasato's representative that Noguchi went to meet Professor Simon Flexner in a Tokyo hotel when Flexner was on the way to the Philippines. Out of this casual and superficial meeting evolved the contact which led to Noguchi's invitation to work with S. Weir Mitchell at the University of Pennsylvania—an association which began in 1900—and later to the Rockefeller Institute for Medical Research.

Throughout his scientific life Noguchi pursued the goals of the Kitasato Institute. Like Hata, he was indefatigable in his research: in 1914, as a member of the Rockefeller Institute, Noguchi was the first to obtain a pure culture of *Spirocheta Pallida* in the cerebro-spinal fluid of patients with general paresis, and subsequently he cultured other *treponemata*. He also isolated and defined the microorganisms of infantile paralysis and rabies. In 1915, he devised a method for obtaining a sterile, bacteria-free vaccine against smallpox.

In the years between 1918 and 1924, Noguchi pursued many projects. He carried out work on a curative serum for yellow fever without, however,

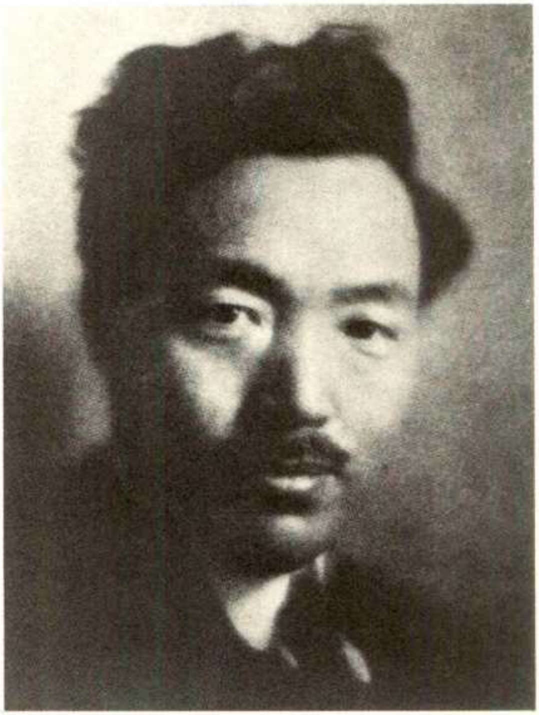

Fig. 2
Hideyo Noguchi in 1900 at the age of twenty-four in the year of his arrival in the United States. Courtesy of Dr. Gustave Eckstein from his book *Noguchi* (New York & London: Harper & Brothers, 1931).

finding it. On repeated study-trips to South America he cultivated the causative organism of Oroya Fever and *Verruga Peruana*. And, always concerned with many disease projects, Noguchi isolated the bacterium *granulosis* from trachoma in 1927.

Suffering from fatigue, ever-worsening angina pectoris, and almost completely neglected diabetes, Noguchi quickly fell victim to yellow fever in West Africa where he had gone in 1928 to study that disease. Thus, Noguchi, as so many of his contemporaries, died as a hero of science. The totality of his achievement is so manifold that, without them, bacteriology in the Western world, as in Japan, would have been immeasurably poorer.[16]

In closing, I wish to acquaint you with a little-known anecdote about

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[16] The most complete biography of Noguchi's incomparable personality was written by Gustave Eckstein, *Noguchi* (New York & London: Harper Bros., 1931).

Noguchi that is as revealing about the man as it is about his personality as a Japanese.

The man who has achieved world-wide fame as Hideyo Noguchi was born and raised as Seisaku Noguchi. As such he finished his studies—but not with the great distinction that his later career would indicate. He never achieved the all-important *igaku hakase* degree for which he was to yearn throughout his life.

When at the age of twenty-two, with all his great future still before him, Sesaku Noguchi was beginning his scientific work at the Kitasato Institute, someone called his attention to a much discussed Japanese novel, entitled *The Life of a Contemporary Student* (by Tsubouchi). In this novel, the hero with a name almost identical to Noguchi's—namely, Seisaku Nonoguchi—is also a medical student of great promise, but he comes to a bad end. Because Noguchi's affairs had not gone well in the months prior to his reading of this novel, the book had a shocking, or rather a warning, effect upon him—so alarming in fact, that Noguchi decided to mislead the evil spirits completely, so as to avoid the fate of the novel's hero, by changing his own name. Noguchi selected a name that was as different from that of the failing student as possible, namely, Hideyo, which really means *hide* = "great man," plus *yo* = "the world." Thus, this scientist, who throughout his scientific life strove to demonstrate the causative organisms of man's disease, in pure culture, began his career by bending fate through the change of his first name. Evidently this move proved to be successful.

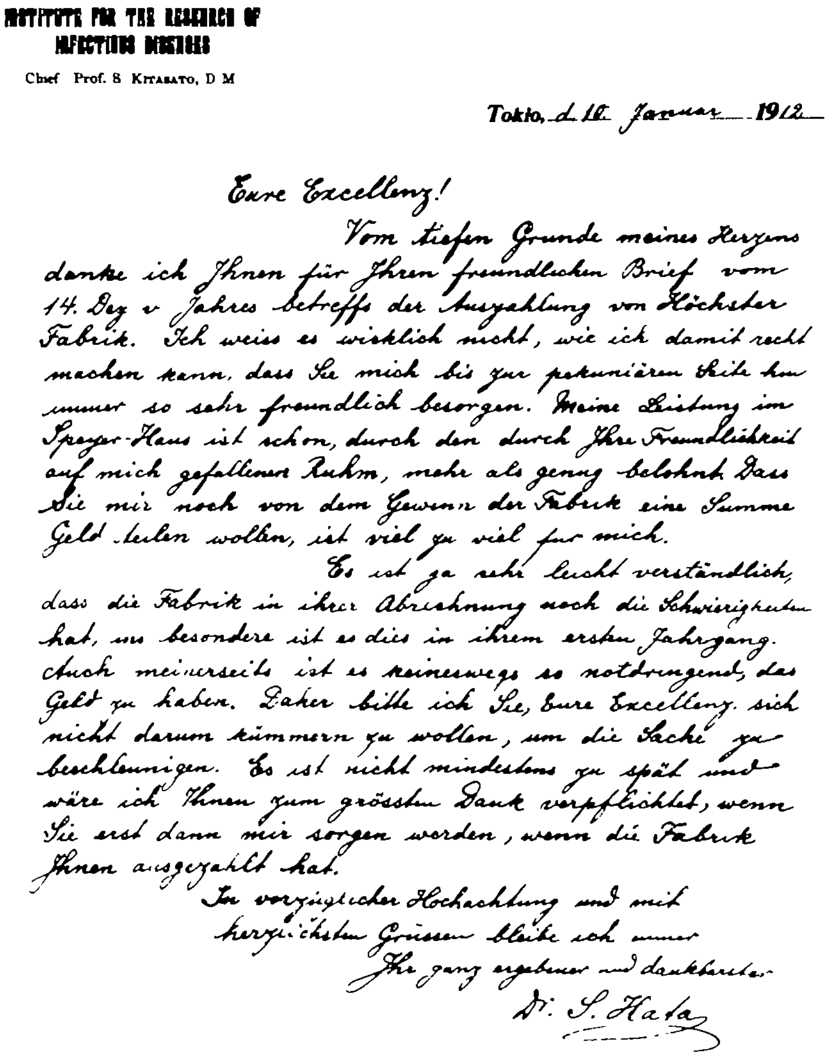
## APPENDIX I

An unpublished letter to Paul Ehrlich from S. Hata written on January 10, 1912, after his return to Japan from the Kitasato Institute for the Research of Infectious Diseases. The letter reads [author's translation]:

Your Excellency,

From the depth of my heart I thank you for your cordial letter of December 14 of last year concerning the payment by the [chemical] works of Höchst. I really don't know how I can accept it that you always look after me from the financial point of view. My accomplishments in the Speyer House [where Ehrlich's laboratories were located] are more than sufficiently rewarded with the fame that has come to me through your kindness. That you wish to share with me part of the income you derive from the Höchster Farbwerke is really too much for me to accept.

It is easily understandable that the factory has difficulty in its accounting, especially in its first year after the discovery [of Salvaran]. Also, so far as I am concerned there is



INSTITUTE FOR THE RESEARCH OF
INFECTIOUS DISEASES
Chief Prof. S Kitasato, D M

Tokio, d. 10. Januar 1912

Eure Excellenz!

Vom tiefen Grunde meines Herzens danke ich Ihnen für Ihren freundlichen Brief vom 14. Dez v. Jahres betreffs der Auszahlung von Höchster Fabrik. Ich weiss es wirklich nicht, wie ich damit recht machen kann, dass Sie mich bis zur pekuniären Hilfe hin immer so sehr freundlich besorgen. Meine Leistung im Speyer-Haus ist schon, durch den durch Ihre Freundlichkeit auf mich gefallenen Ruhm, mehr als genug belohnt. Dass Sie mir noch von dem Gewinn der Fabrik eine Summe Geld teilen wollen, ist viel zu viel fur mich.

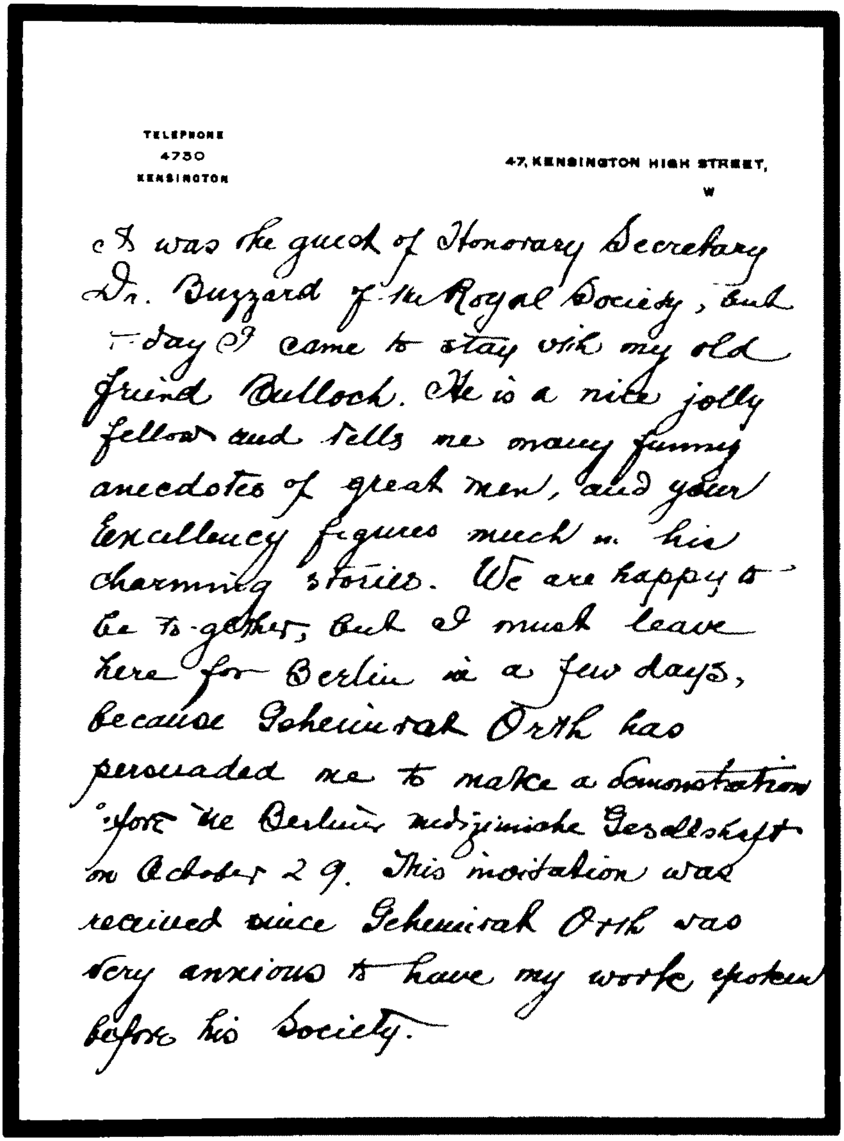
Es ist ja sehr leicht verständlich, dass die Fabrik in ihrer Abrechnung noch die Schwierigkeiten hat, ins besondere ist es dies in ihrem ersten Jahrgang. Auch meinerseits ist es keineswegs so notdringend, das Geld zu haben. Daher bitte ich Sie, Eure Excellenz, sich nicht darum kümmern zu wollen, um die Sache zu beschleunigen. Es ist nicht mindestens zu spät und wäre ich Ihnen zum grössten Dank verpflichtet, wenn Sie erst dann mir sorgen worden, wenn die Fabrik Ihnen ausgezahlt hat.

In vorzüglicher Hochachtung und mit
herzlichsten Grüssen bleibe ich immer
Ihr ganz ergebener und dankbarster
Dr. S. Hata

no necessity whatsoever for me to have the money. For this reason I ask you, your Excellency, not to worry yourself about expediting this matter It is by no means too late, and I would be most grateful to you if you began to look after me only after the factory has made its payment to you.

With deepest respect and most cordial regards, I remain, always your devoted and grateful,

Dr. S. Hata



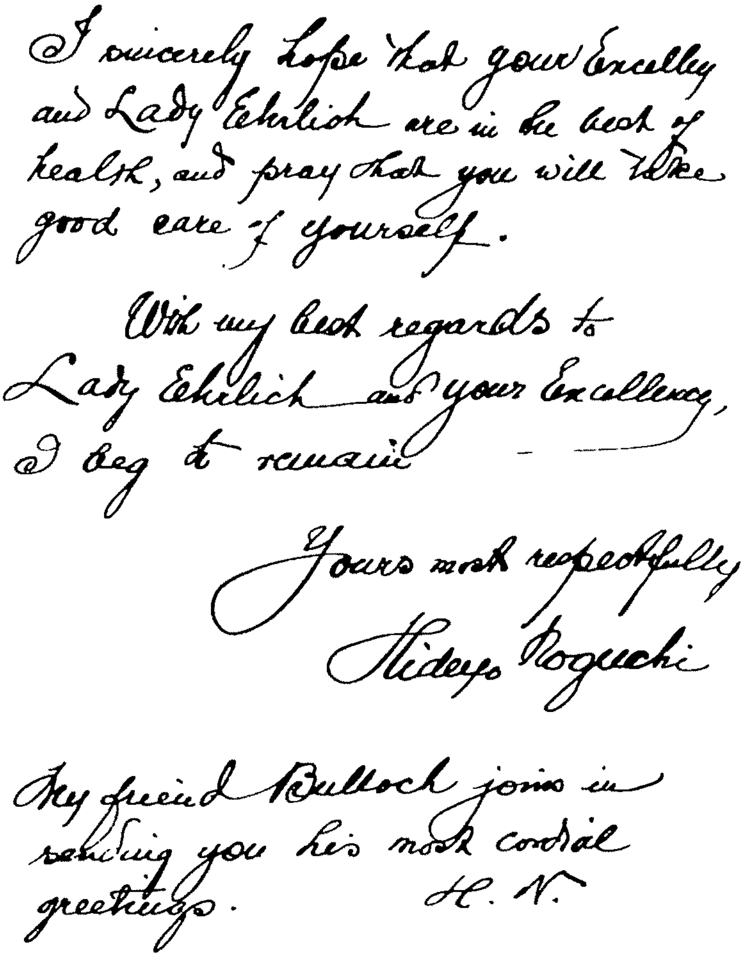
TELEPHONE
4750
KENSINGTON

47, KENSINGTON HIGH STREET,
W

I was the guest of Honorary Secretary Dr. Buzzard of the Royal Society, but to-day I came to stay with my old friend Bullock. He is a nice jolly fellow and tells me many funny anecdotes of great men, and your Excellency figures much in his charming stories. We are happy to be to-gether, but I must leave here for Berlin in a few days, because Geheimrat Orth has persuaded me to make a demonstration before the Berliner medizinische Gesellschaft on October 29. This invitation was received since Geheimrat Orth was very anxious to have my work spoken before his Society.

## APPENDIX II

A hitherto unpublished letter written by Hideyo Noguchi to Professor Paul Ehrlich. Owing to the pervading influence of German culture in Japan, Noguchi knew that Paul Ehrlich as *Wirklicher Geheimer Rat* was cus-



I sincerely hope that your Excellency and Lady Ehrlich are in the best of health, and pray that you will take good care of yourself.

With my best regards to Lady Ehrlich and your Excellency, I beg to remain —

Yours most respectfully
Hideyo Noguchi

My friend Bullock joins in sending you his most cordial greetings. H. N.

tomarily addressed as "Excellency." However, his assumption that Mrs. Ehrlich was to be addressed as "Lady Ehrlich" was erroneous and perhaps derived from his brief stay in England. Since this letter unfortunately is undated it is impossible to establish the reason for the mourning border around the stationery. It may indicate that a death had occurred in the family of his host, Professor Bulloch, or even that the letter was written in 1910, the year of the death of King Edward VII, in whose memory the black-bordered stationery may have been used throughout England. The letter reads [author's transcription]:

I was the guest of Honorary Secretary Dr. Buzzard of the Royal Society, but today I came to stay with my old friend Bulloch. He is a nice jolly fellow and tells me many funny anecdotes of great men, and your Excellency figures much in his charming stories. We are happy to be to-gether, but I must leave here for Berlin in a few days, because Geheimrat Orth has persuaded me to make a demonstration before the Berliner medizinische Gesells[c]haft on October 29. This invitation was received since Geheimrat Orth was very anxious to have my work spoken before his Society.

I sincerely hope that your Excelle[nc]y and Lady Ehrlich are in the best of health, and pray that you will take good care of yourself.

With my best regards to Lady Ehrlich and your Excellency, I beg to remain

Yours most respectfully
Hideyo Noguchi

My friend Bulloch joins in sending you his most cordial greetings.
H.N.

## APPENDIX III

A second hitherto unpublished letter from Hideyo Noguchi to Paul Ehrlich, marked in pencil in German by a former owner as "Famous Bacteriologist." The letter reads (author's translation):

Your Excellency,

Highly esteemed Mr. Privy Councillor! Please accept my thanks for your cordial letter of April 9. I am especially indebted to you for the promptness with which you arranged for my manuscript to appear in print. Moreover, I thank you for your kind letter [of recommendation?] to Court Councillor Spatz.*

With highest esteem, I remain
very respectfully yours
[signed] Hideyo Noguchi

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* Bernhard Spatz, M D pupil of Buhl and Cohnheim, became attending physician at the German Hospital in London, and since 1885 was the editor of the *Munchener Medizinische Wochenschrift*

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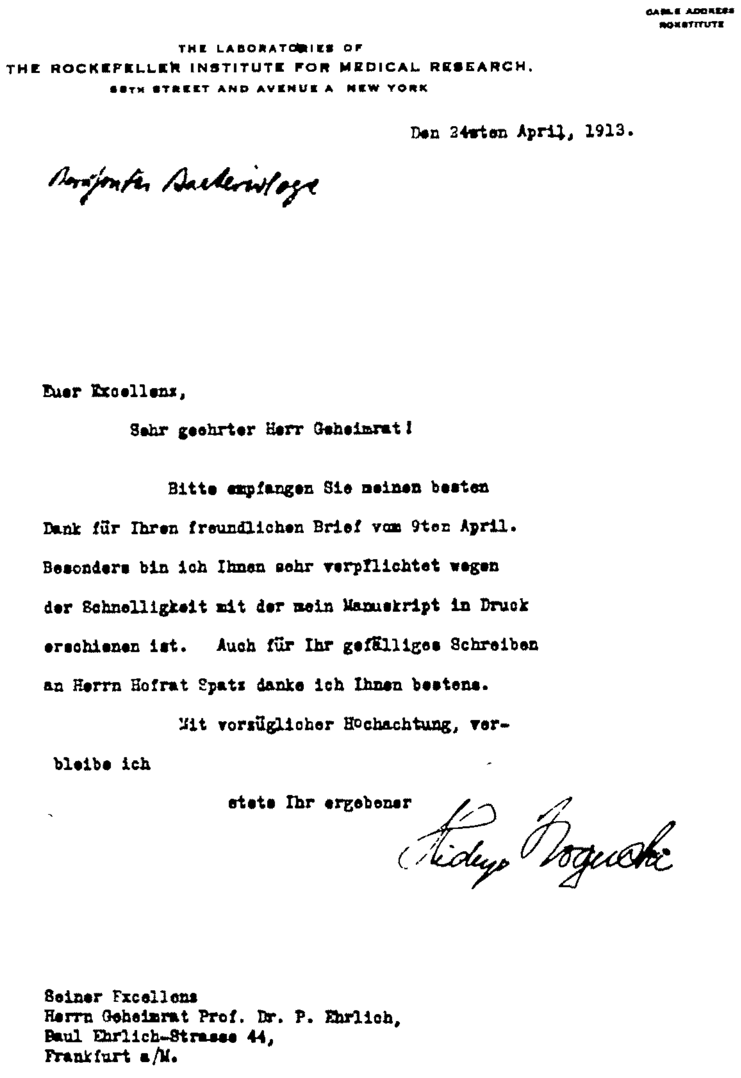

CABLE ADDRESS
ROKSTITUTE

THE LABORATORIES OF
THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH.
66TH STREET AND AVENUE A NEW YORK

Den 24sten April, 1913.

Berühmter Bakteriologe

Euer Excellenz,

Sehr geehrter Herr Geheimrat!

Bitte empfangen Sie meinen besten Dank für Ihren freundlichen Brief vom 9ten April. Besonders bin ich Ihnen sehr verpflichtet wegen der Schnelligkeit mit der mein Manuskript in Druck erschienen ist. Auch für Ihr gefälliges Schreiben an Herrn Hofrat Spatz danke ich Ihnen bestens.

Mit vorzüglicher Hochachtung, verbleibe ich

stets Ihr ergebener
Hideyo Noguchi

Seiner Excellenz
Herrn Geheimrat Prof. Dr. P. Ehrlich,
Paul Ehrlich-Strasse 44,
Frankfurt a/M.

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# A PAPAL PHYSICIAN AND THE SANITATION OF NEW YORK CITY*

SAUL JARCHO

Desiring to pay tribute to Dr. Joseph Garland, I offer in his memory a tortuous and interesting narrative which so far as I am aware has never been told in its entirety. The story ends, appropriately and happily, in the lap of American medical journalism.

The series of events begins with Giovanni Maria Lancisi (1654-1720). A bright young man, Lancisi was graduated from the Sapienza (University of Rome) at the age of 18. Three years later he gained appointment to the Hospital of the Holy Spirit, which is on the Tiber a short distance upstream from Hadrian's Tomb. As professor of anatomy he taught at the Sapienza for 13 years. In 1688 he became physician to Innocent XI, the first in a series of popes whom he was to serve. In 1707 he published his famous study of sudden death (*De Subitaneis Mortibus*), an investigation undertaken at the request of Clement XI and later expanded in his treatise *De Motu Cordis et Aneurysmatibus* (1728). The earlier of these writings was translated by Prof. A. V. Boursy and annotated by Dr. Paul D. White.[1]

Lancisi gained additional distinction in epidemiology. Indeed, both his cardiological and his epidemiological writings are regarded as classics and it would be hazardous to estimate their relative merits.

In 1711, four years after the work on sudden death, Lancisi published an essay on the natural and adventitious qualities of the Roman climate. This work compasses not only a discussion of winds and weather but also contains analyses of various local waters. The data include determinations of weight and crude chemical examinations of sediments. With this volume Lancisi published another work, an account of an *epidemia rheumatica* that raged in the year 1709.[2] The nomenclature is deceptive, since this book deals not with rheumatic fever or arthritis but with a winter outbreak of respira-

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* Joseph Garland Lecture, delivered before the Fellows of the Boston Medical Library on October 27, 1977

[1] J M Lancisi, *De Subitaneis Mortibus*, transl by P D White and A V Boursy (New York St John's University Press, 1971) 212 pp.

[2] J M. Lancisi, *Dissertatio de Nativis, deque Adventitiis Romani Coeli Qualitatibus, cui Accedit Historia Epidemiae Rheumaticae, quae per Hyemen Anni MDCCIX Grassata Est* (Rome· Gonzaga, 1711) 258 pp

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tory infection in which the symptoms began with coryza and went on to sore throat, pleurisy, and pneumonia. Lancisi judged the outbreak to be worldwide.

Lancisi next published his investigation of an epizootic among cattle, with an appendix describing an outbreak among horses.[3] This opus is an octavo, handsomely printed by Salvioni in the Sapienza—*In Archigymnasio Sapientiae*. In English translation the title reads as follows: *By Giovanni Maria Lancisi, member of the inner chamber and chief papal physician, a historical dissertation on the bovine plague imported from the district of Campania into Latium in the year of 1713, and on the measures advantageously applied by the Most Holy Father Pope Clement XI to prevent sickness of the air and costliness of produce*.

The first two major parts of the text present the history of the outbreak and the control measures enacted by ecclesiastical authority. The third and final part contains what Lancisi calls "a philosophico-medical disquisition on the individual phenomena and their signs, causes, and reasons."[4]

Lancisi found that the disease in cattle was not spontaneous but was acquired in all cases by contagion. Anatomical observations convinced him that the poison spread from the mouth, nose, and eyes to the internal organs. Death was caused not by fever but by obstruction of swallowing and of breathing. He concluded that the cause was "nothing else than corpuscles or particles, endowed with extreme thinness, nimbleness, and activity, and moved within a very short period of time, so that they are transferred or pass over from one body to another either through contact or at least through fomites by proximity." Further, he inferred that the causative corpuscles belonged "in the category of substances that irritate, corrode, and disturb the texture, movement, and temper of both the solid and the liquid parts of the animal" and, in addition, that the causative corpuscles possessed the ability to multiply.[5]

This book, obviously valuable, is rarely read and rarely quoted, probably because it has been overshadowed by Lancisi's other writings.

In 1717 Lancisi published his classic treatise on epidemiology, the *De Noxiis Paludum Effluviis* (On Harmful Emanations from Swamps).[6] In elegant Latin sentences he presented a systematic survey of the ancient and modern literature and included with it not only the information gained in recent epidemic outbreaks but also original clinical and entomological observations.

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[3] J M Lancisi, *Dissertatio Historica de Bovilla Peste Anno MDCCXIII Latio Importata Cui Accedit Consilium de Equorum Epidemia* (Rome Salvioni, 1715) 260 pp

[4] Lancisi, *De Bovilla Peste*, preface (unpaginated)

[5] Lancisi, *De Bovilla Peste*, pp 176-177

[6] J. M Lancisi, *De Noxiis Paludum Effluviis Eorumque Remediis* (Rome Salvioni, 1717) 479 pp

Since the statements in Lancisi's book on swamps are too numerous for adequate or intelligible summary, I must be content with a small selection. Lancisi points out that dry land is in general more healthful than malodorous swamps. He considers which swamps are the most dangerous. He shows that movement of waters, even under an open sky, inhibits them from becoming corrupt.

Lancisi's plan and procedure are especially impressive. From epidemiological experience supported by ancient tradition he formed the opinion that swamp fevers are caused by something that passes from swamps to men. He therefore undertook to analyze the emanations.

What is it that goes from swamps to men and causes fever? He shows that the material projected into the atmosphere by noxious swamps contains both inanimate matter and living matter.[7] He next describes the results of distilling the water of swamps. This process produced a sequence of different odors, showing that swamp water contains a variety of chemical substances—phlegmatic, sulphurous, "acrovolatile," and fetid. Both the inorganic matter and the live animalcules that come from swamps can enter crannies in the human body.[8] Swamps abound in creatures such as flies, blister-beetles, other beetles, craneflies, arachnids, prawns, and leeches. These creatures, including their immature forms, can be captured—and they can be examined, especially if the observer equips his eyes with a microscope, *si suos praesertim oculos instruat microscopio*. In paludic collections the mosquitoes are especially numerous and worthy of study.[9] If rainwater is kept in an open vessel in the summer, it becomes "polluted" (*foedari*) much faster and more abundantly with mosquitoes than with other creatures.

One of Lancisi's experiments on paludic emanations is especially attractive by virtue of its simplicity. He would take a cloth of the length of 20 palms. Part of it was covered by a waxed linen sheet and folded up; the remainder was uncovered. After exposure to a wind that was blowing from a swamp, the entire cloth was tied up and kept in the corner of a house for a week. When both halves of the cloth were unfolded, the part that had been covered contained nothing, whereas the part that had been exposed directly to the wind from a swamp was full of tiny little "worms" of various shapes, accompanied by minute ova.

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[7] Lancisi, *De Noxus*, p 43 ff.

[8] Lancisi, *De Noxus*, p 67-70

[9] Lancisi, *De Noxus*, p 60

The conclusions which Lancisi derived from this controlled experiment were as follows:

> In my opinion it is very certain evidence that, although we do not see them, the ovules of insects, together with other effluvia, are dispersed from a swamp in all directions, and especially to the region to which the wind blows. Where access is open, they promptly slip in, especially into rough, sticky, and porous bodies, and become closely entangled. Here, if the necessary moisture is present, their progeny are fostered. . . .[10]

I have elsewhere shown[11] that our concepts of infection can be traced to Anaxagoras and Lucretius and that about 30 B.C. Marcus Terentius Varro attributed febrile disease both to tiny animalcules and to noxious miasms. The same hypothesis of double etiology appears in Lancisi, and it is interesting that his American follower Cadwallader Colden, whom I shall discuss presently, omitted the animalcular component while retaining the miasmatic. It is significant also that Lancisi's evidence and his reasoning culminated in the opinion that the main practical method for the control of paludic fevers is drainage.

An additional item in Lancisi's book on swamp fevers is worthy of more than passing mention. This is his inclusion of a map,[12] which must be one of the earliest extant published contributions to medical cartography. The chart, which measures 36.8 × 18.0 cm. and is on a scale of approximately 1: 340,000, shows the terrain and the streams along the Tyrrhenian seacoast between Ostia and the Gulf of Terracina and hence includes Rome, Castel Gondolfo, and the Pontine Marshes. Superimposed near a place which, probably for valid reasons, was named Cisterna, are radiating lines which show the winds. The simultaneous depiction of terrain, drainage, and wind directions suggests what is indeed the case, viz. that Lancisi was strongly influenced by the Hippocratic treatise *Airs, Waters, Places*, a book that was the guide of thinkers and writers on epidemiology from ancient times almost up to the present era.

The summary thus far presented refers to the first third of Lancisi's treatise. The remainder of the book, not summarized in the present discussion, is a description of five outbreaks of infectious disease which occurred in Rome or in other parts of the papal domain in Latium and in Etruria between 1695 and 1709.

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[10] Lancisi, *De Noxiis*, p 60

[11] S Jarcho, "Medical and nonmedical comments on Cato and Varro, with historical observations on the concept of infection," *Trans & Stud Coll Physicians of Phila* , ser 4, 1976, *43* 372-378

[12] Reproduced in S Jarcho, "Yellow fever, cholera, and the beginnings of medical cartography," *J Hist Med.*, 1970, *25*: 131-142, see p 136 and fig 2

The epidemiological descriptions that are given in the writings of Lancisi—and this applies to other authors also—can be supplemented by the use of firsthand materials of a different and extremely interesting kind. These are the placards or notices that are customarily designated as broadsides. In 1957 and 1958 Mr. Mortimer Neinken generously donated to the New York Academy of Medicine almost two thousand broadsides which relate to outbreaks of disease in Italy between 1576 and 1849.[13] In this collection 870 items are from the seventeenth century and 1,041 are from the eighteenth. Fourteen deal with disease in Rome, and many more deal with sicknesses that affected Rome in common with other places. A special subset of 21 broadsides from Bologna, dated 1656 to 1658, describes the last great outbreak of plague in Italy. Bologna is part of what may be called Lancisian epidemiology because that magnificent city was in the Papal States and Lancisi was in effect the papal epidemiologist and the holder of what amounted to research grants from the Vatican. Papal munificence is frequently acknowledged in Lancisi's writings, and abundant evidence shows that in the case of Clement XI Lancisi's gratitude was warranted.

Many of the broadsides that have been mentioned are headed by the emblem of a pope, a cardinal, or other highly placed ecclesiastical or civil personage. The text usually starts with the announcement that one city has placed a ban or quarantine against persons or goods coming from some other city in which an outbreak of a communicable disease is in progress. Thus, Reggio established a quarantine against Mantua. Occasionally the text is a proclamation by ecclesiastical or civil officials announcing an outbreak in their own terrain and describing the measures and especially the restrictions that were to be enforced for its control. Lancisi usually included information of this kind with his epidemiological analyses, but that which is bound in a formal Latin treatise is likely to be less vivid than the vernacular proclamation that is posted on a wall.

We now turn from Rome, the Vatican, and the Pontine Marshes to the British colony of New York and to Dr. Cadwallader Colden (1688-1776), a graduate of Edinburgh (1705), Surveyor General of the province of New

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[13] S Jarcho, "A collection of Italian broadsides dealing with public health," *Acad. Bookman*, 1958, *11* 2-10 Summary in *J Hist Med*, 1958, *13* 412-413

York, member of the Governor's Council (1720-1775), and Lieutenant Governor (1761-1776).[14] Colden was a typical eighteenth-century polymath. He made an important contribution to the botany of New York; this was published by Linnaeus.[15] His *History of the Five Indian Nations*, published in 1727, was reprinted repeatedly. His contribution to physics and philosophy, *An Explication of the First Causes of Action in Matter* (1746), has been characterized as the most ambitious scientific effort ever attempted in the colonies.[16] Colden wrote also on calculus, on astronomy, on climatology, and on infectious diseases.

In 1743 Colden wrote his "Observations on the Fever which Prevailed in the City of New York in 1741 and 2." This work was published first, by journalistic methods that would have horrified Dr. Garland, as a series of untitled anonymous articles in the *New York Weekly Postboy*[17] and was reprinted in the *Pennsylvania Gazette*.[18] Such was the beginning of its vicissitudes.

Colden's essay starts with a long excerpt from Lancisi's *De Noxiis Paludum Effluviis*. He introduces it with a significant comment:[19] "I expect this may be the more acceptable because it is a rare book and perhaps not another copy of it [is] in this part of the world besides that which I have."

By way of further introduction Colden offers a description of the fevers which "recur yearly in the summer from the time the weather begins to grow hot to the end of September: they commonly cease during the winter colds: they are milder at their first appearance, but grow more and more malignant as the season advances; they are at first commonly of the intermittent kind, but more frequently (especially as the hot weather advances) the paroxisms only remit, and at the same time many have continued fevers. . . . These are the general characters of these fevers, collected from several parts of this [Lancisi's] book. . . . "

In the light of modern knowledge it is evident that from Lancisi's text

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[14] S Jarcho, "Biographical and bibliographical notes on Cadwallader Colden," *Bull Hist. Med* , 1958, *32*· 322-334, *idem*. "Cadwallader Colden as a student of infectious disease," *Bull. Hist. Med* , 1955, *29* 99-115, *idem*. "Obstacles to the progress of medicine in colonial New York The letter of Cadwallader Colden to Governor Robert Hunter (1720)," *Bull Hist Med* , 1962, *36*· 350-461

[15] C. Colden, "Plantae Coldenghamiae in Provincia Noveboracensi Sponte Crescentes," *Acta Soc Regiae Scient Upsaliensis*, 1749, *4* 81-136; 1751, *5*: 47-82

[16] B. Hindle, "Cadwallader Colden," in *Dictionary of Scientific Biography* (New York. Scribner, 1970-76), vol 3, pp. 343-345.

[17] *New York Weekly Postboy*, nos. 49-51, Dec. 26, 1743, to Jan 9, 1744

[18] *Pennsylvania Gazette*, nos 787, 789, 790, Jan. 11-Feb 22, 1744

[19] This and the succeeding excerpts have been taken from the final printed version of 1811 (see below) The earlier versions, printed in newspapers, are less correct

Colden has fashioned an introductory composite of malaria, typhoid, and paratyphoid fever. This conflated presentation of the fevers of Latium he considered generally but not completely applicable to the fevers of New York.

After his exordium Colden gives a 3,000-word summary of Lancisi's treatise, emphasizing incidents in which outbreaks of febrile disease either followed inundation and stagnation or were halted by drainage.

The authoritative dicta of Lancisi moved Colden to consider conditions nearer home. For example, "the ill effects of noxious vapours are different in different constitutions of the air, and in different climates." For this reason, he says, the same kind of spirits cannot be produced from molasses in North America that is produced from it in the West Indies. Further, Lancisi's observations as to the relation between terrain and fevers had been confirmed by recent occurrences at Bristol, Pennsylvania, and along the Paltz River in Ulster County, New York.

An even more important corollary of Lancisi's contentions had to do with the City of New York:

> 1st. It is well known that part of the town chiefly afflicted with the epidemical distemper these two last summers [1741-42] is built upon a swamp or moist slimy ground; that it is flat and the waters not easily drained from thence . . . and that the moisture of these grounds is to be observed in every cellar of the houses built on them. 2dly. No person that walks along the docks, but is sensible of the filthy smell there . . . 4thly. That these parts of the town have always been subject to epidemical disorders, every summer toward autumn. . . .

Colden concluded that the drains needed improving and that their development and maintenance should be transferred from private subscribers to the municipality, and should be supported by a public tax. Drainage, he added, was important even when infection was introduced from abroad, since "it is well known that these infections are like a leaven, which will lie dead in cold weather; but as soon as the weather becomes hot, they ferment anew, and propagate and spread . . . ."

That Colden's essay impressed at least a few citizens is evident from a letter addressed to him on January 22, 1744, by a New York alderman named James Alexander, who wrote that "the paper I believe & hope has had the Effect desired to witt to Convince a Majority of our Magistrates of the necessity of removing Skinners Tanners &c to fresh water & of Either cleaning or filling up the Slips before Summer. . . . "[20]

The parts of New York City that were the object of Colden's admonitions were the so-called Fly Market and Fly Slip situated close to the East River, near the end of Maiden Lane. The market was erected in 1706 and demolished in 1821.[21]

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[20] *The Letters and Papers of Cadwallader Colden*, vol. 3, 1743-1747 (New York. New-York Historical Society, 1919), pp. 45-47 See also a letter from Colden to Johnson [Dec. 1744], *ibid.*, pp. 95-96.

This district is mentioned very frequently in the *Minutes of the Common Council of the City of New York.*[22] The corporation of New York had been indicted by a grand jury because of "Sundry Nusances." The insanitary Fly Slip was formally declared to be a nuisance and on May 3, 1744, a law was passed for its correction. The text reads in part:

> . . during the Latter part of the two Last Summers a Most Noisom Smell was Generally Perceived by all who went into the South East part of this City: In Which part thereof a Most Mortall and Infectious Distemper then there raged while other parts of this City Retained Perfect health and Which Mortall and Infectious Distemper in all human Probability Arose and was Occasioned by the Noisom Smell aforesaid.[23]

In his *Documentary History of the State of New York,* E. B. O'Callaghan says that the corporation of New York voted Dr. Colden their thanks for his treatise on the fevers of New York.[24] Another historian, Martha Lamb, says that Colden recommended "sanitary measures which were so prolific in results that the Common Council of New York tendered him a vote of thanks."[25] I hope that these stories are correct but I have never been able to find documentary evidence which would support them.

Let us now resume the story of Colden's manuscript. The fevers which he discussed were those that afflicted New York in 1741 and 1742. The newspapers carried his essay in instalments in 1743 and 1744. At some later time Colden revised the text but did not publish the revised version. He died "in the 89th year of his age on the memorable 28th of September, 1776, a few hours before the city of New-York was in flames. . . ."[26]

In 1810 Drs. David Hosack and John W. Francis of New York founded *The American Medical and Philosphical Register: or Annals of Medicine, Natural History, Agriculture, and the Arts.* The first volume is dedicated to

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[21] I N. P Stokes, *The Iconography of Manhattan Island, 1498-1909* (New York Dodd, 1915-1928), vol 3, p 958

[22] *Minutes of the Common Council of the City of New York, 1675-1776* (New York: Dodd, Mead, 1905), 8 vols , see esp vol 5, pp 111-114, 118-121, 145-146, 162-229, 266-267, 343-344 See also J Duffy, *A History of Public Health in New York City, 1675-1866* (New York: Russell Sage Foundation, 1968), pp 42-47

[23] Minutes of the Common Council (see n. 22 above), vol 5, p. 118

[24] E. B. O'Callaghan, *The Documentary History of the State of New-York* (Albany, Weed, Parsons, 1849-51), vol 3, p 832

[25] M. J Lamb, *History of the City of New York* (New York Barnes, 1877), vol. 1, p 589

[26] Anonymous, "Biographical sketch of the late Honorable Cadwallader Colden," *Am Med & Phil Register*, 1811, *1*: 297-303, see p 299

De Witt Clinton. Its third number includes an anonymous biography of Colden,[27] which tells us that the "numerous manuscript papers left by Dr. Colden at the time of his death, and which for many years were supposed to have been lost, have lately been found, and are now in the possession of his grandson, Cadwallader D. Colden, Esq. attorney-general for the southern district of this state. . . . Among these are . . . A corrected copy of his Account of the Fever which prevailed in New-York, in the years 1741-2. . . ." The same issue of the periodical contains a paper by Colden on the climate and diseases of New York and also the essay that we have been discussing. Most of Colden's papers are now in the collections of the New-York Historical Society.

* * * *

The story of the Pontine marshes and the Fly Market of New York has many implications. The first of these has already been mentioned, viz. that in the eighteenth century the different febrile diseases were scarcely separable, despite complex traditions inherited from antiquity. Colden speaks of intermittents giving way each summer to continued fevers. This probably means that early vernal malaria was followed by estivo-autumnal malaria, typhoid, paratyphoid, and even yellow fever. That some of the so-called yellow fever was really infectious hepatitis has been established by study of temporal incidence.[28]

In the realm of sanitary practice there is the greatest possible contrast between measures decreed by a pope and measures debated by New York aldermen.

Another problem has to do with the transmission of European medicine to North America. It is apparent that English and Scottish medicine reached the transatlantic domain by travel of Americans to Europe and by entry of European people and books to America, whereas Italian medicine reached us almost entirely by books.

The final consideration takes us back to the realm of Dr. Garland. In colonial times the organ of medical journalism was the newspaper. This is why Colden's essay made its debut in the *New-York Weekly Postboy*. Not until later were special medical periodicals established. That the work of the journalistic founders has been continued by able and devoted successors and has been carried to a high literary and scientific level, is axiomatic with all those who remember and honor the name of Dr. Joseph Garland.

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[27] Anonymous, "Biographical Sketch of the Late Cadwallader Colden "

[28] S Jarcho, "John Mitchell, Benjamin Rush, and Yellow Fever," *Bull Hist Med.*, 1957, *31* 132-136

# RICHARD BRIGHT AND MERCURY AS THE CAUSE AND CURE OF NEPHRITIS*

STEVEN J. PEITZMAN

Ignored despite centuries of exalted uroscopy, the kidneys were, pathologically, orphans—assigned no important disease—until 1827. In a series of publications beginning in that year, Richard Bright (1789-1858) described all important components of the complex uremic syndrome, and correctly associated them with diseased kidneys.[1] For this and other advances historians credit the Guy's Hospital physician with exceptional powers of observation and deduction, and consider him the pre-eminent early British exponent of clinical-pathologic correlation.

But Bright did not devote all his time to the autopsy rooms. Having made a novel diagnosis in patients under his care, he wished to prescribe effective treatment. Not an innovator in therapeutics, he could choose from available agents: an unlimited menu of purgatives; the bleeding lancet; diuretics and diaphoretics; opium; and, the always controversial mercury. Like many other cautious practitioners throughout more than four centuries of mercurial use, Bright expressed special reservations about these drugs and over two decades carefully modified his application of them in patients with renal disease. Examining Richard Bright's selection of therapy for an essentially "new" disease illustrates the etiologic and therapeutic thought of his day, and introduces a forgotten controversy in the history of mercurial excess.

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[1] Bright's major publications on renal disease are "Cases illustrative of some of the appearances observable on the examination of diseases terminating in dropsical effusion" (1827) in his *Reports of Medical Cases Selected with a View of Illustrating the Symptoms and Cure of Diseases by a Reference to Morbid Anatomy* (2 vols.; London: Longmans, 1827-1831); "Cases and observations illustrative of renal disease accompanied with secretion of albuminous urine," *Guy's Hospital Reports*, 1836, *1*: 338-379, and "Tabular view of the morbid appearances in 100 cases connected with albuminous urine," *Guy's Hospital Reports*, 1836, *1*: 380-400. These are reprinted in *Original Papers of Richard Bright on Renal Disease*, ed. A. Arnold Osman (London: Oxford University Press, 1937). Page numbers in subsequent footnotes refer to this edition.

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Although the nineteenth-century clinical pathologists had little enthusiasm for the previous century's formal "systems," as practical physicians they could not choose treatment in a theoretical vacuum. Richard Bright rationally derived his treatment of albuminuric nephritis from the conventional etiologic framework into which he placed the disease. This framework was based on concepts already weary with age in 1827: "suppressed perspiration," the "sympathy" between organs, and insufficient or excess "action" of organs. Some brief review of these ideas may allow easier appreciation of Bright's thinking.

Physicians have long looked upon the skin as far more than just a comfortable and well-fitting wrap. That it can actively "breathe" and transmit vapors through the opening of tiny pores has been continuously believed, with varying conviction, since the time of Galen. The unhindered exit through the skin of bodily effluvium—the "insensible perspiration"—was held necessary for health;[2] obstruction to this flow (suppressed perspiration) caused congestion and disease in other organs of flux, such as the bowel, stomach, or kidneys. This remote disturbance occurred because between these organs and the skin there existed "sympathy" or "consent," a kind of functional interdependence by which an imbalance in the activity of the one could derange another. Particularly when the efflux of an organ was obstructed (as in suppressed perspiration or catamenia), through the material "metastasizing" elsewhere, considerable mischief might arise. Bright tried to understand nephritis in terms of this set of beliefs:

> In the treatment of this formidable disease . . I look to the circumstances with which it is most frequently connected—I mean the suppression of the perspiration, and the consequent general inflammatory conditions. . [3]

and again:

> An intemperate course of life, or some other cause, has predisposed the Kidneys to suffer. The patient has, in this state, been exposed to vicissitudes of temperature; the irritable kidney has immediately sympathized with the skin, and morbid action has been induced . . .[4]

Exposure to cold air was popularly believed to be the commonest cause of suppression of the perspiration. Bright suggested forcefully in several papers that intemperance also predisposed to albuminuric dropsy, but it is not clear whether he saw the use of alcohol as merely favoring exposure to chill, or irritating the kidneys either directly or via the stomach. His focus on intemperance may have reflected a bias: he studied mostly ward patients, generally of the lower classes, and might equally well have linked drinking with any other disease frequently observed in the Guy's population.

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[2] See E. T. Renbourn, "The natural history of insensible perspiration: a forgotten doctrine of health and disease," *Med. Hist.*, 1960, *4* 135-152

[3] Bright, *Guy's Hospital Reports* (n. 1 above), p 122

[4] Richard Bright, "On the functions of the abdomen and diagnostic marks of its disease" (Gulstonian Lecture, 1833), *London Med Gaz.*, 1833, *11*. 380.

What was the nature of the organ disturbance resulting from "sympathy" with the chilled and suppressed skin? Although the formal dogma of Brunonian theory may have lost by the 1820s much of whatever British allegiance it could claim, and William Cullen's concept of tissue "irritability" was nearly archaic, the legacy of these doctrines and their precursory ideas persisted. Moreover, the system of Broussais, partly reflecting these earlier formulations, gained wide acceptance by the 1830s and further advanced organ "irritation" as the current justification to exsanguinate the sick. In the daily business of treating patients' ills, physicians in 1827 still decided first whether a sick organ was operating above or below its usual level of energy. Was there, on the one hand, "excessive action," inflammation, irritation—or, on the other, "defective action," torpidity? Making such decisions directed therapy, since most agents of the materia medica could be categorized by their stimulating or quieting ("depletive," "antiphlogistic") ability. Having concluded the usual cause of nephritis to be inflammation induced by "suppressed perspiration," Richard Bright attempted to treat the disease by applying these conventional guidelines, as one may term them—for they recalled, but no longer constituted a complete "system."

First, if the suppression of insensible perspiration could be relieved early in the course of the disease, the kidneys should regain normal function. Bright urged to this end the avoidance of further chill:

> On the subject of clothing I have already said all that is neccessary: let flannel be worn constantly, and every precaution be habitually adopted which may obviate the effects of whatever is calculated to chill the surface or check the perspiration.[5]

And, although recognizing that "the disease being, unfortunately, most apt to occur in those least able to submit to the absence from business," he further advised emigration to "some decidedly southern abode. . . .

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[5] Bright, *Guy's Hospital Reports* (n 1 above), p 131

One of the more healthy of the West-India islands, as St. Vincent's, would probably be beneficial."[6] After attention to the underlying suppressed flux of the skin, symptomatic therapy was aimed at the kidneys and serosal (pericardial and pleural) membranes. Bright interpreted the albuminuria and occasional hematuria observed in his patients to mean that the kidneys were over-active and inflamed. The pericarditis and pleuritis also reflected irritation induced by "sympathy." To reduce this excess stimulation, "active depletion" was required, and this inevitably meant bleeding and purging. Although Richard Bright discussed bleeding in little detail, in his 1836 summary of the treatment of albuminuric dropsy[7] it is the first mode of management he suggested, at least for the early periods of the disease. He later forbade over-zealous bleeding, intelligently noting that the patients were often already pale and wasting "red particles" and serum into the urine. Purgatives, however, Bright gave without hesitation, particularly to the patients described in the 1827 *Reports of Medical Cases*. Powerful vegetable laxatives were available in an astonishing variety in the nineteenth century, and in their use Bright typified the practice of polypharmacy, trying many drugs serially or combined. To the patient described as case XIII in the *Reports of Medical Cases* he gave within a few days' time magnesia, linin, jalap, capsicum, calomel and colocynth, all more or less archaic cathartics. Though not offered to that unfortunate individual, elaterium—a powerful purgative derived from a variety of gourd—was also a favorite remedy.[8] Other alternatives were "the combination of Jalap, Supertartrate of Potash, and a little ginger repeated from time to time . . . [or] . . . frequent doses of Castor Oil."[9] Diuretics and diaphoretics Bright prescribed only occasionally and with little enthusiasm, and—despite his recognition of the inflammatory nature of uremia—he only infrequently gave opium.

This course of therapy so far outlined is predictable and undistinguished: any knowledgeable early nineteenth-century physician would on his own likely have treated Bright's Disease very much as did Dr. Bright.[10] Disagreement would have arisen, however, over the use of mercurials. Despite protesting pamphlets of the anti-mercurialists, in the early nineteenth century, mercury use in England continued to increase—only opium and antimony could compete with the metal for popularity.[11] Accepted for centuries as treatment for syphilis, mercury—especially in the form of calomel—was urged in the eighteenth century for liver ailments, yellow fever, and, finally, for almost any malady. An important authority on materia medica, Jonathan Pereira, advised in 1837 that "mercury may be employed in every disease whose name ends in itis," though he allowed that "it is not equally serviceable in all."[12] The suffix "-itis" denotes inflammation, and Pereira's aphorism expressed the prevalent confidence in the anti-inflammatory or "antiphlogistic" action of mercury, although calomel was also variously looked upon as primarily a sialogogue, stimulant, sedative, diaphoretic or diuretic.[13] Observing severe pericarditis and pleuritis, as well as hematuria, in nephritic patients, Richard Bright prescribed mercury as an antiphlogistic for some of the earlier patients. He reserved it, however, for those individuals whose symptoms seemed refractory to other remedies. Of the twenty-three patients reported in the initial 1827 *Reports of Medical Cases*, Bright gave mercury to nine. Ten others had already received the mineral before hospitalization at Guy's, or from another physician at the hospital (not all subjects described by Bright were under his direct charge). In reviewing his experience with mercury through 1827, he admitted that "it is consistent with the most successful treatment of many forms of inflammatory diseases, that we should have recourse to the valuable combination of Calomel with Opium . . ."—a therapeutic lemma of the day. He concluded, however, that

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[6] *Ibid*, p 125

[7] *Ibid*, p 123

[8] Bright, *Reports of Medical Cases* (n 1 above), p 24

[9] *Ibid.*, p 74.

[10] Because no term existed for the newly recognized chronic renal failure, "Bright's Disease" and "Morbus Brightii" came into widespread use immediately after the Guy's practitioner's publications appeared

> the cases which have proved most successful in my own practice, have generally been those in which I have rigidly abstained from the use of mercury. In some cases I have seen the good effects of other remedies entirely interrupted by the mercurial action; and I have likewise seen several instances in which the cure, when mercurials have formed part of the plan, has been protracted to a great length . . . .[14]

He also noted the rapidity of onset and viciousness with which mercurial salivation occurred in the renal patients given mercury (mercury salts are excreted almost entirely by the kidneys and, therefore, accumulate in persons with renal disease).

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[11] See J. S Haller, Jr , "Samson of the materia medica. medical theory and the use and abuse of calomel in nineteenth century America," *Pharmacy in History*, 1971, *13*· 27-34 and 67-76

[12] J Pereira, "Lecture on materia medica," *London Med Gaz.*, 1837, *18* 419

[13] Haller (n. 11 above), p 32

[14] Bright, *Reports of Medical Cases* (n 1 above), p 75.

In the next publication, the 1836 "Cases and observations illustrative of renal disease," ten patients were detailed; Bright now reported giving mercury to only two of these. Furthermore, he devoted several pages to "the great question, how far the existence of albuminous urine contra-indicates the use of mercury," and his statement is stronger than in the earlier writings:

> . . . I am strongly inclined to believe, that whatever may be the exceptions under the most cautious management, the use of mercury had better, as a general rule, be avoided. When I first published upon this subject, I stated that I had seen much decided mischief from the use of this remedy; yet that I had undoubtedly seen well-marked cases, in which the free use of mercury, even to complete ptyalism, had at least not prevented the patients from deriving great, perhaps even perfect relief from the remedies with which it was combined: and, on a later occasion, I have observed, that I have seen the combination of mercury, squill, and digitalis, give present relief and produce apparent cure; yet I am bound to say, that these cases are rare, while the instances of failure and obvious injury from this remedy are numerous.[15]

But still unwilling to fully reject mercury, he conceded later that "in the very early stages of acute renal disease," the addition of mercury to opium, antimony, and bleeding may yield benefit.[16] In 1840 Bright contributed to the *Guy's Hospital Reports* the little-known "Cases and observations illustrative of renal disease accompanied with the secretion of albuminous urine: memoir the second," stating as its purpose to "select a few cases which may assist us in forming a fair estimate of the degree of success which we may reasonably expect to attain in the treatment of albuminous urine."[17]

Of the twenty-four mostly non-fatal cases reviewed, Bright (or other Guy's physicians) gave mercury to only four. In his concluding statement of the recommended therapeutic program—he again urged bleeding, purging, warm bed and bath—the use of mercury is not mentioned. In fact, a gradual disinclination to give mercury was Bright's only major change in remedy during more than twenty years of treating nephritis. Having observed over this time that the display of the agent in this malady seemed to yield violent salivation but little benefit, he sided with the antimercurialists, at least in relation to this important "new" disease which soon bore his name. He did not, however, go so far as to endorse the remarkable assertion of several of his contemporaries—that mercury itself, far short of being a cure, was indeed the *cause* of dropsy with albuminous urine.

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[15] Bright, *Guy's Hospital Reports* (n 1 above), p. 127-128

[16] *Ibid* , p. 129

[17] R Bright, "Cases and observations illustrative of renal disease accompanied with the secretion of albuminous urine: memoir the second," *Guy's Hospital Reports*, 1840, *5*. 101-161.

Although the writings of the antimercurialists abounded with accusations that mercury compounds could cause nearly any dread disease or symptom, the possible contribution of these drugs to renal disease in the early nineteenth century deserves consideration: mercury salts are now recognized to be potent and frequent nephrotoxins, and renal damage from calomel has been documented even in recent medical literature.[18] Richard Bright read and cited in his first works the important antecedent publications of William Charles Wells[19] and John Blackall,[20] who independently studied albuminous urine and concluded that mercury was the major cause of this phenomenon. In 1812 the American-born Wells reported his examinations with heat or nitrous acid precipitation of urine samples of 130 patients at St. Thomas Hospital by which he detected "serum" (i.e., albumin) in 78. He was struck by the frequency of this finding and particularly by its apparent association with mercury: "Having found serum in the urine of three out of four patients, who were in a salivation excited by mercury, for the cure of the venereal disease, I thought it unlikely that its presence was either accidental in all of them, or occasioned by that disorder."[21] He then prospectively tested urine specimens of six patients with syphilis before and after they began to take mercury, and found albuminous urine in four, but only *after* they started the drug course. The experiments were novel, but probably did not justify Wells's startling conclusion "that urine containing a considerable quantity of serum must occur very rarely, if at all, in any disease in this country, except dropsy, and that induced by mercury."[22]

In his *Observations on the Nature and Cure of Dropsies* (1814), the

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[18] Examples are V K Wilson *et al* , "Mercury nephrosis in young children," *Brit Med J* , 1952, *1*. 358-360, and H C Gonick, "The adult Fanconi Syndrome," *Medicine (Baltimore)*, 1972, *51*. 107-138

[19] W C Wells, "On the presence of the red matter and serum of blood in the urine of dropsy, which has not originated from scarlet fever," *Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge*, 1812, *3* 194-240

[20] J. Blackall, *Observations on the Nature and Cure of Dropsy* (2d. ed, London Longman, Hurst, Rees, Orme and Brown, 1814)

[21] Wells, *Transactions of a Society* (n. 19 above), p. 229

[22] *Ibid.*, p 231

Exeter physician John Blackall anticipated the work of Bright even more closely than did Wells. Blackall also noted the frequent association of dropsy and albuminuria, but additionally detected pericarditis and pleuritis in some of his patients, as Bright would do later. He also demonstrated at autopsy diseased kidneys in four of the albuminuric subjects. Like Wells, Blackall emphasized the association of mercury use and "dropsy with coagulable urine," and believed that excepting scarlatina, the drug was the most common cause of this finding. His final arguments against its use were then, that:

> Mercury itself is calculated to produce this discharge [i.e., albumin in the urine] . . . Under the use of the sublimate of mercury a bloody sediment has appeared in addition to the presence of serum . . . [and] . . . it often rapidly affects the gums and with great distress . . . .[23]

As described earlier, Richard Bright eventually seconded the advice of Wells and Blackall against mercury use in albuminuric dropsy, and by the 1840s several standard textbooks of medical practice[24] and an important early treatise on poisons[25] echoed this proscription. Nephritis had quickly become a disease for which established medical opinion specifically advised against the use of mercury. Furthermore, several reputable physicians, though not Bright, believed that they had identified mercury as the *source* of renal dropsy.

Whether early nineteenth-century British physicians chose to accept the proposal that mercury ingestion induced kidney disease, or—more importantly—knew how to conclusively test it, has little to do with our current unquestioned awareness of mercurial nephrotoxicity. Nevertheless, at least one writer has suggested that nephritis was unexpectedly common in Bright's day,[26] and it is an intriguing possibility that something in addition to the clinico-pathologic tone of that time explains the rather sudden identification of numerous patients with renal disease. To estimate disease prevalence is always difficult, and essentially impossible retrospectively, considering a then newly-recognized illness of 150 years ago. Bright was able to tabulate 100 cases of renal disease, proved with autopsy, between 1825 and 1836, and one expects that for each of these, at least several others would have suffered the illness, but with less unhappy outcome. Though this would seem a large number of cases, Bright would naturally attract patients with dropsy once his interest in this symptom was known, while Guy's Hospital with its more than 500 beds provided ample unselected clinical material.[27] But even if one cannot prove that fatal nephritis was unexpectedly common in the early nineteenth century, at least the reported frequency of proteinuria is startling. Wells had found "serum" (i.e., protein) in 78 of 130 ward patients' urine samples tested at St. Thomas Hospital by the application of heat or nitrous acid.[28] Bright, in 1828-1829, "by taking the patients promiscuously, as they lay in the wards of Guy's, and trying the effects of heat upon the urine of each,"[29] found "decidedly coagulable" specimens in 18 of 130, plus "traces of albumin" in twelve more. Bright's pupil John Anderson in 1835 tried a similar experiment on 141 patients and detected proteinuria in eighteen;[30] he refers also in his article in the *London Medical Gazette* to another trial by George H. Barlow (a Guy's physician) of 296 patients' urine samples of which twenty-six were coagulable. The application of the spoon and candle (heat coagulation) is a reasonably accurate—though not exquisitely sensitive—means of displaying albuminuria;[31] it appears, therefore, that at least ten percent of random ward patients in London of the 1820s and 1830s wasted substantial protein into their urine. We do not encounter this abnormality nearly as frequently today.

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[23] Blackall, *Observations* (n 20 above), p. 335

[24] Thomas Watson in his popular *Lectures on the Principles and Practice of Physic* (American ed.; Philadelphia: Lea and Blanchard, 1844) stated, "It is an unsettled question whether mercury be advisable, or even admissible [in renal dropsy]. . . . The current of medical opinion sets against it, perhaps too strongly" (p. 795) A more specific warning is that in David Craigie's *Elements of the Practice of Physic* (Edinburgh. Adam and Charles Black, 1837-1840): "But the great objection to the employment of any preparation of quicksilver for the cure of renal dropsy, consists in the fact, that the use of the mineral is known to render the urine albuminous . . and to induce the disease, the effect of which it is expected to remove" (p 1148)

[25] A. S. Taylor, *On Poisons* (London· John Churchill, 1848) Taylor reviewed the publications of the other authors discussed here, and concluded that mercury use in renal disease is "improper " He also cited the trial of an unlicensed practitioner accused of causing death by giving calomel to a renal patient Conviction might have added legal weight to the opinion against mercury in kidney disease, but the crucial issue was avoided the patient was shown to have mistakenly taken twice the recommended dose, and the practitioner won acquittal.

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[26] H. A. Christian, "Kidney disease as described by Richard Bright in the light of the knowledge of a century later," *Ann. Med. Hist.*, 1927, Ser. I, *9*: 337-346.

[27] H. C. Cameron, *Mr. Guy's Hospital* (London: Longmans, Green, 1954), p. 115.

[28] Wells, *Transactions of a Society* (n. 19 above), p. 229.

[29] Bright, *Guy's Hospital Reports* (n. 1 above), p. 120.

[30] J. Anderson, "Observations on renal dropsy," *London Med. Gaz.*, 1835, *15*, 855-866.

[31] The present author briefly revived the spoon and candle test, to the amusement of his colleagues, in the renal clinic of the Veterans Administration Hospital, Philadelphia. In comparison to the modern "dip-stick" for albuminuria, the older technique was less sensitive but reasonably specific—that is, no urine specimen free of albumin by "dip-stick" showed a false coagulation upon heating.

Pervasive mercury use provides an attractive explanation for this seemingly epidemic proteinuria. Goldwater[32] and Haller[33] have extensively described the ascent of mercurial medicines, in particular calomel, through the eighteenth and early nineteenth centuries. Its indications extended over a century by British physicians such as Dover, Curry, and Robert Hamilton, quicksilver in its many preparations was a prominent panacea by 1800. Whereas the Royal College of Physicians *Pharmacopoeia* of 1682 included six mercurial preparations, by 1763 eight were found, and the 1836 edition included thirteen.[34] The Edinburgh *Pharmacopoeia* already accepted eleven mercurials by 1735, and listed ten in 1841,[35] while the Guy's Hospital *Pharmacopoeia* showed eleven in 1826.[36] Physicians' interest—not always favorable—in mercury was also reflected in the periodical and monographic literature: Goldwater cites Proksch's compilation of over 1000 titles on mercury use in syphilis between 1800 and 1889.[37] More difficult to estimate is the use of mercurials outside formal medical practice—the eighteenth and nineteenth-century pills, mixtures, "Ward's White Drops" and other preparations self-administered by citizens casually for minor complaints, and quietly for venereal disease. One recent writer has reviewed evidence that even the renowned "James Fever Powder," primarily an antimonial, may have included mercury in its early recipe, and that James recommended in his original patent that a mercury pill be always taken with the Powder.[38]

In 1827, the year of Bright's first publication on renal disease, the total mercury imported into Great Britain for domestic use was 199,092 pounds, or—if expressed here in apothecary measure—seventy-five grains for every inhabitant.[39] Of course, some fraction of this, likely not large, would have gone for manufacture of barometers, mirrors, and other non-medical uses. By 1841, 302,814 pounds were accounted to home use, equivalent to about ninety grains *per capita*. Drug firm records furnish other clues to the extent of mercurial popularity in early nineteenth-century British medical practice. Bevan's large London drug house included in an 1804 order to an American purchaser over fifty pounds of various mercury preparations, by weight the largest part of the order.[40]

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[32] L. J Goldwater, *Mercury a History of Quicksilver* (Baltimore: York Press, 1972), p. 239-248

[33] Haller, *Pharmacy in History*, (n 11 above)

[34] Royal College of Physicians of London, *Pharmacopoeia Collegii Regalis Londini* (London 1682, 1763, 1836)

[35] Royal College of Physicians of Edinburgh, *Pharmacopoeia Collegii Regii Medicorum Edinburgensis* (Edinburgh 1735, 1841).

[36] Guy's Hospital, *Pharmacopoeia Guyensis* (London· Philanthropic Society, 1826).

[37] J. K. Proksch, *Die Litteratur ueber die venerischen Krankheiten* (Bonn 1891), cited by Goldwater, *Mercury* (n. 32 above), p. 244

[38] J K Crellin, "Dr. James fever powder," *Trans. Brit Soc. Hist Pharm.*, 1974, *1*: 136-143

[39] Abundant statistics of British population, as well as import and export of quicksilver and nearly all other goods, are found in the "Tables of the revenues, population, commerce, &c of the United Kingdom and its dependencies," beginning as volume 1 (1832) of Great Britain, *Parliamentary Papers: reports, returns, and other papers printed by the Houses of Parliament of Great Britain and Ireland.* In the period of time here discussed, consumption of mercury by the British hat industry was apparently not yet sizeable, see Goldwater, *Mercury* (n. 32 above), pp 261-277.

Several nineteenth-century practitioners attempted to describe, without providing true quantification, the increase in mercury use which they had observed. James Hamilton of England, writing in 1820, reminded his readers that mercurials were seen as poisons in earlier times:

> Accordingly, for some ages after mercury became an article of the Materia Medica, physicians recommended it only on the most urgent occasions—but within these few years British practitioners seem to have overlooked the necessity for such caution, and to exhibit that medicine with very little scruple.[41]

Hamilton, author of a book on childhood disease, was particularly concerned about mercury abuse in the younger age group:

> Thus calomel is now in Great Britain almost the universal opening medicine recommended for infants and children, and a course of the blue pill (which is one of the mildest preparations of mercury) is advised, without any discrimination, for the cure of trifling irregularities of digestion in grown persons.[42]

Hamilton was obviously a critic of mercurial excess, and his remarks must be considered accordingly. A less argumentative commentary on early nineteenth-century mercury use was the 1857 *Rational Therapeutics*, an essay by the New Haven physician and Yale professor Worthington Hooker. Hooker, who graduated from Harvard Medical School in 1829, observed during his years of education and practice a changing attitude toward mercury and other "active" measures. In his 1857 essay he outlined the progress in nineteenth-century medical thought, then appealed for an approach to therapeutics neither "heroic" nor nihilistic. Though giving no supportive numerical data, Hooker believed that

> the reign of active medication, thus established chiefly by Cullen, reached its culminating point somewhere in the first quarter of this century. It was not in-

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[40] S Stander, "Transatlantic trade in pharmaceuticals during the industrial revolution," *Bull Hist Med*, 1969, *43* 326-343

[41] James Hamilton, *Observations on the Use and Abuse of Mercurial Medicine in Various Diseases* (American ed.; New York. E. Bliss and E White, 1821), p. 1

[42] *Ibid*, p. 3

> troduced by him in full, but was fairly begun, and then was consummated in the course of a few years by those who followed him. This may be very distinctly seen in relation to the use of mercury. Cullen, it must be evident to everyone that reads his "Practice of Physic", had no idea of the extent to which this remedy was destined to be applied by his successors in the treatment of inflammation and fevers; much less of the common and indiscriminate use of it described by Hamilton.[43]

As pointed out by Alex Berman[44]—and also suggested by Haller's review of American calomel use[45]—enthusiasm for calomel in the United States, especially in massive doses, probably "peaked" somewhat later than Hooker thought. Nevertheless, one can at least agree that mercurials were already extremely popular in Great Britain (and America) during the early decades of the nineteenth century, the same decades which saw Wells, Blackall and Bright detecting epidemic albuminuria and plentiful cases of nephritis.[46]

That this writer firmly believes—but cannot positively prove—that mercury abuse brought about the "discovery" of renal disease, recalls the problem of those practitioners. Wells and Blackall, but not Bright, thought there was such a relationship, but none knew of a mechanism to convincingly prove or refute the hypothesis.

Present-day medical science shares with other disciplines a remarkably well-accepted convention for evaluating new data and hypotheses—mathematical probability testing. Adherence to the correct use of probability analysis has come close to providing a uniform method for agreeing upon "truth," or at least, for reaching a consensus of opinion. This application has been invaluable in both laboratory investigation and problems of disease etiology and therapeutic efficacy. No comparable tool was familiar to physicians of the first third of the nineteenth century: Wells and Blackall, who believed that mercury could induce renal disease, and Richard Bright, who did not, had no generally adopted means of definitively testing their opinions and proving them correct to the satisfaction of the medical community. Certainly the major works of Louis had by their time appeared in English, and the influence of his "numerical method" is evident in Bright's 1836 "Tabular view of the morbid appearances in 100 cases connected with albuminous urine" and George Barlow's 1843 "Account of observations made under the superintendence of Dr. Bright on patients whose urine was albuminous."[47] Though these and similar works illustrated well Louis's teaching, that the cumulation and organized presentation of observations greatly enhances their utility, the retrospective "numerical method" could not itself indicate what numbers or percentages were decisive. Furthermore, the autopsy—the crucial exercise in the works of Louis and his British contemporaries—is not the best instrument for discovering either cause or cure of disease: examining the static conclusion of lethal illness rarely gives a clue to its remote cause, and certainly no case counted in the *post mortem* lists proclaims effective therapy.

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[43] Worthington Hooker, *Rational therapeutics* (Publications of the Massachusetts Medical Society, Vol 1, No. 2, Boston 1857), p. 160.

[44] Alex Berman, "The heroic approach in 19th century therapeutics," *Bull Am Soc Hosp Pharm* , 1954, *11* 321-327

[45] Haller, *Pharmacy in History* (n 11 above)

[46] Two other possible sources of frequent renal disease in the early nineteenth century may also be considered post-streptococcal glomerulonephritis, and amyloidosis secondary to cavitary tuberculosis. Because of crowding and the lack of antibiotics, scarlet fever and other streptococcal infections may have been more common in cities then than today. However, physicians such as Bright and Wells recognized this entity as "dropsy following scarlatina," and seemed able to differentiate it from the chronic or subacute albuminuric renal failure detailed by Bright (see, for example, William Charles Wells, "Observations on the dropsy, which succeeds scarlet fever," *Transactions of a Society for the Improvement of Medical and Surgical Knowledge*, 1812, *3*. 167-186) How often—if at all—post-streptococcal glomerulonephritis may insidiously lead to chronic renal failure years after the inciting event remains a controversy in nephrology, see Barry Brenner and Floyd Rector, *The Kidney* (Philadelphia W B Saunders, 1976), ch. 23. Amyloidosis, an infiltrative disease of the kidneys and other organs, can be caused by extensive cavitary tuberculosis, and could be postulated as a frequent source of renal deterioration in the nineteenth century Yet Bright found active pulmonary phthisis in only four of the 100 autopsy cases registered in his "Tabular view" (n 1 above).

Animal models and experimentation were also unfamiliar to the majority of practitioners of the first decades of the nineteenth century. This writer could find no reference in that period to any attempt to give calomel to a dog, then test its urine for albumin. James Annesley, investigating in India the usefulness of calomel for dysentery, did in 1825 dose several dogs with the medicine and examine their intestines, and this work was expanded by Murray in 1841;[48] but such experiments seem to have been at the time rare and without influence.

Though seemingly lacking any uniform method of resolving questions of disease causation, the early nineteenth-century British physicians perceived no such disadvantage and were not tentative in their beliefs. Practitioners could consult the standard texts, or reprinted lectures, of Elliotson, Watson and others to learn the current authoritative view-points based on clinical experience. To ask, then, why Richard Bright did not join Blackall and Wells in identifying mercury as a cause of albuminuria may be specious, requiring as answer only that such was not his opinion. One can speculate, nevertheless, on the background of his conclusion.

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[47] George Hilaro Barlow, "Account of observations made under the superintendence of Dr Bright on patients whose urine was albuminous," *Guy's Hospital Reports*, 1843, 2nd Ser., *I*. 189-316.

[48] Cited in United States Surgeon General's Office, *Medical and Surgical History of the War of the Rebellion, 1861-1865*, Part II, Vol 1 J J Woodward, "Medical History" (Government Printing Office: 1875-1889), p.713

Bright continued throughout the decades of his interest in renal disease to rely on the old doctrine of insensible perspiration, and to assign importance to any chill experienced by a person who subsequently suffered albuminuric dropsy. In 1843 George Hilaro Barlow published the "Account of observations made under the superintendence of Dr. Bright on patients whose urine was albuminous,"[49] which tabulated in the manner of Louis observations made in the Guy's Hospital renal disease ward. A variety of associated findings were scored in the paper for the thirty-seven patients studied. Thirty-two of them had been "exposed to cold," but only sixteen reported taking mercury "in quantity." Our contemporary response may be to ask how the information on mercury or cold exposure was elicited, or how many persons with *normal* urine might also by chance have recalled a recent chill. But for Bright, these data served to reinforce his preexisting etiologic thought, though probably they were shaped by it.

This acceptance of traditional etiology as outlined earlier may seem oddly in contrast to the implicit confidence in disease specificity found in Bright's work. Though his writings on renal disease do not deal directly with nosography, in retrospect he may be said to have provided "pathognomonic" descriptions of a symptom complex, a laboratory phenomenon, and a pathologic organ change any one of which suffices to define nephritis as a specific disease. Yet his etiologic argument called not for a specific cause, but appealed instead to the humoral precepts of sympathy and suppressed perspiration. A careful reading of Bright's discussions partly resolves this possible paradox. In 1827 Bright wrote that

> the altered action of the kidney *is the result of the various hurtful causes influencing it* through the medium of the stomach and the skin, thus deranging the healthy balance of the circulation, or producing a decidedly inflammatory state of the kidney itself:—that *when this continues long, the structure of the kidney becomes permanently changed* . . . .[50]

The 1836 paper on renal disease began:

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[49] Barlow, *Guy's Hospital Reports* (n. 47 above)

[50] Bright, *Reports of medical cases* (n 1 above), p 3

> The importance and extensive prevalence of that form of disease which, *after it has continued for some time is attended by* the peculiar changes in the structure of the kidney now pretty generally known by the names of "mottling", "white degeneration", "contraction" or "granulation" impresses itself every year more and more deeply on my mind.[51]

(The emphasis in both passages is that of the current writer.) For Bright, the lesion was not itself "the disease." The disease was still diffuse and ineffable—"a state of great congestion, perhaps an actual process of slow inflammation"—a disturbance flowing among skin, stomach, and kidneys. The kidneys eventually become the source of certain symptoms, but for Bright they were more the victims than the "seat" of a pervasive disease state. Such a formulation does not require nor even invite the idea of a specific causal agent, such as mercury.

Although Richard Bright did advise against the use of mercury for the treatment of albuminuric dropsy, further condemnation of the metal—by labelling it the *cause* of the disease—was likely inhibited also by his educational and institution background. Bright was a Guy's man, and Guy's was traditionally vigorous with medications: the new pathologic anatomy induced no therapeutic nihilism there. The Hospital's most prominent physician just before Bright was James "Calomel" Curry. Believing most disease to be hepatic, and all hepatic disease to be curable by mercury, Curry won his alliterative alias by endlessly championing calomel. As a young medical officer at Guy's, Bright would have attended lectures given by Curry.[52] Also out of Guy's came the popular combination of squill, digitalis and mercury called "Guy's Pills." The listing of eleven distinct mercurial preparations in the Guy's *Pharmacopoeia* of 1826 has been cited earlier. Favoring the tradition of therapeutic forcefulness at Guy's was the passage of the Apothecaries Act of 1815. This law required apothecary students to pass some months in hospital training, and Guy's was quickly able to absorb large numbers of these future general practitioners. Since apothecaries by law could charge only for medicines dispensed, not for their service, it is unlikely that Guy's would have attracted the apothecary students were not its staff firmly confident in the efficacy of drugs.

Probably, then, both Bright's etiologic system and institutional setting favored his interpretation of the observed data: chills, not mercury, caused renal disease. And his was the crucial opinion. He had elegantly and fully described the disorder, and the reputation earned by this achievement helped make him the leading consultant of London. Had he perceived mercury to be the source of "Morbus Brightii," would later nineteenth-century patients have suffered less often from the disease of nephritis, and the cure of calomel?

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[51] Bright, *Guy's Hospital Reports* (n. 1 above), p. 93.

[52] Not the Scottish physician James Currie, this error is repeated in several short biographical articles about Bright. For Curry's evaluation of calomel, see his *Examination of the Prejudice commonly Entertained against Mercury* (London: J. M'Creery, 1809).

# PHOTOGRAPHY AND THE SELF IMAGE OF AMERICAN PHYSICIANS, 1880-1920*

DANIEL M FOX AND JAMES TERRY

A culture of medicine distinct from normal life became visible in the closing decades of the nineteenth and the early twentieth centuries. This new culture of medicine required increasingly specialized knowledge, personnel and equipment. Historians have described the emergence of this new culture relying almost exclusively on written sources. Oral sources have occasionally been available for recent careers and events. Non-verbal sources, commonly used for earlier periods, have not yet been explored systematically for the study of the more recent medical past. The most common examples of the use of non-verbal sources in modern medical historiography are descriptions of buildings, particularly hospitals and laboratories, in which notable medical activities occurred.

This paper describes the systematic study of medical photographs and advocates their use as sources by historians. Because modern photography developed at the same time as the new culture of medicine a century ago, photographs are plentiful and should be regarded as significant statements about the tastes and values of physicians who took, commissioned or tolerated them.

Photographs, like other non-verbal sources, complement and rely on but cannot replace written sources. Non-verbal sources, properly used, enhance our capacity to imagine the past by placing us in closer contact with what was seen or touched or even smelled and heard before. Such contact can be deceptive, since perception is culturally determined. Seeing the past as contemporaries saw it—avoiding historicism and anachronism—is often more difficult than reading the past correctly. Most historians are not trained to be as sensitive to changes in our visual heritage as to shifts in styles of rhetoric and written expression.

After examining and categorizing several thousand medical photographs from the late nineteenth and early twentieth centuries, we hypothesize that these photographs document the emergence of a culture of medicine distinct

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from normal life. Photographs document this culture by preserving the image physicians had of themselves: an image they were eager to project to a public which had long been skeptical of their capabilities. Although physicians projected their self-image through photographs, they were usually not articulate about either the emergence of the new culture of medicine or the record of the culture that would remain in photographic sources. As Jacques Barzun wrote in a notable essay on the methodology of cultural history, "To ourselves we have no style—we just are. . . ."[1] Because of the lack of verbal self-consciousness about the style of photographs, our hypothesis cannot be confirmed by direct reference to written sources but must be supported by visual and inferential data.

## I

Historians have been skeptical about our argument on a number of public occasions. Our colleagues' doubts, like our own during the earlier stages of this project, reflect the deeply rooted methodological caution that has prevented historians from becoming permanent captives of numerous scholarly fads in the past century. Historians have, for instance, been dubious about the value of most generalizations about individual and social behavior.

Resistance to our hypothesis about the significance of photographs is in some measure skepticism about the value of speculative cultural history. Most historians are reluctant to believe that the people and institutions of any place in any period resembled each other so completely that the study of one set of sources leads to generalizations that can substitute for the study of other sources. The contrary view of historical method, the assertion of cultural unity, has been advocated most vigorously in the United States in the past generation by scholars of literature and the fine arts associated with the American Studies movement.[2] The hypothesis of cultural unity has also been supported by many sociologists.[3] Behind both groups is a rich tradition of nineteenth-century thought, mainly German and French, arguing that people in any geographical area in any period of time with common history and institutions shared a spirit or set of attitudes which differed from the particular attributes of individuals.[4] Most contemporary historians resist such speculation and assert that documentary behavioralism is the only valid method of history.

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[1] Jacques Barzun, "Cultural History as a Synthesis," in Fritz Stern ed., *The Varieties of History* (Cleveland, Ohio. Meridian Books, 1956)

[2] A recent review of methodology in American Studies is Cecil F. Tate, *The Search for a Method in American Studies* (Minneapolis: University of Minnesota Press, 1973). Scholars associated with the American Studies movement have used visual sources for almost half a century. The problems particular to visual sources have rarely been explored systematically. In the absence of extended general analysis of the use of visual sources, the problems involved in using photographs in historical interpretation remain largely unstated. Notable pioneer efforts in the use of visual sources for the study of nineteenth-century American society and culture include, Lewis Mumford, *The Brown Decades: A Study of the Arts in America, 1865-1895* (New York: Harcourt, Brace and Co., 1931); Francis Otto Matthiessen, *American Renaissance* (New York, Oxford University Press, 1941); John A. Kouwenhoven, *Made in America* (Garden City: Doubleday, 1948), Henry Nash Smith, *Virgin Land* (New York. Vintage Books, 1950), and Leo Marx, *The Machine in the*

Documentary behavioralism is our phrase for the belief that valid statements about events or ideas in the past require evidence that is recorded in writing, preserved and available for other historians to examine. As an antidote to the worst excesses of philosophical idealism and the law-making of the social sciences, documentary behavioralism is a useful approach to the past. But documentary behavioralism is a rigorous rather than a rigid standard. Historians must deal in a systematic and convincing way with non-verbal evidence when it bears on their areas of concern.

Medical photographs from the late nineteenth and early twentieth centuries are plentiful and have common characteristics but lack a clearly documented contemporary explanation. If anyone wrote down why a photograph was taken, why it was composed as it was and why it was worth preserving, the written records have not yet been found. Harvey Cushing, the only physician we have discovered who cared deeply enough about both writing and photography to record the medical life of his times in both words and pictures, provided no written explanation for the photographs he preserved.[5] Nevertheless thousands of photographs exist. To ignore such a plentiful source is at least as unsettling as assessing its value without the security of written evidence.

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*Garden* (New York: Oxford University Press, 1964) More recent efforts to study the history of popular culture and non-elite groups have produced significant work using visual sources. Examples include Robert C Toll, *Blacking Up The Minstrel Show in Nineteenth Century America* (New York. Oxford University Press, 1974) and Robert Sklar, *Movie-Made America* (New York. Random House, 1975). Sklar's recent paper, "The problem of an American studies 'philosophy' A bibliography of new directions," *American Quarterly*, August 1975, *27*, 245-262, is an outstanding review of contemporary methodological issues

[3] Useful introductions to the history of sociological thought are: Lewis A Coser, *Masters of Sociological Thought Ideas in Historical and Social Context* (New York: Harcourt, Brace, Jovanovich, 1971); Don Martindale, *The Nature and Types of Sociological Theory* (Boston: Houghton Mifflin, 1960); and Alvin W. Gouldner, *The Coming Crisis of Western Sociology* (New York Basic Books, 1970)

[4] There is a vast literature on the intellectual history of the concepts of nationalism, culture and national character. Stern, *op cit* (n 1 above), remains a useful collection of the writings of historians of the past several centuries as they reflect the intellectual history of their times

[5] Scrapbooks, journals and other sources for Cushing's life and work in the Yale Medical Historical Library, New Haven, Connecticut

The case for medical dominance of decisions about what to photograph—that is, the case for our hypothesis that photographs document physicians' self-image—rests on inference, not on appeal to precepts about culture or society. We infer our stance from the history of photography and its application to medicine and from the history of late nineteenth-century aesthetics.[6]

The history of photography made possible though it could not guarantee physicians' control of their photographed self-image in the late nineteenth century. Technical and aesthetic conditions temporarily made amateur photographers more important than professionals and created congruence between what most people believed photographs meant and what physicians believed was important about their work.

Technical innovation in the last third of the century made photography broadly accessible. Prior to the 1880s, most photographs were made from wet plates. Light sensitive silver nitrate was coated over a copper plate or over glass. Wet-plate equipment was cumbersome, exposure times for photographs were slow, and consistently good results were hard to achieve. Daguerreotypes, which were the only dry plate photographs, were made with copper plates as a medium for the silver nitrate and thus could not be reproduced. Even though daguerreotypes required cumbersome equipment and had limited usefulness, their popularity, particularly in the United States in the 1850s, foreshadowed the mass market for photography created several decades later. By the late 1870s, the invention of dry photographic emulsion made possible the development of cellulose film. Photography changed rapidly between the 1880s and the 1920s. Equipment became smaller, lighter, cheaper and more versatile. At the same time that mass markets were created for cameras, film and developing services, professional photography became an accepted trade, and a few pioneers explored photography as art.[7]

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[6] We have found only one other paper in the historical literature which makes similar inferences from the history of photographic technique and popular perceptions of it and applies these inferences to explain other historical questions. Ferenc M. Szasz and Ralph F. Bogardus, "The camera and the American social conscience: The documentary photography of Jacob A. Riis," *New York History*, October 1974, *55*: 409-436. Two recent books by Michael Lesy, *Wisconsin Death Trip* (New York: Pantheon, 1973) and *Real Life. Louisville in the Twenties* (New York: Pantheon, 1976) attempt, by arraying photographic images without any grounding in written sources, to insist that only by immersion into the images themselves could one recapture the look and feel of what life was like in times past. Lesy begs the methodological questions raised by the use of visual sources by displaying rather than examining and explaining the images he has selected. Wright Morris, photographer and novelist, reviewing Lesy's second book in *The New York Times Review*, November 24, 1976, *2*: 24-26, put the problem quite clearly: "Numerous contrasting versions of real life in Louisville might be assembled from these same materials. Photographs do reassure us about the nature of the visible world, but they do not determine what is real about it." Two important additions to the literature on the use of photographs for the study of history appeared after this paper was submitted for publication: Marsha Peters and Bernard Mergen, " 'Doing the Rest': The uses of photographs in American studies," *American Quarterly*, Summer 1977, *29*: 280-303; and David Galloway, *A Family Album* (New York: Harcourt, Brace, Jovanovich, 1978).

An aesthetic free period began about 1880 and lasted until the second decade of the twentieth century. Because cameras could now capture most action in most settings, the composition of photographs of people in motion ceased to be limited by traditional aesthetic rules about images that were posed or imagined. Aesthetic judgments about images which occurred quickly and often casually in historical time required new rules. For a brief period after the 1880s most photographers, whether professional or amateur, were unconstrained by conventions. The aesthetics of photography as art, journalism, advertising, or as a tool for teaching and scientific documentation were just beginning to develop. Like snapshots of family occasions today, most formal photographs in the years around the turn of the century represented mainly what a photographer or his employer wanted to preserve, limited by what the cameras at hand could do and what the subjects were willing or able to tolerate as appropriate.[8]

Aesthetic freedom did not mean the absence of convention, merely the lack of established canons for choosing among inherited or developing conventions. Most posed photographs, medical and general, borrowed the conventions of the graphic arts. Ironically the medium which rendered much engraving and drafting obsolete long imitated earlier graphic forms in composition, lighting and tone. Informal photographs—candid shots, hurried poses—also reveal the photographer's feeling that the right time to snap was when what he saw resembled what the traditions of the graphic arts taught him to see.

In a short time, however, photography ceased to imitate earlier conventions and became, in William Ivins' phrase, the "norm of truthfulness."[9] Historians of photography note that this change occurred more rapidly in the United States than in Europe. American daguerreotypists, according to Richard Rudisill, "seldom if ever, cared to go as far into allegorical mystique" as their European counterparts. By the time dry plates, portable equipment and the marketing genius of George Eastman made personal photography broadly accessible to the middle class, photography had become the standard for reality. Rudisill continues: "Once an image could be certified as reliable by its origin in a daguerreotype it was acceptable as definitive truth to be questioned thereafter only on the basis of a better image produced by the same means."[10]

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[7] The standard general works on the history of photography, placing it in the context of the development of science, technology and commerce are Helmut and Alison Gernsheim, *A Concise History of Photography* (New York: Grosset and Dunlap, 1965), Beaumont Newhall, *The History of Photography from 1939 to the Present Day* (New York The Museum of Modern Art, 1964), and Robert Taft, *Photography and the American Scene* (New York Macmillan, 1938, Dover Publications, 1964) A notable recent work is Reese V. Jenkins, *Images and Enterprise Technology and the American Photographic Industry 1839-1925* (Baltimore. Johns Hopkins University Press, 1975).

[8] The standard works discuss aesthetic change, but better sources are writings about and exhibitions of pictorial, or art, photography. For example: The William Hayes Ackland Memorial Art Center, *The American Situation The Camera's Century* (Chapel Hill: University of North Carolina Press, 1977), and Patricia Hills, *Turn of the Century America* (New York Whitney Museum of American Art, 1977) Jonathan Green, in his introduction to *Camera Work A Critical Anthology* (New York Aperture, 1973) wrote, "During the last half of the nineteenth century, there was no recognized aesthetic that would certify the straight photograph as an expressive object" (p. 18)

Physicians had an early and intense involvement with photography. One physician, R. L. Maddox of England, was an important technical innovator who published a paper in 1871 which established the rules for dry-plate photography. Photographs of cadavers and of mental patients became abundant in the 1840s and 1850s. During the American Civil War, notable photographs of wounds and field hospitals were widely circulated.[11]

Beginning in the 1870s the medical press began to publish what became in the twentieth century an extensive literature on photographic technique. Physicians were offered many practical suggestions about taking, developing and using photographs. The medical press paid increasing attention to photography. Although only two monographs on the subject appeared between 1870 and 1880, six were published in the next decade, and seventeen between 1890 and 1900. The largest number of these publications were in United States or German journals.[12]

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[9] William M. Ivins, Jr., *Prints and Visual Communication* (New York Da Capo Press, 1969), 94

[10] Richard Rudisill, *Mirror Image. The Influence of the Daguerreotype on American Society* (Albuquerque: University of New Mexico Press, 1971), 160, 231

[11] For Maddox and the impact of his work in the United States, see Taft, *op cit* (n 7 above), 365-377. Maddox's paper appeared in the *British Journal of Photography* for September 8, 1871 For Dr Hugh Diamond as a photographer of mental patients, see Edward Lucie-Smith, *The Invented Eye* (New York· Paddington Press, 1975), 40. An exhaustive survey of the interests and contributions of physician-photographers is Oliver Mathews, *Early Photographs and Early Photographers* (New York: Pitman Publishing Co , 1973). A general review of nineteenth-century medical photography is Alison Gernsheim, "Medical photography in the nineteenth century," *Medical and Biological Illustration*, 1961, *50*· 85-92, 147-156. Photographs were used to illustrate, for instance· Hugh Diamond Welch, *Physiognomy of Insanity* (London, 1854) and G B. A. DuChenne, *Album de photographie pathologique* (Paris, 1862) For Civil War medical photography see Roy Meredith, *Mr Lincoln's Camera Man Mathew B Brady* (New York Scribner's, 1946), and James D. Horan, *Timothy O'Sullivan, America's Forgotten Photographer* (Garden City. Doubleday, 1966). Occasionally cited for medical photography but unreliably annotated is Otto L Bettmann, *A Pictorial History of Medicine* (Springfield, Ill Charles C Thomas, 1956).

[12] The best secondary source is Alison Gernsheim, *op. cit.* (n. 7 above). Other useful secondary sources include: L A. Julin, "A history of still photography in the operating room," *Journal of the Biological*

The medical photographic literature was usually silent about aesthetics. Emphasis was on equipment and technique narrowly defined to include the elements of each picture, but never the finished photograph as a whole. Dr. Ellerslie Wallace, Jr., of Philadelphia appears to have been the only American physician to publish an entire book about photography in the nineteenth century. In *The Amateur Photographer*, published in 1894, Wallace made no direct reference to medicine. He described the most recent advances in photographic technique and took an orthodox, painterly approach to aesthetics. The only way to learn to compose photographs, he declared, is "by the unremitting study of good paintings . . . not by random photography of any scene that may happen to please at the moment."[13]

In a paper on "Photography in Medicine" in the *Journal of the American Medical Association* in 1897 Wallace moved inconsistently toward a purely medical aesthetic. Although he repeated briefly his bias toward "sympathy with artistic studies," he emphasized the uniqueness of medical subjects.

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*Photography Association*, 39, July 1971, 129-143; R. Ollenshaw, "Medical illustration: The impact of photography on its history," *Journal of the Biological Photography Association*, February 1968, *36*: 3-13, C. F. Mayers, "From drawing to photography in color . . . ," *Medical Life*, 1936, *43*: 590-604, Lola L. Szadits, "Medical Illustrations and Portraits: a Brief Survey of Collections in America," *Eye to Eye*, 1953, *1*: 13-22, and E. B. Wells, "Pictorial resources at the National Library of Medicine," *Medical Annals of the District of Columbia*, 1967, *36*: 365-366.

A representative sample of primary sources, drawn from the medical literature to 1900, follows. In alphabetical order by author: P. Bernard, "De quelques applications de la photographie à la médecine," *Journal des Sciences Médicales de Lille*, 1888, *2*: 169, Julius Mount Bleyer, "Researches of rapid photography by means of Edison's kinetography . . . with a historic review of fifty years of photography," *Journal of the American Medical Association*, 1896, *26*: 671-677, 727-730; D. M. Bourneville and P. Regnard, *Iconographie photographique de la Salpetriere* (Paris: Delahaye, 1876-80), 3 vols., Louis A. Duhring and Francis Fontaine Maury, eds., *Photographic Review of Medicine and Surgery* (Philadelphia: J. B. Lippincott and Co., 1871-72); H. Edwards, "Medical photography," *Birmingham Medical Review*, 1894, *35*: 149-155; W. His, "Ueber Verwertung der Photographie zu Zwecken anatomischer Forschung," *Anatomischer Anzeiger*, 1891, *6*: 25-30, Ludwig Jankau, *Die Photographie in der praktischen Medizin* (Munich: Seitz and Schauer, 1894) (numerous papers by Jankau appear in the German medical press of the 1880s and 1890s); R. Kretz, "Die Anwendung der Photographie in der Medicin," *Wiener klinische Wochenschrift*, 1894, *7*: 823-834, Albert Londe, *La photographie médicale: Application aux sciences médicales et physiologiques* (Paris: Gauthier-Villars et fils, 1893); C. B. Longnecker, "Photographic illustrations in modern medical and surgical literature," *Pennsylvania Medical Journal*, 1906-07, *10*: 108-110, A. S. Murray, "Photography applied to surgery," *Johns Hopkins Hospital Reports*, 1892-94, *3*: 423-428, G. H. Niewenglowski, "L'oeil et l'appareil photographique," *International medicinischer photographische Monatschrift*, 1895, *2*: 65-69, A. Neisser, *Stereoscopischer medicinischer Atlas* (Cassel: T. G. Fisher, 1894), W. Noyes, "Composite portraiture of the insane," *Science*, 1888, *11*: 252; G. S. Pierson, "Medical photography," *New York Medical Journal*, 1886, *48*: 728-730, S. Rowland, "Report on the application of the new photography to medicine and surgery," *British Medical Journal*, 1896, *1*: *passim*, Ransford E. Van Gieson, "The Application of Photography to Medical Science," reprint from *The New York Journal of Medicine* (New York: Thomas Holman, 1860), C. Workman, "Photography in medicine and allied sciences," *Glasgow Medical Journal*, 1890, *35*: 36-47.

[13] Ellerslie Wallace, Jr., *The Amateur Photographer* (Philadelphia: Porter Coates, 1884), 188-189.

Without perceiving any contradiction, Wallace enthusiastically described the need for new rules of technique and judgment to govern photography through the microscope and the difficulties of photographing soft tissue. "Among the wonders of modern Photography," he asserted, "[is] . . . the fact that some kinds of diseases accompanied by eruption will show the eruption on the photograph *before* it is visible to the eye." For physicians who became fascinated with photography as an aid to diagnosis and teaching, "there is no end of work . . . and of fresh matter too."[14]

Where Wallace held that medical photography should both imitate and be different from art, Dr. Boardman Reed of Atlantic City, writing in *The Medical Record* in 1889, asserted the more conventional view that the photograph was, by definition, reality; what one saw was what was. For Reed, who came to photography as recreation and an extension of puttering in a chemical laboratory, the direct correspondence of the photograph to reality prevented any tendency "to stretch the truth." Photography, he believed, contrary to Wallace, who looked simultaneously to medicine and to the history of art, "conduces to literal exactitude in all respects."[15]

However, literal exactitude, photography as truth, created grave problems for the medical profession because pictures were easily accessible to the laity. A physician in Galveston addressed this problem in a long letter to the editor of *The New York Medical Journal* in 1894. Deploring the "craze for photography" in medical illustration, Dr. William Keiller criticized photographs which "expose the person quite unnecessarily and therefore indecently." Worried mainly about photographs of "nude women in innumerable modifications of the dorsal, dorso-lateral and Trendelenberg positions," he argued that diagrams were both more "circumspect" and more useful.[16]

Aesthetic questions did not arise for most physicians who took, examined or saved photographs. They assumed that pictures were simply frozen reality, even if they were posed and even if their composition owed a great deal to the imaginative history of the visual arts. Dr. Wallace recognized the importance of the iconography of art, but adhered so rigidly to it that he accepted inconsistency rather than the more difficult task of stating a new aesthetic principle. But most of his contemporaries, within and outside medicine, had little apparent difficulty evading the question of the relationship of the photographed image to reality.

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[14] Ellerslie Wallace, "Photography in medicine," *Journal of the American Medical Association*, 1897, *28*, 775-776

[15] Boardman Reed, "Why physicians should cultivate photography," *Medical Record*, 1889, *36* 514-515

[16] William Keiller, "The craze for photography in medical illustration," *New York Medical Journal*, 1894, *59* 788-789

Detailed exactitude was highly regarded by physicians, increasingly carefully trained in precise scientific and clinical observation. Pride in precision and detail was reinforced by other events in the period. The concept of "realism," supported by the Darwinian paradigm, became a useful description for widely admired activities in politics, commerce, diplomacy, painting and prose. For most citizens of Western Europe and the United States, "realism" explained much of the complexity of late nineteenth-century life. Among the events often discussed under this label were rapid economic change, the spread of new technology, the development of the natural sciences, the diplomacy and international conflict of imperialism and colonialism, the bitter competition of economic life, and the persistence of misery, poverty and disease.[17]

Photographs seemed to be a logical way to record the complexity of life. People who valued realism welcomed photography. Physicians, influenced both by the ideas and assumptions of the times and the internal history of their profession, were comfortable with this new medium. Photography reinforced strongly held assumptions about the world in which they lived.[18] It offered no difficulties of understanding or technique that could not be explained by the same principles used to account for the advance of medical science and indeed the progress of civilization.

## II

Photographs are not frozen reality. They are collections of images consciously or unconsciously selected by some combination of subject, photographer and collector. The principles by which any collection of photographic images were selected and preserved were occasionally written down, but mainly for photographs which were consciously taken as art. For most photographs, and certainly for most photographs of medical settings, the principles by which images were selected and preserved must be inferred.

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[17] There is a notable literature on this subject A classic work is Robert C Binkley, *Realism and Nationalism, 1852-1871* (New York and London: Harper and Brothers, 1935) Recent books include for art, Linda Nochlin, *Realism* (London and New York Pelican Books, 1971); for American politics and society, Morton Keller, *Affairs of State* (Cambridge Harvard University Press, 1977), and for international commerce and diplomacy, Fritz Stern, *Gold and Iron Bismarck, Bleichroeder and the Building of the German Empire* (New York· Alfred A Knopf, 1977)

[18] *Cf.*, Hills, *op. cit* (n. 8 above), 12 "To these artists the reality of the technological and communications explosion and the truth of the effects of industrialization on everyday lives contained greater creative possibilities than idealized Grecian women, pastoral landscapes " A similar point is made about Jacob Riis' influence by Szasz and Bogardus, *op. cit.* (n 6 above), 423, 525; *cf* Lucie-Smith, *op cit* (n 11 above), 34, 44 A notable example of the convergence of art, photography and medicine to reinforce the concept of realism was the work of Thomas Eakins, notably his anatomical drawings and photographs, see *The Art of Philadelphia Medicine* (Philadelphia Museum of Art, 1965).

Inferences about patterns of images can be made on the basis of an iconography. In order to make tentative generalizations about medical photographs in the aesthetic free period of 1880-1920, we developed a set of visual categories as the basis for an iconography. An iconography permits general statements about the significance of whole groups of images by comparative analysis. Iconographic analysis is thus a method for "reading" any given picture, comparable to the tools used to "read" written documents—where the word "read" includes by implication such other intellectual activities as "understand," "evaluate," "decipher," and "place in context."

Photographs, like written texts, have both a content and a style. Both content and style must be analyzed to use photographic images as historical documents. However, because the analysis of the style of images is not as well developed as literary or rhetorical analysis, and because the act of the photographer differs from the comparable acts of writers and speakers, terms like "content" and "style" are not specific enough. These terms permit images to be classified only according to subject matter wherever the photographer's choices are not very clear or sophisticated. Most photographic classification to date has been subject-indexing: for instance, images of surgery, images of public health measures. Such classification does not treat the visual document in its full richness and complexity. An iconography is necessary to approach photographs in ways which enable the questions and conclusions generated by the study of one image to offer the richest suggestions for approaching other images. Such a method will help to make the visual past, along with its broader historical context, more intelligible.

The use of iconographic analysis has an established place in the methodology of contemporary art history, from the principles of Heinrich Woelfflin to the more recent work of E. H. Gombrich and Kenneth Clark. Our method draws upon and parallels the general assumptions of such analysis, but it does not derive directly from it, partly because art and photographic historians have long been in conflict about the aesthetic importance of photographs. Without taking sides in this conflict, we have been influenced by contemporary critics and analysts of photographs such as John Szarkowski and Hilton Kramer as well as by art historians.[19] We offer the system that follows as a theoretical and methodological departure from the previous work of art historians, photographic critics, and social historians.

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[19] Heinrich Woelfflin, *Principles of Art History* (Gloucester, Mass P Smith, 1965). The numerous significant works by E. H. Gombrich on this subject include, *Art and Illusion A Study of the Psychology of Pictorial Representation* (Princeton: Princeton University Press, 1969), *Art, Perception and Reality* (Baltimore. Johns Hopkins University Press, 1972); and *Illusion in Nature and Art* (London: Duckworth, 1973) The works of

The inconography that we have used consists of an analysis of three separate components of the image and its history. We suggest a fourth analytic procedure as a check on the results of the first three. The first step in analyzing a photograph involves treating it, temporarily, as a self-sufficient "text" in visual terms. Several issues require attention. The subject of the picture must be studied and questioned. Is it rare to see a photograph of a particular subject at a particular time? Or is the image like thousands of other pictures in its general choice of a subject to depict? The details of the subject must also be examined. Since the very earliest days of photography, the camera's ability to capture images not available to or noticeable by the average human eye has been recognized. Some effort should also be made to assess any unusual details and to decide whether their presence is common or uncommon, intentional or unintentional.

Another approach to photographs as self-sufficient "texts" involves attention to the treatment of subject and detail. Consideration of the framing of the picture is important. What is included or excluded in the two-dimensional frame of the picture, and their juxtapositional effect on each other, should be noted and tentatively evaluated. For example, are hospital personnel purposely included in a ward scene? Do the instruments and cabinets of a surgical amphitheater appear in a photograph by accident or design? Similar questions must be raised about the photograph's timing. Is the precise moment of exposure significant to the picture's subject or to its impact? How much time did the photographer have to prepare, to pose his subjects and to choose his angle of view? If people are present in the image, how conscious are they that a picture is being made? Finally, explicit questions should be asked about the photographer's vantage point. Whether the photograph is taken head-on or from above or below, whether the people appear in a central position or on the periphery, for instance, may have some significance for the photographer's perception of his subject.

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Erwin Panofsky are important to the modern methodology of art history, notably *Meaning in the Visual Arts* (Garden City, N Y Doubleday, 1955). Kenneth Clark's best-known extended iconographic analysis is *The Nude A Study in Ideal Form* (Garden City Doubleday, 1956) For John Szarkowski see *The Photographer's Eye* (Garden City. Doubleday, 1966) Hilton Kramer's views are expressed in various short pieces, many collected in *The Age of the Avant-Garde An Art Chronicle of 1956-1972* (New York Farrar, Straus and Giroux, 1973). Two books of considerable interest which appeared after this paper was submitted are Jonathan Bayer, *Reading Photographs Understanding the Aesthetics of Photography* (New York: Pantheon Books, 1977), and Susan Sontag, *On Photography* (New York. Farrar, Strauss, and Giroux, 1977)

A final "textual" question involves technical information. Pertinent information includes the kind of lighting, the quality of the print achieved and evidence of possible cropping or retouching of the negative. Where possible, the technical capacities and limitations of the photographic equipment itself should be taken into account.

A second component of iconographic analysis moves beyond the photograph as text to consider the actors in the various stages of the life of the image. The first and most obvious person to consider is the photographer. In rare cases, something is known about the men who recorded medical images, and this information should be examined for possible significance. The photographer's biography and training in the state of the art, his intentions in making the images, and his biases in subject matter and style of presentation and technique should be checked where possible.

A more difficult set of questions involves the potential audience for photographs. In some cases pictures are made for entirely personal reasons; in others, the photographer has a specific audience in mind, such as the readers of an illustrated bulletin for a hospital or medical school, or the recruits to be gained by the publication of a manual on how to become a nurse. Where the photographer is anonymous and the presumed audience is ambiguous, a pertinent question is, why was the image saved? What significance may the picture have had, and to whom, as a slice of the medical past or the record of an institution?

The third component of iconographic analysis considers the image in historical context. The picture may record something which can be cross-checked in other sources of medical or social history. For instance, was the use of rubber gloves common or uncommon in surgery at a particular place and time, and hence, is their absence in a given photograph significant? How novel were particular public health procedures when they were photographed? Such questions lead beyond the immediate facts at issue. The importance of photographing and subsequently saving the image of a medical "first" during the great age of medical "firsts" around the turn of the century is, for example, a historical phenomenon worth examining.

Related questions about the depiction of the subject by the photographer concern his perception of his environment and of historical change. "State of the art" questions examine the potential response of the photographer to the scenes and artifacts around him. Was he impressed by medical progress, enough so that he portrayed it favorably in his images—or was the technical and aesthetic information available to him considerably more primitive? Photographic history is important here, both technically and aesthetically.

Did the photographer take the "best" head-on shot possible (ca. 1880) or the "best" snapshot (ca. 1920) or did he rely on older conventions to guide his finger and lens? Photographic history suggests rough dates for the acceptance of the concept of "snapshots." Before this time most images were composed according to the standards of paintings and other graphic media. Some judgment should also be made about the photographer's obedience to, ignorance of, or violation of prevailing attitudes toward his art and its proper results.

In addition to the contextual fields of medical and photographic history, social history for the period of a given photograph can be used to generate questions and possible information about the image. Many medical images from the turn of the century are enriched by a knowledge of the development of urban institutions, social reform movements, or the general sense of rapid social change. The iconographic analysis must include some effort to assess the way a specific picture of a medical setting or situation reflects, in even the most distant sense, the state of society at the time the image was recorded.

The fourth analytic procedure derives from the dichotomous tensions in photography since its invention in the nineteenth century. As a way of comparing one image to another, it is helpful to run through a series of dual qualities, deciding in each case on which side of an imaginary intellectual line the image falls. Is the image an effort at art or is its purpose technical or scientific? Even if the image was aimed at illustrating some written text or adorning a wall, it can be said to fall on the "art" side. Similarly, one must ask whether the image is composed or candid, pre-imagined or accidental. A related, though even more speculative question is, does the image convey only the photographer's expressed meaning or does it contain latent, multiple meanings of which he was unaware when looking through the lens? These questions, while concerned with the consciousness of the recording process, force attention to details and their significance which may provide additional inferences about aspects of text, context or photographic "actors."

A second set of dualities begins with the tension between two-dimensionality and three-dimensionality. Do the objects in view look "real" or do they, by their juxtaposition on the photographic paper, seem abstract shapes? Is the impulse of the viewing eye, when first exposed to the image, to focus on the content of the photograph, or to see its two-dimensional pattern? In terms of the quality of the photograph as technical achievement, is the texture or print quality more predominant, or does the image exhibit more skill in its focusing, lighting, and timing? These questions seek to assess the precise manner of the photograph's impact, as another way of approaching the problem of what contemporaries saw and felt about a picture, or why it was saved, or how it related to the aesthetic or practical issues of its time.

We developed and expanded our iconography as we sought and examined photographs. After the first collecting trips, to the National Library of Medicine, we felt comfortable with the broad categories we had tentatively stated. In the early stages of the inquiry, we analyzed each photograph according to what became a tiresome, if useful, list of questions. Subsequent collecting trips proceeded more efficiently. After subjecting several thousand images to our categorizing questions, we became adept at deciding quickly whether particular photographs were conventional or unusual and whether they confirmed or challenged our tentative generalizations about medical images and self-images. The narrative that follows is based on the analysis of photographs located mainly in the National Library of Medicine, the National Archives, the Smithsonian Museum of Science and Technology, the Countway Library, Yale University Medical Library, the New York Public Library and the library of the United Hospital Fund of New York. In addition, we examined numerous photographs used to illustrate books and reports of the period.[20]

## III

Photographs taken during the aesthetic free period after 1880 document an increasingly sharp contrast between medical and domestic culture. In many shots, personality is subordinate to technique. Occasionally a caption makes subordination explicit. For example, one 1909 photograph of equipment and a patient staring into space at the Central Dispensary in Washington, D.C.,

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[20] The best source for medical photographs in organized collections is Ann Novotny and Rosemary Eakins, *Picture Sources 3* (New York. Special Libraries Association, 1975).

In the course of discovering and analyzing photographs we were aided by a number of generous archivists and scholars Lucinda Keister at the National Library of Medicine assisted at an early and critical stage of the project Richard Wolfe of the Countway Library at Harvard devoted considerable energy and initiative to helping us find useful photographs Audrey Davis and Everett Jackson at the Smithsonian Institution provided access and welcome assistance Louise Heinze, archivist of the United Hospital Fund of New York helped find photographs and provided an audience for the earliest version of this paper Moe Foner and Louise Johnson of Local 1199, RWDSU/AFL-CIO, shared photographs as they prepared the special issue of the *1199 News* for September, 1976, titled, *Working in Hospitals Then and Now* Guenther Risse, Judith Leavitt, Ronald Numbers and Patricia Ward, University of Wisconsin; Martin Pernick of Hershey Medical Center, Pennsylvania State University, David Musto and John Thompson of Yale University; and Harry Fritts, Marvin Kuschner, Steven Stowe and Peter Williams of the State University of New York at Stony Brook made useful contributions to the discovery and analysis of pertinent photographs was captioned "X-Ray Department," ignoring the patient.[21] Just as technique came to dominate personality, domesticity began to serve the purposes of scientific medicine. As hospitals rather than homes became the places in which middle and upper class people were sick, rooms were at first domestic. Photographs from the 1870s and 1880s display hospital rooms decorated with rugs, plants, and pictures. Private rooms often were elegantly appointed.[22]

However, hospitals gradually came to resemble industrial establishments more than homes. Most hospital rooms photographed after the turn of the century contain few concessions to domesticity. Wards changed dramatically. Before the mid-1880s, most wards were dark and cluttered. After the mid-1880s wards are typically light and clean.[23] Ward design, following theories of hygiene and disease, began to emphasize the segregation of illness from the routine of daily life.

Physicians' private offices reflected the new culture of scientific medicine more slowly than did hospital rooms. Most offices appear in photographs either carefully organized or cluttered. The appointments of a Boston ENT specialist's consulting room in the 1880s suggest that it was furnished to evoke both comfort and orderly knowledge.[24] Most photographs from Western States show physicians and patients conversing in cluttered offices: equipment, not the dress or stance of individuals, connects these images to medicine.[25]

Numerous photographs were posed according to the conventions of the graphic arts rather than the emerging culture of medicine or the increasingly independent aesthetics of photography. Several photographs of nurses and children evoke the classic madonna of Christian art.[26] Sometimes, particularly before the technical changes in photography of the 1880s, pictures were posed in ways which suggest both painterly conventions and social status. Soldiers, for instance, seemed often to be displaying their ranks as well as their wounds.[27]

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[21] X-Ray Department, Washington, D C., Central Dispensary, 1909 National Library of Medicine (hereafter NLM) file #WX2AD6C6

[22] Two examples, both from Roosevelt Hospital, New York, in 1898, are a photograph of a private room and of the N B Bliss Ward From *The Roosevelt Hospital, 1871-1957* (New York Roosevelt Hospital, privately printed, 1957), New York Hospital Fund Library (UHF) Our procedure for the balance of this paper will be to cite the most accessible examples when we refer to a class of pictures

[23] For an uncluttered room, Massachusetts General Hospital, Glass Plate c 1900, negative #1H7P72-8290 Smithsonian Institution, Medical Division, Museum of History and Technology The NLM collection and UHF Annual Reports have many examples of wards in various stages of clutter and darkness. For a discussion of changes in hospital rooms in the period see John D Thompson and Grace Goldin, *The Hospital A Social and Architectural History* (New Haven Yale University Press, 1975), 207-216.

[24] Examining Room, no date, Countway Library (CWY).

[25] The most accessible source for Western photographs is Robert Karolevitz, *Doctors of the Old West A Pictorial History of Medicine on the Frontier* (New York Bonanza Books, 1967)

[26] For instance, a photograph taken in 1898 at the Massachusetts Charitable Eye and Ear Infirmary and used to illustrate Jane Hodson, *How to Become a Trained Nurse* (New York William Abbatt, 1898)

Ethical constraints did not operate uniformly to protect patients' privacy. People who received medical charity were the most frequent subjects for photographers. Their expressions suggest that they generally accepted the camera as another intrusion in their lives, to be borne cheerfully, stoically or sullenly.[28] However, physicians and private patients west of the Mississippi River appear to have been more relaxed about photographs.[29]

Patients' anonymity was often a result of disease or age as well as of ethics or local customs. A series of photographs of patients with smallpox, most painfully disfigured, was taken by a house officer in Boston in 1902 to illustrate his notebook of patients' records.[30] A frequent shot of babies in a hospital bassinette combined anonymity with sentimentality.[31] The frequency with which this photograph was taken and reproduced may reflect anxiety about the substitution of children, a fantasy expressed elsewhere in the culture of the period; for example, in Mark Twain's *The Prince and the Pauper*.

There was a major change in the subject matter of medical photographs between the 1880s and the second decade of the century. By the end of the period, the affliction rather than the patient became the focus of photographic interest.[32] Although this change was in part a result of improved equipment and technique, it paralleled well-documented changes in the preoccupations of physicians. Lesions were worth examining more closely when physicians had more confidence that they could be explained.

[27] "Collection of photographs illustrating gunshot wounds received during the U S Civil War," c 1870, NLM, Box #1 Notable poses combining images of both social status and painterly conventions are illustrations in Lewis A Sayre, M D. *Spinal Diseases and Spinal Curvature* (Philadelphia: Lippincott, 1875)—courtesy of Richard Wolfe. Conventions of erotic art blend uneasily with scientific exactitude in many photographs published since the end of the last century The earliest example we have found is Dr. C H Stratz, *Der Koerper des Kindes: Fur Eltern, Erzieheir, Aerzte und Kunstler* (Stuttgart Ferdinand Enke Verlag, 1904)

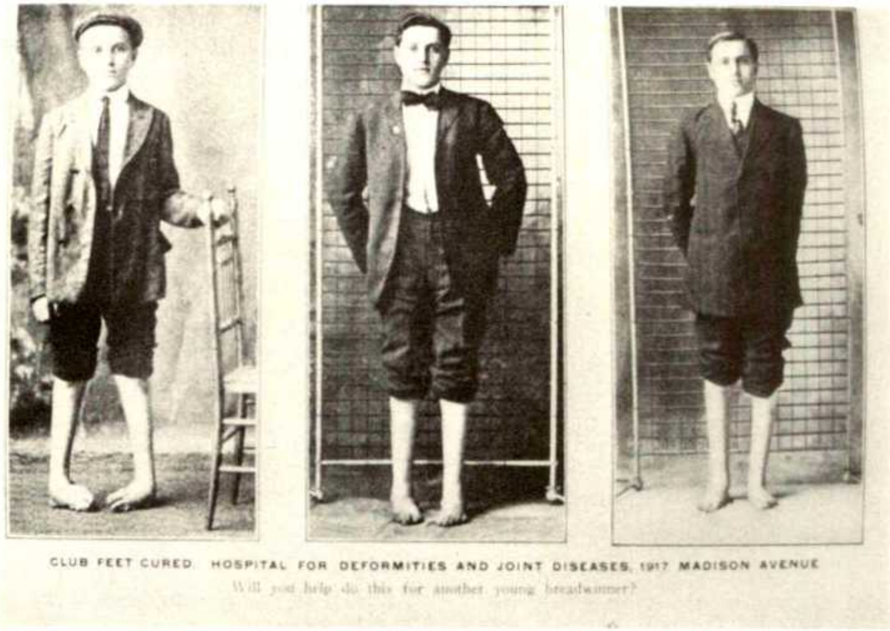
[28] For instance, Waiting Room of Hospital for Deformities and Joint Disease, 1917, UHF Annual Report, Washington, D C , Dispensary, Outdoor Dispensary Patients, 1909, NLM file #WX2AD6C6, neg. #66-444

[29] Karolevitz, *op cit* (n 25 above), *passim*

[30] Irving R Bancroft, Resident Physician, Clinical Records of the Smallpox Hospital, 112 Southampton Street, Boston, 1902, CWY

[31] Boston Lying-In Hospital, 1899, CWY, Manhattan Maternity Hospital and Dispensary, c 1912, UHF Annual Report

[32] A striking example, one of many at the NLM, is of a Leg Probe, Mukden, Manchuria, during the Russo-Japanese War, 1904-05 Russo-Japanese War Album, shot #15. Similar shots are available for the period of World War I


CLUB FEET CURED. HOSPITAL FOR DEFORMITIES AND JOINT DISEASES, 1917 MADISON AVENUE
Will you help do this for another young breadwinner?

Club-Feet Cured
*(Courtesy of United Hospital Fund of New York)*

Although images of medicine became increasingly impersonal during the period, other American photographers were personalizing social problems. Jacob Riis' photographs of slum dwellers aroused international attention and concern in the 1880s and 1890s.[33] A few years later, Lewis Hine's compassionate portraits assisted reformers crusading against child labor and substandard housing.[34]

Most medical photographs, in contrast, lacked explicit social messages. Some pictures were sentimentalized to stimulate philanthropy.[35] Other illustrations of health care for the poor were didactic. In one series, a young man with club-foot displayed increasingly better tailoring as his handicap was corrected.[36] Many photographs documented what written sources tell us was a different standard in health care offered to the poor. For instance, poor patients were often treated in public clinics, in full view of other waiting patients.[37] The invasion of the privacy of the poor can, however, provide insight. One notable example is a photograph of a venereal disease clinic in Arkansas in the 1920s. Patients received treatment in random racial order.[38] Despite official segregation, the medical image transmitted by the United States Public Health Service, at least in this instance, was colorblind.

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[33] Szasz and Bogardus, *op. cit.* (n. 6 above). Many of Riis' photographs are at the New York Public Library and the Museum of the City of New York.

[34] Hine's work has become increasingly accessible in the past several years. The best single source is *America and Lewis Hine: Photographs, 1904-40* (New York: Aperture, 1977). An interpretative essay in this catalogue by Alan Trachtenberg sets Hine's work in context.

[35] For example, many of the photographs in the annual reports of the Hospital Saturday and Sunday Association, later the United Hospital Fund.

[36] "Club Feet Cured," 1912, UHF Annual Report.

Without written evidence historians cannot describe with confidence the feelings evoked by images. But some photographs invite speculation about the feelings they may have evoked because they suggest insights in the imaginative and autobiographical literature of the period. Three examples illustrate this point. A photograph of William Osler on rounds at the Johns Hopkins Hospital, surrounded by students and colleagues, first attracted our attention because it was taken from an unusual perspective; bed height across the ward, with the action in the center toward the rear and the foreground empty. The photograph could have been taken from a patient's bed on the opposite side of the ward. The cluster of men around the bed, observing and discussing intently, could have evoked curiosity, or apprehension, or hope in patients who observed it from a similar perspective.[39]

The other examples are drawn from the Public Health Service collection of photographs of arriving immigrants. Medical inspection was frightening; it could lead to quarantine or return to the country of origin. A photograph of Asian males being examined in a group by a portly, sharply uniformed jackbooted physician stated a sharp contrast in status and power.[40] A subtler point was made by a photograph taken on Ellis Island of a young half-undressed woman being examined by a woman physician and a nurse. The examination was witnessed by older immigrant women, still clothed according to the conventions of European villages. The shock of the young woman being touched by an older woman in public may have accounted for the looks we suspect are of bewilderment mixed with fear.[41]

Some photographs provide strikingly different views of the same event or individual. Medical students, for example, apparently posed smiling or

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[37] For instance, Oregon University Medical School, Dispensary, c. 1910, NLM file #W19A07P8U5 and Volunteer Hospital, Emergency Room, c. 1912, UHF Annual Report.

[38] USPHS VD Clinic, Hot Springs, Arkansas, National Archives, Records of the Public Health Service, Box 9 90-G-654 (NA).

[39] Osler Rounds on Wards, c. 1890, Cushing Collection, Yale.

[40] Angel Island, Physical Examination, Box 22 90-G-22C-8, NA.

[41] Ellis Island, c. 1907, NA. Photograph available only from Brown Brothers.

Inspecting Immigrants Angel Island, California
*(Courtesy of National Archives)*

clowning with their cadavers as a matter of ritual from the 1890s on.[42] A rare photograph of a gross dissection laboratory at a major medical school of the period indicates that the cadavers were as impersonal and mechanical as they are today.[43] The poses may have been efforts to humanize an unpleasant confrontation. Although most photographs of well-known physicians were formal, a few suggest a different image. Two photographs of William Osler working at his desk in Baltimore exemplify the difference between cultural ideals and actual images. In one, Osler was in shirt sleeves, surrounded by a clutter of books and papers. In the other, dressed in a suit and staring into the distance beyond the viewer, he assumed the pose common to portraits of business and professional men of the period.[44]

A few photographs make strong aesthetic statements, although we do not know how consciously the photographers planned to stimulate strong feelings in viewers. The aesthetic value of some images is their capacity to shock; for instance, photographs from the First World War of a nurse

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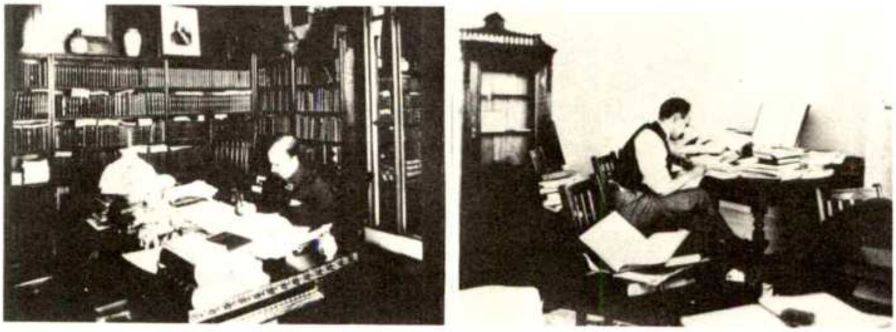
[42] Students with Cadaver, Harvard, 1905, CWY; Dissection Class, Pennsylvania Women's Medical College, 1890-1892, neg. #72-10355, Smithsonian.

[43] Dissecting Room, Rush Medical College, Chicago, 1900. Classified File #8, WHC (x3) 26036. State Historical Society of Wisconsin. Courtesy of Dr. Guenther Risse.

[44] Both photographs, from the late 1890s, are in scrapbooks of the Cushing Collection at Yale.



Two views of William Osler at work in his study
*(Courtesy of Yale Medical Library)*

bathing the eyes of a gassed soldier and of a double amputee writhing in agony.[45]

Other photographs are arresting because they are unusual. For instance, an unexpected view of the rounds of a Visiting Nurse, short-cutting across New York tenement rooftops, conveys an exhilarating sense of perspective, motion, and a previously unsuspected view of a woman at work.[46]

Photographs can compel attention to interesting people and settings. In a photograph of three levels of a nursing hierarchy, facial expressions approximate rank.[47] A photograph of staff serving a ward at the Boston City Hospital possibly joined by a few favored patients seems consciously posed to suggest an extended family.[48] A glass-plate photograph of a nurse at the Massachusetts General Hospital holding a small child highlights an archetypal human relationship in the impersonal setting of a modern hospital.[49]

The images of surgery, more than of any other aspect of medicine, suggest the increasing visibility of a new culture of medicine. In Civil War surgery, as on most occasions when surgery has been performed over the past several thousand years, spectators and surgical assistants were undifferentiated.[50] Like executions and, for much of history, war, surgery was a public event. Photographs of Civil War field surgery are similar in composition and perspective to the well-known Mathew Brady photographs of the execution of Lincoln's assassins.

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[45] Field Hospital #326, Royamieux, France, 1918 Neg #58-381; Double Amputee, n.d. file #W0700 C25, both in NLM.

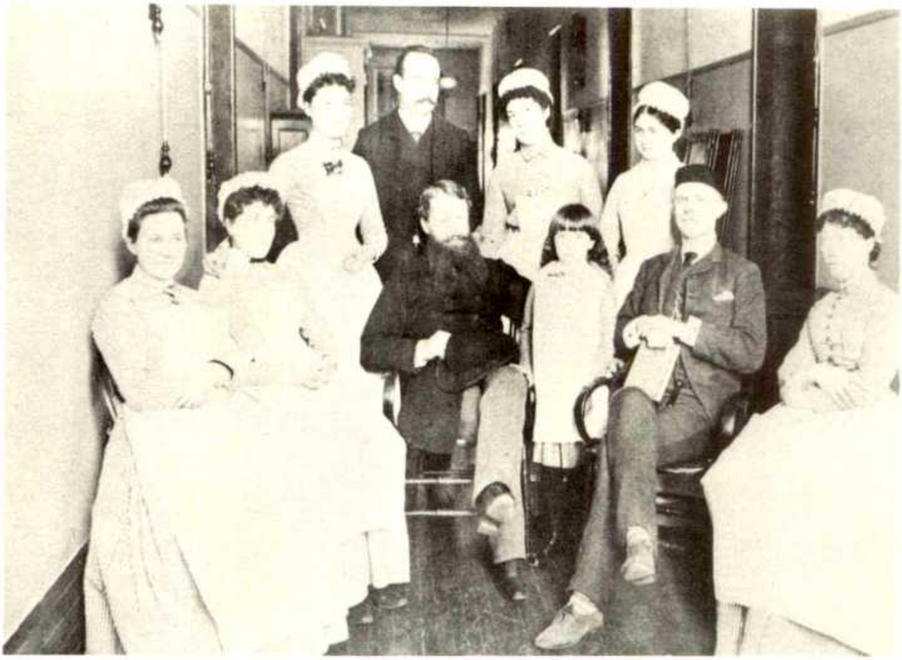
[46] Nurse Cutting Across Rooftops, c. 1910, Visiting Nurse Service of New York.

[47] Head Nurse, Probationer, Pupil, Boston City Hospital, 1898, from Hodson, *op. cit.* (n. 21 above).

[48] Ward L. Boston City Hospital, n.d., CWY.

[49] Massachusetts General Hospital, Glass Plates c. 1900 in drawer 43-429, Smithsonian.

[50] Unidentified Civil War Surgeon Operating, NLM neg. #61-100.



Staff of Ward L, Boston City Hospital
*(Courtesy of Countway Library)*

The images of modern surgery were visible by the 1870s. The surgical amphitheater had become conventional in teaching hospitals. In a photograph of an amphitheater at Massachusetts General Hospital in the 1870s things and people have familiar relationships, except for slop buckets in the foreground and a retiring nurse near the exit.[51] Twenty years later, a photograph of surgery at Yale differs from later images only by a portrait on the wall of the operating room.[52] Nurses had now joined what had been an exclusively male group around the surgeon.

Photographs of surgery were commonplace by the turn of the century. The most frequent shot posed staff and equipment. Sometimes changes in equipment and procedures can be documented by comparing photographs taken years apart in the same hospital.[53]

Surgical photography became increasingly stylized. Pictures of what was

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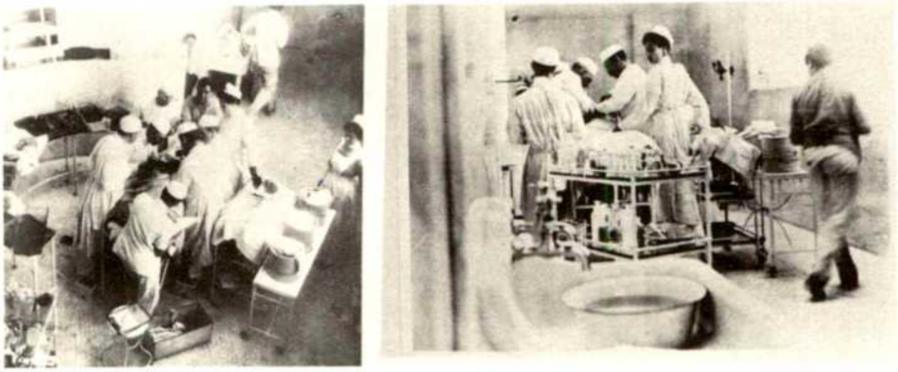
[51] Massachusetts General Hospital, Abdominal Ward, n.d., CWY.

[52] Yale-New Haven Hospital, Radical Mastectomy, 1894, NLM neg. #67-271; also Yale.

[53] Operating room, German Hospital, now Lenox Hill Hospital, New York City, 1902, neg. LH8P 72-8981; same operating room, 1917, LH10P 72-8983, Smithsonian.



"The All-Star Operation"
*(Courtesy of Yale Medical Library and National Library of Medicine)*

called the "All-Star Operation," celebrating the opening of a new amphitheater at the Johns Hopkins in 1904 are, as far as we know, the first series of photographs taken as a contribution to the history of medicine. Letters in the Harvey Cushing Collection discuss the personnel and setting. Although the most famous photograph in the series conveys what had become the conventional perspective in modern surgery, with the audience looking down at the surgeons, other shots, relatively rare in twentieth-century medical photography, show the operation from an eavesdropping perspective. [54]

The modern images of surgery were clearly established by the First World War. Surgery had changed rapidly in just a few decades. Once the most public of medical skills, surgery became in a few decades a ritual open only to the elect. Like war, surgery would not become a public event for the general public again until the 1960s and 1970s when live television intruded into both the battlefield and the operating room.

A comparison between two photographs summarizes the themes we have inferred. One photograph depicts an operation at Bellevue in the 1870s; the other, surgery at New York Hospital, taken by the eminent artist-photographer, Edward Steichen in 1929.[55] In the 1870s, surgery was performed in a ward, amid other patients and familiar objects. The photographer communicated no particular didactic or dramatic purpose. In the Steichen photograph, the images are scientific, detached, efficient, and quietly dramatic. The Steichen photograph was taken at the request of an advertising agency, to promote the hospital. By becoming art and advertising, the medical image became common cultural property, accessible to both physicians and laity.

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[54] All-Star Operation, 1904, NLM neg. #67-74 and 67-75. Also Cushing Collection, Yale. Extensive description of the occasion and anecdotes about events preceding, during and after the photograph can be found in correspondence in the Cushing Collection at Yale: Harvey Cushing to George Smith, December 13, 1937; Hugh Young to George Smith, December 1, 1938; John M. T. Finney to George Smith, December 1, 1937.

[55] The Bellevue photograph is reproduced as the cover of *1199 News*, September 1976. The most accessible source for "Death Takes a Holiday" is Edward Steichen, *A Life in Photography* (New York: Doubleday, 1963).

Our inquiry into the use of photographs as sources for the study of the history of medicine thus leads to a broader generalization than we had anticipated. We conclude that the self-image of medicine had become its public image by the second or third decade of this century. Put another way, we hypothesize that the image physicians had of themselves, an image associated with an increasing sense of competence grounded in the findings of biological and clinical science, became the image the public had of physicians.[56] This transformation was not, of course, mainly a result of photography. Journalism, popular literature and, probably most important, patients' contacts with medical care, created the popular image of medical efficiency and efficacy. But photographs, and later movies and television, focused, communicated and legitimized images which were considerably more diffuse in actuality. The public image of medical competence, which was at least reinforced by photographs, has led to high expectations of medical achievement, and to bitterness and anger when these expectations are not realized.

Visual images can never communicate the full complexity of people and events. Our exploration is tentative. We worked with photographic sources to increase our understanding of the past. This effort to systematize our inquiry is a result of our conviction that sources worth using are worth using carefully. If our methods are of value it will only be because they provide additional—and not substitute—tools for historians. Our inferences can only be worthwhile if they are supported by other historians using different sources.

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[56] By the 1920s photography had become a metaphor for both reality and reform, for is and ought. Thus, in December, 1921, in a form letter requesting information from leading medical educators, Abraham Flexner wrote, "I am trying to assemble a composite photograph, so to speak, of what a modern medical school ought to be." Rockefeller Foundation Archives, Tarrytown, New York, Foundation Papers Box 711.

# *MEDICO HISTORICAL NEWS AND ACTIVITIES*

## CORRESPONDENCE AND REPORTS

### ENCOMIUM

### WALTER PAGEL'S EIGHTIETH BIRTHDAY

On Sunday, 12 November 1978, Professor Walter Pagel received congratulations on his eightieth birthday. A splendid festschrift in celebration of his seventy-fifth was reviewed in the *Bulletin* five years ago[1] and his scholarly achievements were given editorial recognition. On both sides of the Atlantic he was generously and warmly acclaimed. The years since then have confirmed his reputation and added to his laurels. But the golden opinions have not been unqualified. His particular interest in hermetic and other "far-out" sectors of Renaissance thought has seemingly caused him at times to identify (almost) with strange and stranger concepts and in consequence has occasionally led to complaints of *obscurum per obscurius*. ("I am a pilgrim in the land of otherness," he has said.) His sympathetic, learned, penetrating studies have, however, drawn these sectors in from the periphery of scholarly concern, and if his writings have not fixed them in its center, what at any rate seems sure is that they will not again drift from notice nor fail of serious assessment. Besides, if Pagel be properly understood, these elements will not be gilded and distorted. Like other parts of the complex pattern of thought in a world we never made they will fit into appropriate place. With them, shaped and colored by their nearness, will be the standard elements of the history of thought which scholars have long before this apprehended but in ways that will be a little bit different because Walter Pagel has seen them differently. Like other historians he has filled gaps in our information about the past and has added in some measure to our understanding of it; beyond this, and in the company of a smaller group, he has changed, and continues to change, the very character of our perception. There are young scholars both in Europe and in America who are pursuing fifteenth, sixteenth, and seventeenth-century medicine in ways often unlike Pagel's but who belong, nevertheless, to the eras shaped by him—the ones which he and they study and the one in which he and they think and write. It is unsurprising that the best of them are happy to acknowledge the debt.

Four score, Walter! The young scholars, I feel sure, will "see you and raise you." What do you say to five score?

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[1] Allen G. Debus (Editor), *Science, Medicine and Society in the Renaissance:* Essays to honor Walter Pagel, 2 vols. (New York: Neale Watson Academic Publications, 1972). Reviewed by Owen Hannaway, *Bull. Hist. Med.*, 1973, *47:* 637-640.

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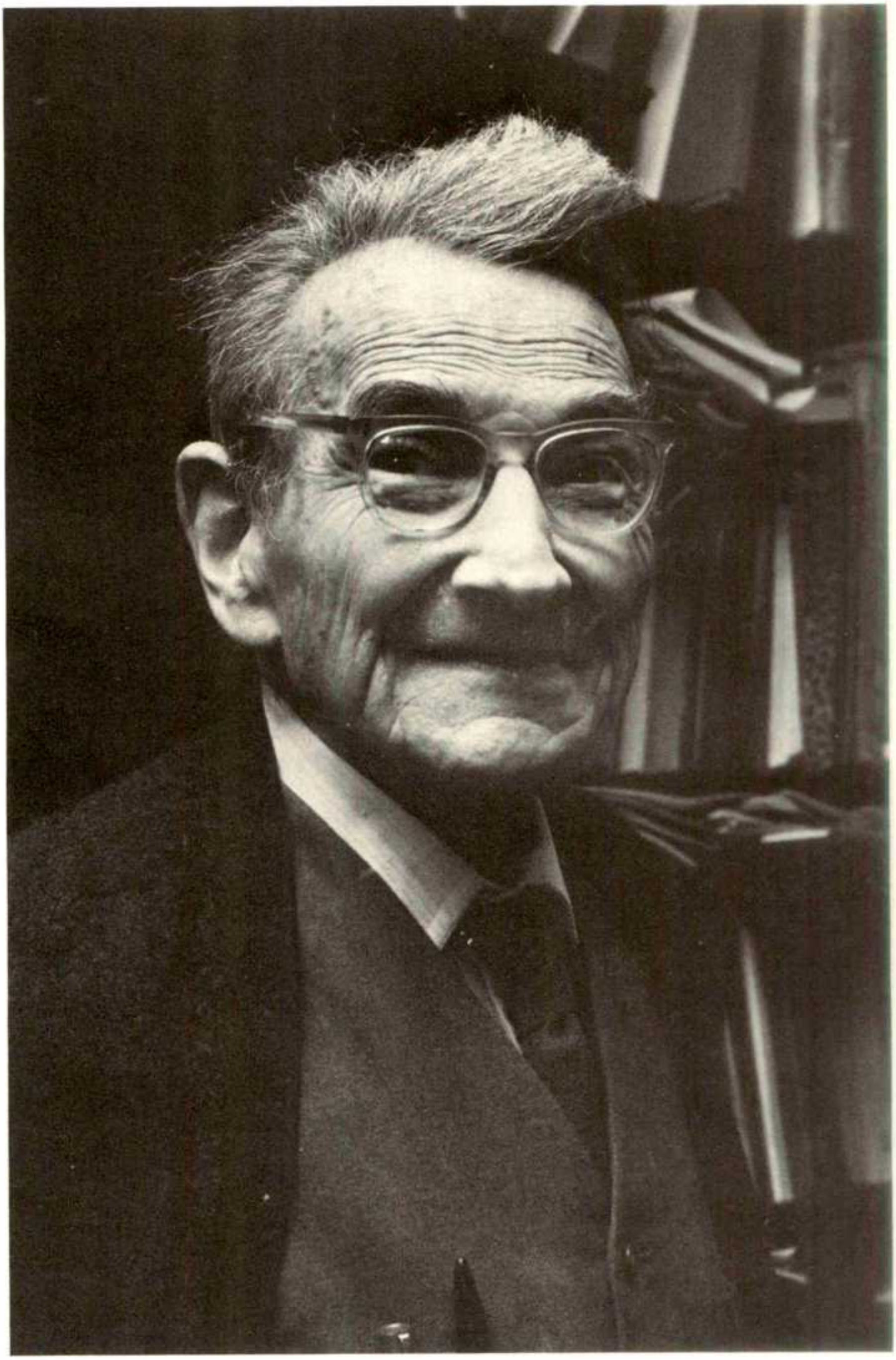

Dr. Walter Pagel

# INTERNATIONAL SOCIETY FOR THE HISTORY OF MEDICINE

During the past year, the International Society has fulfilled its role as organizer of biennial international congresses and link between historians of medicine on the international level. The Council of the Society met in Paris on June 4, 1977, and on June 3, 1978, and in Plovdiv, Bulgaria, on August 21, 1978. The American Association for the History of Medicine was officially represented by Mrs. Ruth Champagne, Dr. Caroline Hannaway, and Dr. Genevieve Miller, *seriatim*.

The ISHM now consists of 592 members, from 44 nations, with the four strongest contingents from the USA (88), Italy (72), France (54) and the Federal German Republic (40). The Society's assets stand at French francs 18,736; annual dues are Fr. f. 40. The Council is composed as follows: *President*, de la Broquerie Fortier (Canada); *Honorary Vice-President*, Franz-André Sondervorst (Belgium); *Vice-Presidents*, Luigi Stroppiana (Italy), Dora B. Weiner (USA), Farid Sami Haddad (Lebanon), Jean-Charles Sournia (France); *General Secretary*, Louis Dulieu (France); *Treasurer*, Isidore Simon (France).

A report on the Society's activities since the Quebec Congress in August 1976 was presented by Secretary General Dulieu at the Plovdiv Congress on August 24, 1978. Dr. Dulieu saluted Dr. Fortier's accomplishment in publishing three volumes of *Proceedings* of the Quebec Congress; he announced the forthcoming publication of an up-to-date membership directory; he thanked national publications such as the *Bulletin of the History of Medicine* for announcing ISHM activities; he especially welcomed publication of the *ISHM Bulletin* in bilingual format; and, lastly, he announced the place, date and themes for the next International Congress.

The 27th International Congress for the History of Medicine will be held in Barcelona, Spain, September 7-13, 1980. The following topics have been chosen for the program: The History of Psychiatry and of Psychoanalysis; The Influence of Iberian Medical Emigrants; The History of Medical Translations; Avicenna (millennium); The Development of Naval Medicine; Varia. Questions about the Congress can be addressed directly to: 27th International Congress for the History of Medicine, Academia de Ciencias Médicas de Cataluña, Paseo de la Bonanova 47, Barcelona 6, Spain.

DORA B. WEINER
*Vice-President, ISHM*

# APPOINTMENTS IN THE HISTORY OF MEDICINE

*Memorial University of Newfoundland*

A named professorship has been established at Memorial University of Newfoundland to be known as the John Clinch Professorship in the History of Medicine and Physiology. The professorship is named for Dr. John Clinch, the Gloucester physician-clergyman, who died in 1819. He had lived 36 of his 70 years in Newfoundland, mainly at Trinity. At the turn of the 19th century Dr. Clinch vaccinated against smallpox in Trinity, Portugal Cove and St. John's, thereby introducing to the New World the discovery of his compatriot, colleague and friend, England's Edward Jenner.

Dr. K. B. Roberts, Professor of Cell Sciences and Physiology in the Faculty of Medicine at Memorial, has been named first John Clinch Professor. Dr. Roberts, who has taught physiology and experimental pathology for thirty years, obtained his medical degrees in 1945 from the University of London and his doctorate in philosophy at Oxford. He has held teaching posts at the University of Edinburgh, the Medical College of Bagdad, and Oxford University. Prior to his appointment at Memorial he was for seven years Reader in Physiology at the University of London.

Dr. Roberts was the first Associate Dean of Medicine in Memorial's new Medical School established in 1967. He joined Memorial part-time that year and full-time in September 1968. His inaugural lecture as the John Clinch Professor of the History of Medicine and Physiology was given on October 30, 1978.

*McGill University*

Dr. George Weisz, recently of Paris and formerly of Montreal, has been appointed Assistant Professor of the Social History of Medicine in the Department of the History of Medicine, at McGill University. Dr. Weisz received his Ph.D. from State University of New York at Stony Brook in 1976, his doctoral dissertation having been on the subject of higher education in France, 1850-1885. During his years of research in France, Dr. Weisz concentrated increasingly on medical education and the development of the medical profession in France during this same period.

Dr. Weisz will be responsible for teaching the comparative social history of medicine in western European countries in the 19th and 20th centuries and plans to extend his research interests to include the development of the medical profession in the Province of Quebec. In joining other historians, a medical sociologist, and a medical anthropologist, Dr. Weisz will help to fulfill the wider role of the Department of the History of Medicine in teaching the social dimensions of medicine in McGill's Medical Faculty.

# ANNOUNCEMENTS

*American Historical Review*

The *Review* is interested in expanding its file of book reviewers in the history of medicine. The editors invite scholars with appropriate credentials in this field to write, requesting reviewer cards, to *American Historical Review*, Ballantine Hall 721, Indiana University, Bloomington, Indiana 47401.

*Center for Photographic Images of Medicine and Health Care*

The National Endowment for the Humanities has awarded a grant to the Health Sciences Center, SUNY at Stony Brook, to support a Center for Photographic Images of Medicine and Health Care for the next three years. The Center will collect and catalogue photographs bearing on the history of American medicine. Slide reproductions of the photographs will be available at cost, either individually or in packets designed to explore a particular health care theme. The Center intends to advance the uses of photographic images for both teaching and research in the history of medicine. Small development grants will be offered to teachers to collect and organize groups of photographs around a theme in medical history suitable for undergraduate, professional, or continuing medical education audiences. For additional information contact the Center for Photographic Images, Department of Community Medicine, Health Sciences Center, State University of New York, Stony Brook, N.Y. 11794, or the members of the Project Committee: Daniel M. Fox and James Terry, Health Sciences Center, Stony Brook; Judith Leavitt and Guenter Risse, University of Wisconsin, Department of History of Medicine; Martin Pernick, The Milton S. Hershey Medical Center, Department of Humanities.

*Johns Hopkins Institute of the History of Medicine*

The 21st series in the Hideyo Noguchi Lectureship was presented October 4-6, 1978, by Dr. Jean Starobinski, Professeur ordinaire, Histoire de médecine, and Professeur ordinaire, Langue et litterature françaises, Université de Geneve. The lectures, on "The Sense of the Body (A History of Coenesthesia)," were titled individually: "Reil's Coining of Coenesthesia," "Coenesthesia and the Physiological Definition of Personal Identity (Schiff, Ribot, Wernicke, Dupré)," and "Coenesthesia and Language. (a) Blondel's Theory of Morbid Consciousness. (b) Bodily Sensation in Modern Writers."

*National Library of Medicine*

HISTLINE (History of Medicine Online), NLM's newest computerized data base, began operation on October 1, 1978. It joins MEDLINE and the other online bibliographic files of the NLM network. HISTLINE contains citations to medical historical literature, most of it published between 1970 and 1978. The data base, which is updated monthly, is expected to grow by some 5,000 citations a year. The new file contains references to current monographs, journal articles, symposia and similar composite publications. It provides an expeditious and expanded facility for searching literature which is subsequently cited in NLM's annual *Bibliography of the History of Medicine*. The Bibliographic information for HISTLINE is collected and processed by NLM's History of Medicine Division.

*New Jersey Pharmaceutical Association*

The Association has organized the Pharmacists Institute of New Jersey which will establish in the Trenton Historic District a pharmacy museum, a circulating library, and provide educational programs. The Institute's premier lecture, "Poster Art and Pharmacy," was presented by William H. Helfand, Sr., Vice President of Merck, Sharp and Dohme. Mr. Helfand, Past-President of the Institute of the History of Pharmacy, is a collector of prints relating to medicine and pharmacy

*Morris H. Saffron Collection of Historical Medicine*

On October 18, 1978, a collection of books on historical medicine previously owned by the Academy of Medicine of New Jersey was formally presented to the George F. Smith Library of the College of Medicine and Surgery of New Jersey in Newark. Notable volumes in the collection, which has been growing since 1911 when the Academy of Medicine was founded, are Renaissance medical classics and eighteenth and nineteenth-century editions of continental and American authors. It is now shelved in a special rare book room and named in honor of Dr. Morris H. Saffron, Visiting Professor of the New Jersey Medical School, who for many years was Chairman of the Library Committee of the Academy and helped to develop the collection. At the opening of the Seton Hall College of Medicine (now the New Jersey Medical School), Dr. Saffron was appointed Professor of Medical History and delivered the first formal series of lectures on this subject in New Jersey Dr. Stanley S. Bergen, President of the College of Medicine and principal speaker at the presentation, emphasized the importance of the collection and the contribution of Dr. Saffron in fostering the humanistic interests of medical students.

*Toronto*

The Hannah Institute for the History of Medicine has announced plans for the compilation of a directory of medical archives in the Province of Ontario. Miss Margaret Dunn, who has been appointed Archives-Researcher for the Hannah Institute, will be responsible for the compilation of the directory It is proposed that the information be collected and recorded in a manner compatible with modern technological procedures for storage and retrieval of such data. The Institute will not become an archival repository but will provide information on the location and content of medical archives in Ontario. Valuable cooperation has already been given by various Ontario institutions and the Institute hopes for the support of the medical profession and the general community as well.

*Awards*

At the meeting of the History of Science Society in Madison, Wisconsin, Allen Debus and Merritt Roe Smith shared the Pfizer Prize which is awarded for books judged the best volumes on the history of science published in the past year. Dr. Debus, Morris Fishbein Professor of the History of Science and Medicine at the University of Chicago, was honored for his two-volume publication, *The Chemical Philosophy: Paracelsian Science and Medicine in the Sixteenth and Seventeenth Centuries*. Dr. Smith of MIT received the award for his *Harpers Ferry Armory and the New Technology: The Challenge of Change.*

On June 17, 1978, in Orinda, California, Dr. George P. McGovern was awarded the honorary degree of Doctor of Science from John F. Kennedy University, which is an accredited, independent university, designed primarily to meet the needs of older adult students who want to expand their education. Dr. McGovern is Director of the McGovern Allergy Clinic and Chairman of the Texas Allergy Research Foundation. Among other activities he currently serves as Professor of the History of Medicine and of Allergy at the University of Texas Graduate School of Biomedical Sciences in Houston.

## PROGRAMS OF MEETINGS

*George W. Corner History of Medicine Society, Rochester, N.Y.*

October 3, 1978: "Bonesetting Families," James Cyriax.

*Handerson Medical History Society, Cleveland, Ohio*

October 10, 1978: "The History of Artificial Organs," Yukihiko Nose.

*Josiah C. Trent Society for the History of Medicine, Durham, N.C.*

October 13, 1978: "Collecting Medical Books," G.S.T. Cavanagh.

*Mayo Foundation History of Medicine Society, Rochester, Minn.*

October 11, 1978: "Problems of Medicine at Sea," Stanley A. Lovestedt.

*Society of Medical History of Chicago*

October 26, 1978: "Henry Hill Hickman," William Dennis Ashley Smith.

*University of Virginia Medical History Society, Charlottesville*

October 18, 1978: "Hospital Infections: Some Remarks on Their Recent History," Harry F. Dowling.

*Washington Society for the History of Medicine, Washington, D.C*

September 28, 1978: "The City Hospital and the Medical Schools: Parasitism or Symbiosis," Harry F. Dowling; "Historical Attitudes toward the Gout," Zelda Taplitz.

## JOB OPENINGS

*Queen's University, Kingston, Ontario*

Queen's University invites applications for the Hannah Professorship in the History of Medicine from scholars with experience in this field and preferably physician-historians with experience in contemporary medicine in North America. The successful candidate will receive an appointment in the Faculty of Medicine but must be eligible for a cross-appointment in the Department of History. Duties will primarily involve teaching in the Faculty of Medicine but participation in the programmes of the Department of History will be expected.

Applications or inquiries should be directed to: Dr. T.F. McElligott, Associate Dean, faculty of Medicine, Queen's University, Kingston, Ontario K7L 3N6, Canada.

*University of California, San Francisco, Calif.*

The University of California is seeking an Assistant Professor for the Department of the History of Health Sciences in the School of Medicine. This tenure track position in the history of medicine will become available as of July 1, 1979. Faculty members share in the teaching of undergraduate elective courses and graduate seminars in this small department. Applicants should have a Ph.D. degree in the history of health sciences and a broad background in the history of medicine Research interests should be in the history of European or Asian medicine. Send inquiries and resumés to Professor G. H. Brieger, U-406, University of California, San Francisco, Calif. 94143. The University of California is an affirmative action-equal opportunity employer.

# BOOK REVIEWS

L. R. Lind, *Studies in Pre-Vesalian Anatomy. Biography, Translation, Documents.* Philadelphia, American Philosophical Society. 1975. ix + 344 pp Ill $18 00.

Professor L. R. Lind has labored long and hard in the field of the early history of anatomy. Tangible evidence of his scholarship and dedication to this subject of inquiry is readily available in the several renderings of anatomical writings and his commentaries upon them that have been published during his long and fruitful career. Two notable examples of those efforts are his versions of Vesalius' *Epitome*, and of Berengario da Carpi's *Isagogae brevis*.

In 1949, Professor Lind prepared an English version of the so-called though misnamed *Epitome* of the *Fabrica*, and by so doing helped to advance its author's original intention for the work Vesalius designed the brief composition for the instruction of students, and even today, owing almost entirely to Professor Lind's diligence in translating and editing the elementary Vesalian text, the composition, in its modern edition is still being used in medical history classrooms. Some ten years after the publication of his edition of the *Epitome*, Professor Lind brought forth an English version of the concise dissection manual known as the *Isagogae* which Berengario da Carpi, Vesalius' Bolognese predecessor, composed and published in 1522. As the first English translation of the *Isagogae* to appear in print since the seventeenth century, Lind's version affords yet another valuable instrument in the armamentarium of those who seek to probe into the origins of the Vesalian revolution.

These earlier editions notwithstanding, Professor Lind's most significant contribution to the study of the early development of the anatomical sciences in Western Europe will undoubtedly be his most recent publication. The product of over a decade of intense research, this work will surely become an indispensable source for data concerning the history not only of anatomy but also of all the biomedical sciences during the sixteenth century. By supplying the English reading public with selections drawn from the writings of eight pre-Vesalian anatomists, Professor Lind has thrown open doors formerly held shut to scholars and students alike, and has set the stage for new, innovative assessments of the role played by those investigators in the development of anatomy as a science.

The eight anatomists chosen for consideration by Professor Lind are Alessandro Achillini, Alessandro Benedetti, Berengario da Carpi, Gabriele Zerbi, Niccolò Massa, Andrès de Laguna, Johannes Dryander, and Giovanni Battista Canano. Each section of the book consists of a sketch of the principal events in the lives of these men, a discussion of their work, a list of their published writings, and, when available, a facsimile of their likenesses Only the depiction of the unfortunate Zerbi is missing from Professor Lind's portrait gallery. The most significant portions of his book, however, are his renderings of selected samples from the investigator's anatomical writings.

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Versions of the entire text of four of these writings are presented, including Achillini's *Annotationes anatomicae*, Massa's *Liber introductoris anatomiae*, de Laguna's *Anatomica methodus*, and Dryander's *Anatomia capitis humani*. Summaries of five medical works composed by Zerbi, and of Berengario da Carpi's *Commentaria . . . super anatomia Mundini*, as well as renderings of Books I, III, and V from Benedetti's *Anatomice sive historia corporis humani*, and of Book I of Canano's *Musculorum humani corporis picturata dissectio* complete the main body of Professor Lind's anthology. In its Appendix, reproduced but not rendered from the Latin, five letters written by Benedetto Rizzoni to Zerbi are printed, as are the latter's will, and also the last testaments of Benedetti and Massa.

To preface the eight principal sections of his book, Professor Lind sets forth a general history of pre-Vesalian anatomy. Comprising a review of the historical and cultural background in which anatomy developed during the sixteenth century, a brief account of the investigators Professor Lind considers as the precursors of the pre-Vesalians, a consideration of those investigators themselves, and an explanatory evaluation of their place in history when compared to the historical status accorded Vesalius. Although much of this material is both necessary and enlightening, certain of Professor Lind's constructions demand questioning.

In light of modern scholarship, his claim that Mundino was the first European to dissect the human body publicly can scarcely be maintained. The novelty of some important Galenic writings to early sixteenth-century anatomists is a fact virtually overlooked by Professor Lind. Finally, the omission of a crucial initial statement in a passage by Lynn Thorndike cited by Professor Lind on p. 7 of his present work alters Thorndike's intended meaning and distorts his thesis concerning the origins of European anatomical investigation.

Despite these minor imperfections, Professor Lind's work can certainly be recommended to any reader interested in exploring the development of the biological sciences. An invaluable source for information on the lives and works of some of the men who investigated the structure of the human body during the first half of the sixteenth century, the book can also serve as a guide to the study of the society in which they lived. Copiously annotated, carefully indexed, and provided with a bibliography of sources, this excellent work's additional merit must not go unmentioned. Unlike many other important and well printed authoritative texts, this one is well worth its modest price. Therefore, both its author and its publishers are deserving of high commendation and grateful acknowledgment.

Reviewed by YNEZ VIOLÉ O'NEILL, Ph.D., Associate Professor, Medical History Division, Department of Anatomy, University of California Center for the Health Sciences, Los Angeles, Calif.

*Women in Medicine: a Bibliography of the Literature on Women Physicians*. Compiled and edited by Sandra L. Chaff, Ruth Haimbach, Carol Fenichel and Nina B. Woodside. Metuchen, N.J.: Scarecrow Press, 1977. 1,124 pp. $35.00.

This is the first comprehensive bibliography on women in medicine. Compiled at the Florence A. Moore Library of Medicine, Medical College of Pennsylvania, in cooperation with the Center for Women in Medicine, it lists in 1124 pages over 4000 items published between 1750 (the earliest item found) and 1975. It includes books, journal articles (from non-medical sources also), dissertations, alumnae and alumni magazine articles and occasional chapters from books arranged within fourteen broad subject categories and subdivided geographically. The citations are numbered and almost all of them are annotated, the annotations ranging in length from a few lines to two pages. In many cases, the annotations are really informative abstracts which makes this bibliography fascinating for browsing as well as a good reference source. Approximately 9 percent of the items listed are in languages other than English but the titles have been given only in English translation.

Over 300 pages are devoted to "Biographies," subdivided geographically. Included are physicians of the 19th and 20th centuries, mostly of the U.S. (200 pages). Women physicians of antiquity must be looked for in the index under "Greece (ancient)," "Egypt (ancient)," "Italy, Rome (ancient)" or under individual physicians' names. There are three citations under Aspasia (2d cent.) in the Personal Name Index. They are included in the "History of Women in Medicine" section under "Europe, Western" and "International." I could find only one of these citations in the Subject Index under "Greece—Native Physicians." "Healers" can be found in the Subject Index only as a subdivision under "Greece (ancient)," "Middle Ages," etc.

There are 75 pages of citations on "History of Women in Medicine," again arranged geographically, with general histories listed under "International" and ancient Greece and Rome under "Europe, Western." There are almost 70 pages on Medical Education (including graduate education) covering the 19th and 20th centuries. Another 70 pages cover Missionary and Wartime Activity. Over 100 pages cover "Psychosocial Factors:" literature on the social and cultural attitudes affecting women in medicine. A section on "Fiction," mercifully brief, contains such gems as Seifert's *Girl Intern*. ("I don't care about being a doctor" she tells him. "I'm most glad about being a girl . . . and that you're a man.") The rest of the sections are entitled "Medical Activity," "Specialties," "Medical Institutions, Societies, and their Journals," "General" and "Nonmedical Activity."

There are three indexes: author, subject, and personal name. It is difficult to find information on physicians of antiquity and the Middle Ages through the subject index unless one knows the physicians' names. These citations, however, seem to be concentrated mainly in the section on "History of Women in Medicine." The Personal Name Index is particularly good, listing individually the physicians in collective biographical articles, etc. Often these physicians are not named in the annotations.

Some of the subject boundaries are not quite clear—e.g., there are sixteen citations on midwifery but not Thomas Forbes' *The Midwife and the Witch* published in 1966. Was this an oversight?

Over 95 per cent of the items listed have been examined by the compilers. This should mean that at least that many citations are correct, for which librarians will be grateful. The only errors I could find were in the annotations—e.g. No. 1977: the gift to Johns Hopkins Medical School was 500,000 dollars, not 100,000. One important fact that must be mentioned is that most of the items listed in this bibliography have been collected and are now housed in the Library of the Medical College of Pennsylvania.

It is understandable that in a project of this size, there was no attempt to evaluate the items listed. In doing an exhibit on women in medicine a few years ago, I discovered that of the books published up to then, most ranged from not very good to just plain bad. Users of this bibliography should be aware of that Biographical dictionaries were not included but the excellent articles in *Notable American Women* should have been mentioned.

To sum up: (1) The subject boundaries should have been defined more precisely in some areas (2) Foreign titles should have been given in the original as well as in English translation (3) The subject index should be improved to provide better access to physicians before the 19th century. (4) It would be more helpful to have general histories of women in medicine listed at the beginning of that section under "General" instead of under "International." Perhaps a better arrangement would have been: "General," then chronological with geographic subdivisions.

In spite of these minor criticisms, this bibliography is an excellent one which will be an invaluable source for anyone interested in any aspect of women in medicine. For example, anyone with the time to read through the annotations can come up with a list of the first women physicians in almost every country. The compilers are to be congratulated for this tremendous undertaking. One hopes they have enough energy and funds left to provide regular supplements.

Reviewed by DORIS THIBODEAU, M.S., Librarian, Institute of the History of Medicine, Johns Hopkins University, Baltimore, Md.

JOSEPH SCHILLER and TETTY SCHILLER. *Henri Dutrochet (Henri du Trochet 1776-1847): Le matérialisme mécaniste et la physiologie générale.* Paris: Albert Blanchard, 1975. 227 pp. Ill Fr.50

Little has been written on the life and work of the talented French physician turned physiologist, Henri Dutrochet, known to science primarily for his discovery of the phenomenon of osmosis. The Schillers' volume, the first book devoted wholly to Dutrochet, comes as a welcome addition to the literature and as a valuable source for future study on Dutrochet and the development of physiology in the nineteenth century. The authors have gathered a number of unpublished materials relating to Dutrochet including an autobiography and list of works (perhaps written by Dutrochet to serve as a guide for the preparation of the official Académie des Sciences *éloge*); an unpublished memoir dating from the 1840s on the reaction of plants, particularly Chara, to external stimuli; and a series of letters, many of them addressed to Etienne Geoffroy Saint-Hilaire, who acted as Dutrochet's go-between with the Académie des Sciences. In addition, the Schillers have included a brief biography which makes use of new sources, an iconography, an extremely useful series of annotations to the list of Dutrochet's works, and several chapters analyzing Dutrochet's scientific achievements.

Most of the limited historical research on Dutrochet up until now has focussed on his role in the formulation of the cell theory: did Dutrochet publish the essential concepts of the cell theory over a decade before Theodor Schwann, or was Dutrochet deceived by poor microscopes into describing globules that were not really there? The Schillers enter this somewhat fruitless debate on the side of Dutrochet, depicting him as one of the "*frustrés*" of history, a man who failed to receive the recognition that was his rightful due, despite the fact that Dutrochet himself never claimed any credit for the cell theory. Fortunately, the discussion of the cell theory forms only a small part of the Schillers' analysis. Joseph Schiller has written extensively on the history of physiology in the nineteenth century, and in this book the Schillers offer a wide-ranging examination of Dutrochet's work in the context of the development of that science in Europe. Dutrochet, according to the Schillers, was the first physiologist to replace the vitalism of Xavier Bichat with a philosophy characterized as "mechanistic materialism." His belief that the basic phenomena observed in plants and animals are identical, the Schillers assert, laid the basis for general physiology, later elaborated upon by Claude Bernard.

Dutrochet, who worked primarily with plants, insisted from his earliest work that one should not separate plant from animal physiology. Whereas most French naturalists emphasized the differences between plants and animals, Dutrochet was constantly concerned with exhibiting their resemblances. Thus Dutrochet was led to suppose that plants and animals were composed of similar structural elements. In an effort to prove that plants enjoy the same faculties as animals, he conducted significant research on osmosis, response to stimuli, and the movement of fluids in plants. Osmosis, which offered him an explanation for many types of movement in plants and animals, illustrated for Dutrochet the fact that an essentially physical phenomenon might only manifest itself in living tissue. Like Geoffroy Saint-Hilaire, he believed that physics and physiology were inextricably linked. Not only could physical laws explain events in organisms, but phenomena occurring in living matter (e.g. osmosis) might elucidate the laws of physics.

The Schillers aptly convey the innovative nature of many of Dutrochet's experiments on living matter. Not only did Dutrochet regularly employ the microscope, but he also made use of a broad array of instruments and techniques—nitric acid fixation, the osmometer, the eudiometer, the galvanic pile, the pneumatic pump, and thermo-electric devices, to give a few examples. While many of the conclusions that Dutrochet derived from his experiments were somewhat speculative, he appears to have been more open to criticism than many of his contemporaries. More than once he rejected with good spirit his previously cherished theories. His 1837 compilation of earlier memoirs was introduced with the statement: "I consider as null and void all that I have previously published on these matters which is not contained in this collection."

In addition to a tendency to over-modernize the scientific thought of their subject and to be overly concerned with asserting his priorities to scientific discoveries, a few other criticisms may be made of the Schillers' otherwise useful and informative work It seems surprising that although the Schillers cite a wide variety of primary sources in the history of physiology, they cite no recent historical research other than Joseph Schiller's own writings. The Schillers dissect Dutrochet's scientific thought and show how the various aspects of his thought cohere, but they do not sufficiently integrate his thought with their account of his life. Dutrochet presents an interesting case as one of the few scientists of note in France who did not reside in Paris or hold an official position. Although Dutrochet regularly communicated with the Académie des Sciences and was eventually elected a member, he conducted most of his research alone on his family estate in Touraine. What effect, if any, did Dutrochet's isolation have on his scientific career? The Schillers tell us little about how Dutrochet's ideas developed and how they were influenced by his interaction with the French scientific community. These criticisms are meant less to detract from the Schillers' book than to note that a full-scale biography of Dutrochet is still lacking.

Since the manuscript of the Schillers' book was completed in 1973, several excellent articles on Dutrochet have appeared, including Vladislav Kruta, "Dutrochet," *Dictionary of Scientific Biography, 4.* 263-265; John V. Pickstone, "Globules and coagula: Concepts of tissue formation in the early nineteenth century," *J. Hist. Med.*, 1973, *28*: 336-356; "Vital actions and organic physics: Henri Dutrochet and French physiology during the 1820s," *Bull. Hist. Med.*, 1976, *50*: 191-212, and "Locating Dutrochet" (an essay-review of the Schillers' book), *Brit. J. Hist. Sci.*, 1978, *11*. 49-64. This recent work has done much to fill some of the gaps left by the Schillers' study.

Reviewed by Toby Appel, Ph.D., Research Historian, The Charles Willson Peale Papers, National Portrait Gallery, Washington, D.C.

Alma M. Myer. *The Golden Page: A Biographical Novel.* Hicksville, N.Y.: Exposition Press, 1977. 231 pp. $10.00.

This book, contrary to its title page, is *not* a novel. (The publishers added the subtitle without the author's approval.) It is a nonfictional, popular biography of William Beaumont, the nineteenth-century American physician and physiologist. Although not footnoted, the book rests on solid research, particularly in the Beaumont Collection of the Washington University School of Medicine Library; and every quotation, so far as I can tell, is historically authentic. The author, a sister-in-law of the late Dr. Jesse Myer, who wrote the standard *Life and Letters of Dr. William Beaumont*, completed this work over thirty years ago but refrained from publishing it. In preparation for its belated appearance, she neither updated nor revised her original manuscript; thus there are no references to post-World War II scholarship Nevertheless, *The Golden Page* far surpasses Jesse Myer's book for readability and will give devotees of medical biography an enjoyable evening with Dr. Beaumont.

Reviewed by Ronald L. Numbers, Ph.D., Associate Professor of the History of Medicine, University of Wisconsin, Madison, Wis.

## THE BULLETIN RECENTLY RECEIVED*

William Andrews. *The Doctor in History, Literature, Folk-Lore, etc.* Detroit: Gale Research Co., 1970 (Facsimile reprint of 1896 edition). 287 pp. Ill. $12.50.

Nancy Disher Baird. *David Wendel Yandell, Physician of Old Louisville.* Lexington: University Press of Kentucky, 1978. xi + 116 pp. Ill. $4.95.

Katherine Traver Barkley. *The Ambulance. The Story of Emergency Transportation of Sick and Wounded through the Centuries.* Hicksville, N.Y.: Exposition Press, 1978. 207 pp. Ill. $8.50.

Paul F. Basch. *International Health.* New York: Oxford University Press, 1978. vi + 380 pp. $16.95; prepaid $9.95.

Vern and Bonnie Bullough. *The Care of the Sick: The Emergence of Modern Nursing.* New York: Prodist, 1978. 311 pp. $15.00.

Hamilton Cravens. *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy, 1900-1941.* Philadelphia: University of Pennsylvania Press, 1978. xvi + 351 pp. Ill. $17.50.

*Culture and Healing in Asian Societies. Anthropological, Psychiatric and Public Health Studies.* Edited by Arthur Kleinman and others. Boston: G. K. Hall, 1978 (University Books). 462 pp. $22.50.

Allen G. Debus. *Man and Nature in the Renaissance.* Cambridge-New York: Cambridge University Press, 1978 (Cambridge History of Science Series). x + 159 pp. Ill. $15.95 (hardcover); $4.95 (paperback).

Rudolf E. A. Drey *Apothecary Jars. Pharmaceutical Pottery and Porcelain in Europe and the East, 1150-1850.* With a Glossary of Terms Used in Apothecary Jar Inscriptions. London & Boston: Faber & Faber, 1978. 249 pp. Ill. $37.50.

Henri F. Ellenberger. *Les mouvements de libération mythique et autres essais sur l'histoire de la psychiatrie.* Montreal: Les Editions Quinze, 1978. 343 pp. $12.95.

George E. Gifford, Jr. (Editor). *Psychoanalysis, Psychotherapy, and the New England Medical Scene, 1894-1944.* New York: Science History Publications, 1978. xxiii + 438 pp. Ill. $35.00 ($28.00 prepaid).

Robert S. Gottfried. *Epidemic Disease in Fifteenth Century England. The Medical Response and the Demographic Consequences.* New Brunswick, N. J.: Rutgers University Press, 1978. 13 + 262 pp. Ill. $17.00.

Luis S. Granjel. *La medicina Española del siglo XVII.* Salamanca: Editions Universidad de Salamanca, 1978 (Historia general de la medicina Española, III). 250 pp. Ill. 750 pp.

Gerald N. Grob. *Edward Jarvis and the Medical World of Nineteenth-Century America.* Knoxville: University of Tennessee Press, 1978. viii + 300 pp. $14.00.

Leone McGregor Hellstedt (Editor). *Women Physicians of the World. Autobiographies of Medical Pioneers.* Washington-London: Hemisphere Publishing Corp., 1978. xv + 420 pp. Ill.

Albrecht Hirschmüller. *Physiologie und Psychoanalyse in Leben und Werk Josef*

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* The *Bulletin* reserves freedom of decision as to the publications to be included in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

*Breuers*. Berne: Hans Huber Verlag, 1975 (Jahrbuch der Psychoanalyse, Beiheft 4). 461 pp. Ill. Fr. 56.

B. H. Kean. Kenneth E. Mott and Adair J. Russell (Editors). *Tropical Medicine and Parasitology. Classic Investigations*. Ithaca & London: Cornell University Press, 1978. 2 vols. Ill.

Judith Walzer Leavitt and Ronald L. Numbers. *Sickness and Health in America. Readings in the History of Medicine and Public Health*. Madison: University of Wisconsin Press, 1978. ix + 454 pp. Ill. $20.00 (cloth); $8.95 (paper).

Victor A. McKusick. *Mendelian Inheritance in Man: Catalogs of Autosomal Dominant, Autosomal Recessive and X-Linked Phenotypes*. 5th ed. Baltimore & London: The Johns Hopkins University Press, 1978. xcl + 975 pp. $29.50.

Ruth C. Maszkiowics. *The Presbyterian Hospital of Pittsburgh from Its Founding to Affiliation with the University of Pittsburgh*. Pittsburgh, Pa.: Presbyterian-University Hospital, 1978. xx + 107 pp. Ill. $10.50 (Available from Presbyterian-University Hospital, Desota at O'Hara St., Pittsburgh, Pa. 15213).

Juan Antonio Paniagua. *El Doctor Chanca y su obra médica* (Vida y escritos del primer médico del Nuevo Mundo). Madrid: Ediciones Cultura Hispanica, 1977. 133 pp. 300 ptas.

*The Parisian Education of an American Surgeon· Letters of Jonathan Mason Warren (1832-1835)*. With notes and introduction by Russell M. Jones. Philadelphia: The American Philosophical Society, 1978 (Memoires of the American Philosophical Society, vol. 128). 266 pp. Ill. $8.00.

Margaret Pelling. *Cholera, Fever and English Medicine, 1825-1865*. Oxford-New York: Oxford University Press, 1978 (Oxford Historical Monographs). 342 pp. $18.50.

M. Jeanne Peterson. *The Medical Profession in Mid-Victorian London*. Berkeley: University of California Press, 1978. x + 416 pp. $17.50.

Walther Riese. *Selected Papers on the History of Aphasia*. Ed. by Richard Hoops, Yvan Lebrun and Eric Buyssens. Introduction by William Gooddy. Amsterdam & Lisse: Swets & Zeitlinger, 1977 (Neurolinguistics, 7). 144 pp. Ill. Dutch Guilders 48.

Todd L. Savitt. *Medicine and Slavery. The Diseases and Health Care of Blacks in Antebellum Virginia*, Urbana: University of Illinois Press, 1978. 332 pp. Ill. $15.00.

Richard W. Wertz and Dorothy C. Wertz. *Lying-in: A History of Childbirth in America*. New York: The Free Press, 1977. xii + 260 pp. Ill. $10.00.

# CONTRIBUTORS TO THIS NUMBER

Darrel W. Amundsen, M.A., is a doctoral candidate in the Department of History, University of British Columbia, and Associate Professor of Classics, Department of Foreign Languages and Literature, Western Washington University, Bellingham, Wash. 98225

Edward C. Atwater, M.D., is Associate Professor of Medicine and of History of Medicine, University of Rochester School of Medicine and Dentistry, Rochester, N.Y. 14642

Daniel M. Fox, Ph.D., Project Director of the Center for the Study of Photographic Images of Medicine and Health Care, is Professor of Humanities in Medicine and Assistant Vice-President for Health Sciences, State University of New York, Stony Brook, N.Y. 11794

Gary B. Ferngren, Ph.D., is Associate Professor of History, Oregon State University, Corvallis, Ore. 97331

Saul Jarcho, M.D., is Associate Attending Physician, Mt. Sinai Hospital, New York. His address is 35 E. 85th St., New York, N.Y. 10028

Steven J. Peitzman, M.D., is Assistant Professor of Medicine at the Medical College of Pennsylvania and Staff Neurologist at the Veterans Administration Hospital, Philadelphia. His address is 475 West School House Lane, Philadelphia, Pa. 19144

Robert A. Robinson, M.D., is Professor and Chairman, Department of Orthopedic Surgery, The Johns Hopkins School of Medicine, 600 N. Wolfe St., Baltimore, Md. 21205

James Terry, Project Coordinator of the Center for the Study of Photographic Images of Medicine and Health Care, is a doctoral candidate in history, State University of New York, Stony Brook, N.Y. 11794

Ilza Veith, Ph.D., D.Med.Sci., is Professor and Vice Chairman, Department of the History of Health Sciences, and Professor, Department of Psychiatry, University of California, San Francisco, Calif. 94143

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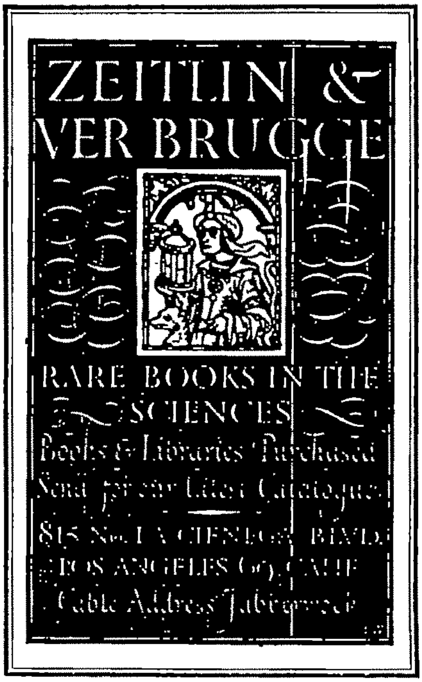

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# Bulletin of the History of Medicine

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# Bulletin of the History of Medicine

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## *THE FIELDING H. GARRISON LECTURE*

# THE PHYSICIAN IN MEDIEVAL MUSLIM SOCIETY*

FRANZ ROSENTHAL

Some time ago I had one of those welcome opportunities to recommend someone for a vacant position. The difficulty was that a nonpareil was required who would be not only an inspiring teacher and crack administrator but also a master of everything concerned with Islam and, in addition, well versed in several other Near Eastern fields. Such requests are not uncommon, and I therefore like to stress the fact that what we call Islamic studies is a very large field, much too large to be mastered by any one individual. The customary complaint of scholars, regardless of the particular vineyard they are cultivating, about neglect of their own field is certainly justified in the case of Islamic studies. It is, however, undeniable that within Islamic studies, the study of Muslim medicine has been reasonably well taken care of in the past and, in particular, in the last two decades. Outside the world of Islam, England and Germany are the countries with the largest share of accomplishments in this respect.

It is especially gratifying that many young scholars have been working on the history of Muslim medicine. Manuscripts have been discovered and described, and texts have been edited and translated. Two fundamental bibliographies have been published, both in the same year—1970. The one by Manfred Ullmann in Tübingen was originally intended as a general survey of the history of medicine in Islam, but its author realized that so much unknown material was buried in manuscripts or remained concealed, unrecognized and misunderstood, in the printed books that it was necessary first

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*The text of the Fielding H Garrison Lecture, delivered on May 4, 1972, at the annual meeting of the American Association for the History of Medicine in Montreal, Canada, is offered here belatedly as a memento of what for me was a truly remarkable occasion.

BHM 52 (1979) 475-491

of all to provide sound bibliographical data.[1] The other work, by Fuat Sezgin of the University of Frankfurt a.M., was conceived within the general framework of a comprehensive history of Arabic literature to about the year 1000. The amount of hitherto unknown manuscript treasures described by Sezgin is indeed remarkable, but the author was also keenly aware of the absence of historical interpretation of the data and felt compelled to make the daring attempt to provide a kind of provisional history of Muslim medicine in the early centuries of Islam.[2] Both works make it clear that the study of the history of medicine in Islam is still in its beginning, that the source material has been identified only incompletely, that it has hardly been studied in sufficient detail or not been studied at all, and that the discoveries to be made and the problems to be solved are legion. A comprehensive archive of microfilms of Near Eastern medical manuscripts does not yet exist as far as I know,[3] and collaborative projects, such as a proper indexing of ar-Râzî's vast *Continens*, of which an Arabic text has now appeared in print, would not be easy to organize. Nevertheless, it is true that as far as Islamic studies as a whole are concerned, the historians of medicine have comparatively little reason to complain about the progress that has been made.

Quite a few of the scholars who are now making significant contributions to Muslim medical studies are general Islamicists rather than historians of medicine, and not without reason. The Arabic medical literature is of considerable importance for the Classicist who can recover from it lost works of Greek authors or use the Arabic translations for the textual criticism of preserved Greek texts, and this applies also to fields other than medicine. Philologians can gain valuable and much needed insights into Arabic lexicography and also, to a lesser degree, into Greek lexicography from comparing the Arabic translation of a Greek text with its Greek original. It is, however, something more basic than such externalities that attracts the student of Muslim civilization to the study of Arabic medicine. Intellectual activity in medieval Muslim civilization tended toward abstract thought and as a rule tried to keep away from the concrete details of human existence. Misguided mental acrobatics and arbitrary abstractions are not entirely absent from Arabic medicine, but, on the whole, it lures the student with an entirely satisfactory combination of profound intellectual concern and intimate contact with the realities facing the individual and the society in which he lived. The specter of having to work in the abstract void, which our outlook on life finds disquieting, is less threatening here than elsewhere in the study of Islam. Looking at Muslim physicians as they are mirrored in the literature, we see a happy union of all that was best in Muslim civilization in thought and action. D. S. Goitein has recently sung the praise of physicians in the Muslim orbit in these ringing words: "The torchbearers of secular erudition, the professional expounders of philosophy and the sciences, disciples of the Greeks, heirs to a universal tradition, a spiritual brotherhood which transcended the barriers of religion, language, and countries." And, Goitein adds, "almost any doctor of distinction was also a member of the entourage of a king, a sultan, a vizier, or a governor."[4]

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[1] M Ullmann, *Die Medizin im Islam* (Leiden-Koln, 1970; *Handbuch der Orientalistik*, Erste Abteilung, *Erganzungsband*, VI, 1) An excellent brief survey of *Islamic Medicine* by M Ullmann appeared in Edinburgh in 1978

[2] *Geschichte des arabischen Schrifttums*, Band III *Medizin-Pharmazie, Zoologie-Tierheilkunde bis ca 430 H* (Leiden Brill, 1970)

[3] The Ma'had al-Makhṭûṭât al-'Arabîyah in Cairo is in the process of publishing catalogues of its microfilms of medical manuscripts from collections in many countries Two volumes have appeared so far (*Fihrist al-makhṭûṭât al-muṣawwarah* (Cairo 1959 and 1978)).

The qualitative aspect of medicine as a science and profession in Islam is extraordinarily well described in Goitein's phrases. They give us a view of the mountain top where the air is pure and the sun shines bright. It is obvious, however, that there must have been a broad supporting base for medical services in Muslim society serving a large segment of it. Conditions there might not have been quite as splendid as they were on the top. The significance of that larger medical establishment for the welfare of society and for the general character of medical practice must have been considerable. The question which I am posing here and which I am trying to answer, however imperfectly and tentatively, is: What do we really know about the quantitative aspect of the medical establishment in medieval Islam? The linking of the study of the history of Muslim medicine with sociology will make it necessary for scholars to go beyond the strictly medical literature in order to collect the necessary information. Historical, biographical, and belletristic works have to be sifted, and they are innumerable. The immense theological and juridical literature can be expected to yield some relevant information, and philosophical works, in particular those dealing with ethics, are likely to make a small contribution. It may, for instance, not be entirely irrelevant to notice that even in a work on logic, a famous eleventh-century Spanish author exemplifies a point in this manner: "Thus, were you to say, '*the* physicians are highly skillful,' everybody who hears you say that would feel you are making a wrong statement. For '*the* physicians' signifies something general, while 'highly skillful' obviously refers only to some physicians," and not to all.[5] All such little bits and pieces submerged in a vast ocean of literature will have to be collected. This is a task which has as yet hardly begun and which will take a long time to bring to fruition. Synthesis and reconstruction will be a difficult and uncertain process, in which much will have to be left to guesswork. It is no accident that present-day advocates of quantitative history think preferably of the history of modern times where the needed information is abundant. It is limited and haphazard for medieval times, in the East to an even greater degree than in the West. The people of the Middle Ages themselves had no techniques for measuring the quantitative aspects of history. Some of these techniques would not have been beyond their abilities, but it seems that they were not particularly interested in this kind of approach. Perhaps they were wiser than we are. History is not inclined to deal respectfully with quantities. At least so far, it has shown no inclination to attribute the development of human civilization to quantitative aspects. History appears to have a way of remembering the quality of achievements to make lasting value judgments.

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[4] S D. Goitein, "The medical profession in the light of the Cairo Geniza documents," in *Hebrew Union College Annual*, 1963, *34* 177 For Goitein's fundamental research, see also below, pp 482 & 489 The second volume of his *A Mediterranean Society* (Berkeley-Los Angeles-London. University of California Press, 1971) was not yet available when I presented the Garrison lecture The chapters on the medical profession (240-261) and druggists, pharmacists, perfumers, preparers of potions (261-272) contain detailed and concrete information complementary and superior to what is presented here

Our first question is necessarily: What information do we have about the total number of physicians in a given locality at a certain time? Our sources are chary with actual figures, which does not come as a surprise after what has just been said. What we do find, are frequent references to *all* doctors in some region or town, without telling us how many they were. We hear that prominent physicians had numerous students; this is vague and, moreover, the students need not always have been men who were practicing medicine or intended to do so. In certain situations we hear about physicians in the plural, such as, for instance, an unspecified number of physicians attached to the army of an 'Abbâsid caliph.[6] An occasional miniature in a manuscript may show us a number of physicians in consultation.[7] Major hospitals are described as most generously staffed, but no figures are given. And, not always but quite frequently, it is a plurality of physicians that is reported to have been in attendance on a ruler or an otherwise prominent personage. In particularly favorable circumstances, it is possible to make a count of at least some of the physicians who served a given ruler during his lifetime.[8] However, actual figures are rarely met with. When they are, they are usually suspiciously round figures. Thus, a ruler in the region of the Caspian Sea in the second half of the tenth century had twelve doctors assembled to treat him for a special, baffling disease. Incidentally, the more the twelve treated him, the sicker he became, and all of them had to be honorably dismissed upon the insistence of the great medical authority who was eventually brought into the case.[9] The figure twelve arouses suspicion, and the story itself does not in fact suggest that twelve doctors could be found together in a provincial town. They had to be gathered from nearby and distant locations. Again we hear that twenty-four, or twice twelve, physicians were appointed when a hospital was founded in the capital city of Baghdâd earlier in the same tenth century.[10] Another report about the selection of a superintendent for the same hospital speaks of more than 100 prominent physicians from all over the area as having been considered for the position. Here, the entire report, and not only the round figure, seems to be of somewhat doubtful historicity. If we could assume, and it does not appear to be an unlikely assumption, that there were really 100 physicians of prominence available at any one time in a city such as Baghdâd, whose population is estimated to have been around 300,000,[11] there was no shortage of physicians per number of inhabitants, as it is clear that the number of prominent physicians was only a fraction of all the practitioners in residence. The best available figure is given for Baghdâd in the year 931. In that year, some 860 physicians from all over town were invited to pass an examination to show their competency. There were others in addition who were not invited, because it was thought unnecessary for them to undergo an examination.[12] Here we get a minimum of one physician for at most three hundred inhabitants, a very high ratio indeed. Strangely enough, approximately the same ratio of physicians to population emerges a few centuries later from a report outlining the foundation of a new city by royal decree in Mongol-ruled Iran. The blueprints called for providing the new town with fifty physicians, each of whom was to have ten students who no doubt were also already capable of practicing. Additional medically trained personnel was to be attached to the new city's hospital. The number of houses in that town was set at 30,000. Thus, there was to be one physician or qualified medical student for each sixty houses. If we assume five persons per house, which it must be admitted will be considered by many a rather low figure, we have again one physician for three hundred people. Unfortunately, the report has been suspected, and with good reason, of being not an actual contemporary document but a much later invention originating not in Iran but somewhere in northern India, and we do not know whether the invention was based upon actual models.[13] It should therefore be considered as the expression of someone's thinking as to what the ideal medical situation ought to be. Since ideals are rarely attained, the actual situation was hardly ever as good anywhere, maybe not even in Baghdâd, except for that one fleeting moment there.

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[5] Ibn Ḥazm, *at-Taqrîb li-ḥadd al-manṭiq*, ed Iḥsân 'Abbâs (Beirut n y [1959]), 85.

[6] Ibn Abî 'Usaybi'ah, *Uyûn al-anbâ'*, ed A. Müller (Cairo and Konigsberg, 1882-4), I, 153.

[7] H Buchthal, "Early Islamic miniatures from Baghdad," In *J Walters Art Gallery*, 1942, *5* 18-39, and K. Weitzmann, "The Greek sources of Islamic scientific illustrations," in *Archaeologica Orientalia in Memoriam Ernst Herzfeld* (Locust Valley, N.Y , 1952), 244-266

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[8] S Y Jadon, "The physicians of Syria during the reign of Salâh-al-Dîn," *J Hist Med & Allied Sci*, 1970, *25* 323-340

[9] Ibn Abî Usaybi'ah, *op cit* (n 6 above), I, 146 The number twelve is not found in the earlier al-Qifti, *Ta'rîkh al-ukamâ'*, ed J Lippert (Leipzig 1903), 149 f, making it even more suspect The medical authority was Jibrîl b 'Ubaydallâh b. Bukhtîshû' (d 1006), and the ruler Khusrawshâh (b. Mubâdir, not in al-Qifti), presumably one of the Musâfirids, but his identity cannot safely be established

[10] Ibn Abî Usaybi'ah, *op cit.* (n. 6 above), I, 310

[11] For the population estimate, see J. C Russell, *Trans Amer. Philos Soc*, 1958, *48* (3). 88a It should be kept in mind that population estimates for medieval cities are highly uncertain.

[12] Al-Qifti, *op. cit.* (n 9 above), 191 f Ibn Abî Usaybi'ah, *op. cit* (n. 6 above), I, 22

Nowhere do we have, I believe, a clear-cut indication as to the distribution of physicians within the larger cities, although, upon careful study, the known location of hospitals may provide some useful hints. What is more distressing, nothing is really known directly about the situation outside the larger urban complexes. It is but a small consolation that our knowledge of society and economics in medieval Muslim villages and rural regions is also practically zero. The great al-Ghazzâlî tells us that a person may buy books on medicine in order to be able to treat himself. However, if there is a physician in a given place, such books can be dispensed with as a superfluous luxury.[14] This implies that there were many localities in which no physician could be found. Al-Ghazzâlî probably had the average small town in mind, rather than sparsely populated rural areas and villages.

What, we may ask next, do we know about the material situation of physicians? Were they generally able to make a living from the practice of medicine, how much did they earn, how did their earnings compare with those of other social groups within society? It is only natural for the literary tradition to stress the success story. We hear about those tremendous salaries and extraordinary gifts, those promotions to high rank and positions of power and influence, that the physicians of the mighty and the rich are frequently reported to have received. Numerous stories tell us about them. It is difficult to translate them into concrete data, except for the bare fact that large wealth was involved. The stories do, however, reflect a larger truth. They tell us something about society's scale of values. Just as reports about vast sums of money lavished upon entertainers or poets illuminate the role played by entertainment in a given social structure, the stories about highly remunerated physicians indicate Islam's great esteem for medical knowledge, and their faithful preservation in literature is to be understood as a continuous re-affirmation of this value judgment even under adverse contemporary conditions. Occasionally, we also hear about the material success of ordinary physicians in ordinary times. In the early thirteenth century, an outstanding scholar, 'Abd-al-Laṭîf al-Baghdâdî, mentioned a man who came to Baghdâd to study with him. The man told him that six years of medical practice in Tiflis had enabled him to acquire very considerable property.[15] Regrettably, no details are given. 'Abd-al-Laṭîf himself, he tells us, was unable to check the man's veracity, and we, of course, are even less in a position to do so.

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[13] E. G. Browne, *Arabian Medicine* (Cambridge University Press, 1921, reprint 1962), 108 f See also Browne's *A Literary History of Persia* (Cambridge. University Press, 1920; reprint 1969), III, 86; and the literature cited by C. A Storey, *Persian Literature* (London Luzac, 1953), I (2), 1230. The putative author was the great statesman and historian Rashîd-ad-dîn Fadlallâh (d. 1318). A charter for a suburb founded by Rashîd-ad-dîn and named ar-Rashîdîyah after him is considered genuine by its editors, Mujtaba Minovi and Iraj Afshar (Teheran, 1350/1972) I have not seen the publication and know about it only from the review by B. G Martin in *J Amer. Oriental Soc* , 1973, *93* 561 f Thus I do not know whether it contains the same or similar details about the hospital there.

[14] Al-Ghazzâlî, *Iḥyâ' 'ulûm ad-dîn, Kitâb Asrâr az-zakâh* (Cairo, 1352/1953), I, 199 Medical self-help books were, of course, also useful for emergencies and for travelers

Quite often, the biographies of highly successful physicians—and, naturally, we have biographies only of them—mention hard and obscure beginnings. One of them is described as having for a long time been a poor general practitioner before he finally achieved fame and fortune. Another was a pharmacist who supplemented his earnings by doing some prescribing of his own, until, by some miraculous feat of diagnosis and cure, he came to the attention of the ruler and gained prominence. It is not unreasonable to assume that the original pre-success status of those men more accurately reflects the economic position of the average physician. Recently, a most illuminating study of the history of prices and salaries in the medieval Muslim East has been published by E. Ashtor, but the author has practically no additional information on the fees and incomes of physicians.[16] On the other hand, Goitein has furnished us with new and valuable facts. His information has been extracted with great ingenuity from Jewish documents dating mainly from the eleventh and twelfth centuries and found in the Cairo Geniza. This repository of discarded writings constitutes the only large-scale hoard of documents from the Near Eastern Middle Ages that tells us something about how the ordinary people of the times worked and lived. Goitein calls attention to the frequent references to contributions made to charity by members of the various professions. The contributions of physicians, he states, are rather large, but, he continues, "it is significant that one physician paid only one dinar, the same as various dyers," a craft that ranked very low on the social scale. Another doctor gave 1½ dinars, the same amount as a silk worker and some storekeepers.[17]

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[15] C Cahen, " 'Abdallatîf al-Baghdâdî, portraitiste et historien de son temps," *Bull d'Etudes Orientales de l'Institut Français de Damas*, 1970 [1971], *23* 123 Another example is the Christian physician in tenth-century Spain who "earned (much) money and real estate with (his practice of) medicine" (Ibn Juljul [below, n. 27], 96, quoted by Ibn Abî Usaybi'ah, *op cit* (n 6 above), II, 41)—again, no details

[16] *Histoire des prix et des salaires dans l'Orient médiéval* (Paris S E V P E N., 1969)

A student of Goitein, A. L. Motzkin, has made a special study of the documents giving information about a physician who was a member of a family of high rank in the Jewish community. He concluded that "physicians belonged to the upper middle class: They travelled widely, lived well, (and) had an excellent education.[18] Clearly there are contradictions in the Geniza evidence, and a generalization such as the one just mentioned does not tell the entire story. The crucial question may be asked whether Jewish conditions as described in the Geniza can be assumed to have validity for Muslim society as a whole. This appears generally to be the case. The practice of the crafts and professions and the rewards for it in the urban setting was not essentially different whether members of the Muslim majority or minority groups were involved. Considerable hesitation seems, however, to be called for in the special case of the study and practice of medicine. Both Christians and Jews enjoyed an unusual reputation as physicians. Consequently, physicians in turn can be expected to have occupied a highly visible position in their communities. It is well possible that the poorer sort of physician was less conspicuously represented among them. Physicians may by and large have constituted a materially more favored element within minority groups than in the world at large.

The medical establishment in Islam also harbored physicians who worked only part-time in the profession and gained only part of their livelihood from it. Here we have another problem for the assessment of the material position of physicians in society. A continuous, and at times very intensive, effort was made to establish and enforce professional standards. It was nonetheless not too difficult to learn the craft from a few textbooks and to function quite competently when circumstances made such a course economically attractive. The great medical writer from eleventh-century Egypt, Ibn Ridwân, is an outstanding if exceptional example of how effective and successful medical self-study could be. Ibn Riḍwân appears to have devoted himself full-time to the practice of medicine in the sense that from a certain period of his life on, his principal income was derived from it. It is noteworthy, though, how often the great men of medicine, those who made the biographical dictionaries, are described and known as having excelled not only in medicine but in a variety of other disciplines. No doubt this reflects the well-known fact that physicians were supposed to live up to the highest intellectual standards of their times, and this required a good acquaintance with an established canon of subjects. In the case of many leading physicians, it is, however, not at all certain that the practice of medicine was their principal source of income. Doubts in this respect are especially pertinent when we are dealing with men who were not members of a minority religion but were Muslims and had family ties to the ruling scholarly establishment. The class to which traditional scholars as a rule belonged had its economic base in commercial enterprise. Even after they had achieved rank and position, scholars were often deriving additional income from commercial investments within or outside their own families. Thus, it comes hardly as a surprise to us when we are told about the late twelfth-century physician Raḍî-ad-dîn ar-Raḥbî that "he liked commerce."[19] Some more detail is given about a slightly younger contemporary of ar-Raḥbî, named Kamâl-ad-dîn al-Ḥimṣî. He, too, liked commerce. Not only that, he also made most of his living from it. This was quite necessary, since he is said to have had an aversion to making money from the practice of medicine. This aversion probably resulted from religious scruples, but we are not told what his motivation was and thus cannot be sure.[20] He even went so far as to treat hospital patients free, although doctors rendering hospital service usually did so on a salary basis. It may be added that al-Ḥimṣî's case turned out to be not very different from those of other volunteer workers then as well as now. After a while, he permitted himself to be persuaded to be put on the payroll and to draw a salary for his services just as his colleagues did. He continued to do so till the day he died. These men seem to have been exceptions. It was comparatively rare for practicing physicians to have close ties to the Muslim scholarly class. Many followed in the footsteps of their fathers and forefathers who had practiced medicine before them, and most chose the practice of medicine as a career that would provide for their livelihood, presumably on a level that by and large did not exceed that of the average shopkeeper.

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[17] S. D Goitein, *A Mediterranean Society* (Berkeley and Los Angeles. University of California Press, 1967), I, 78 See also n. 4 above

[18] A. L Motzkin, "A thirteenth-century Jewish physician in Jerusalem," *The Muslim World*, 1970, *60* 349.

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[19] Ibn Abî Uṣaybi'ah, *op. cit* (n 6 above), II, 194.

[20] *Ibid* , II, 201. Cf Burzôë's statement, below p 488 It may be mentioned that the ninth-century ar-Ruhâwî, in the last chapter of his *Adab aṭ-ṭabîb*, appears to have insisted that physicians work full time and if they cannot earn enough, give up their practice. Cf the rather opaque translation by M Levey, *Medical Ethics of Medieval Islam* (Philadelphia, 1967), *Trans Amer. Philos. Soc* , *57* (3), 93 f Cf. also C. Bürgel in *Sudhoffs Arch* , 1966, *50* 360

Indirect evidence as to the economic position of physicians and their ability to make a living from the practice of medicine comes from the seamy fringes, the realm of quacks and charlatans. The numerous references to their existence and stories about them make it appear to have been a very large realm indeed. The respectable writers on medicine professed to know about them and warned against them. They were a constant subject of grave concern to the authorities charged with the control of honesty and fair dealing in the crafts and in commerce. One might easily get the impression that charlatans were common and even outnumbered bona fide physicians. However, this impression may be deceptive. There is no hard information that would allow us to gauge the actual extent of charlatanry. We can be sure that there was no dearth of ignorant and unqualified physicians. Considering the general state of medicine at the time, this could hardly have been otherwise. But there is a vast difference between lack of ability and outright fraud. While the results may be equally unfortunate in either case, in a sociological context the distinction must be preserved. It was customary for writers on medical deontology to start out with bitter complaints about the situation that existed in their times. They complained that medicine was in a state of decay, and things were quite different from what they had allegedly been in an earlier age. Physicians now no longer live up to the high standards of professional ethics and professional competence. Following the precedent of the Pseudo-Hippocratic *Nomos,* reference was made to contemporary physicians as "physicians in name only, and not physicians in reality."[21] Physicians were of course not the only ones to make such complaints; all scholars made them with respect to their particular disciplines. "The complaint about the times" was a favorite topic. Such statements were often conventional and, like all other medical deontology in Islam, continued a tradition inherited from Hellenism. They reflect common human psychology. Their truth, or untruth, would seem to be something that can hardly ever be objectively decided.

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[21] Cf Ibn Rıdwân, *Pî t-tatarruq bı-t-tıbb ılâ s-sa âdah*, Ms Istanbul Hekımobhlu Alı Pasha 691, fol. 121b; F Rosenthal, *op cıt* (below n 36), 184

Tradition and convention also played a large role in another area where we are tempted to look for further information on the distinction between genuine physicians and quacks. A special small niche in Arabic literature, in particular within the vast structure of Arabic poetry, was reserved for the praise of the good physician and the blame of the bad physician. The topic was popular and much savored. If, for instance, a Christian physician bore the name of 'Îsâ, i. e., Jesus, and if the poet who was treated by that 'Îsâ was not satisfied with his services, his muse might inspire him to rhyme:

Doctor 'Îsâ, please, show pity!—You are Noah's deadly deluge.
Separating soul from body—is your treatment's only purpose.
Doctor 'Îsâ, the physician—and Christ, 'Îsâ, the Messiah—have, indeed,
  nothing in common.
While the latter, Scripture tells us,—had the dead restored to new life,
You are quite a diff'rent 'Îsâ.—You are here to kill the healthy.[22]

The imagery, it may be noted, is conventional. The poet quite possibly made up the entire incident. Another poet who lavished praise upon an efficient physician named Ibrâhîm may, or may not, have had in mind an actual occasion, but he was a bit more original in his imagery:

Ibrâhîm excels in his medical knowledge so much
That he is rightly called the true heir to medical science.
There are many who have wiped out or obscured its wide track,
Whereas he stands out as the man who will follow and clear it.

With the subtlety of his thinking he gives the impression
To be acting between blood and flesh as a calm mediator.
When the soul is annoyed with the body that it inhabits,
The good doctor succeeds in arranging a truce between them.[23]

Whether it is praise or blame, we are dealing with a traditional subject. And again, we have to look for its roots in Classical Antiquity, in those centuries of late Hellenistic civilization preceding the coming of Islam. Late Greek and Byzantine anthologies such as those of Stobaeus and Pseudo-

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[22] The verses were reported by ath-Tha'âlibî, *Yatîmah* (Damascus, 1304/1886-7), I, 218. The poet is uncertain. The verses were quoted by ash-Sharîshî, *Sharh al-Maqâmât al-Harîrîyah* (Cairo, 1306/1889), in the name of Abû Nasr b. Kushâjim, whom the *Yatîmah* mentions in this connection apparently as a son of the well-known poet Kushâjim. From ash-Sharîshî, the verses were taken over in the collection of Kushâjim's poetry by Khayrîyah M. Mahfûz, *Dîwân Kushâjim* (Baghdâd, 1390/1970), 128 f. It may be noted that Kushâjim wrote a long poem in praise of his physician; see *Dîwan Kushâjim*, 173-175.

[23] Ath Th'âlibi, *op. cit.* (n. 22 above), I, 507. The poet appears to have been as-Sarî ar-Raffâ. The word "soul" here and in the preceding verses is, translated more literally, "spirit.

Maximus Confessor[24] follow the same literary pattern and reveal a similar intellectual attitude in their chapters devoted to medicine and physicians. The positive side is pretty much neglected. Most of the information is either neutral or outright negative. Human psychology is no doubt more responsible for this than are the facts of the situation. Literary circles considered it more fashionable and rewarding to make fun of physicians than praise them. Muslim littérateurs did not differ in this respect from their Hellenistic predecessors. The poets among them relished malicious satire, especially when it entailed no real risks, and seized every opportunity to show off their biting wit. But whatever attention was paid by them to physicians, was directed toward the upper layers of the profession. The physicians they had in mind as a rule were, it would seem, the highly reputed and supposedly well qualified physicians of the well-to-do. Their criticism was not directed against charlatanry. Much as we may regret it, it affords no clue as to the possible prevalence of fraudulent practitioners. In fact, we cannot really prove that quackery was quantitatively much more prevalent in Muslim society than it has been in more modern times. It probably loomed large in the minds of medieval men fearful as they perforce were of disease, and their fears may have led to exaggeration and distortion. Moreover, quackery can be assumed to be in direct proportion to the general level of education. It was high in Islam for its time, but the number of individuals whose educational level made them easy marks for charlatans was naturally very large.

Not surprisingly, quackery was a lucrative business where it went unchecked. You might sell, we are told, a medicine that guaranteed the preservation of healthy teeth and helped to eliminate bad mouth odor, incidentally a much discussed subject and the butt of numerous jokes. Small wonder, then, that you could make sixty to seventy silver dirhams a day selling it, not counting assorted copper coins.[25] Hawking a kind of paste or cake that served as a general tonic and had the additional virtue of being able to allay the pangs of hunger for twelve hours would always be profitable business, but in a period of famine and inflation, it could bring in no less than three hundred dirhams a day.[26] These figures were most probably exaggerated by the enterprising salesmen themselves or by the writer who reported them.

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[24] Stobaeus, V, 898-901, in the edition of C Wachsmuth and O. Hense, and Pseudo-Maximus Confessor, *Patrologia Graeca*, XCI, 947-950

[25] E Wiedemann, "Über Charlatane," *Sitzungsberichte der physikalisch-medizinischen Sozietat in Erlangen*, 1911, *43* 225 f , from the *Kashf al-asrār* of the thirteenth-century al-Jawbarī (cf S Wild, in *Proceedings of the VIth Congress of Arabic and Islamic Studies*, 58-63 [*Kungl Vitterhets Historie och Antikvitets Akademien*, *Filologisk-filosofiska serien*, XV, 1975])

[26] Wiedemann, *op cit* (n 25 above), 211.

We are also unable to translate the amounts given into anything approximating their true purchasing power. Furthermore, the preparation that served to relieve the discomfort of hunger is described as having been effective, as it might well have been. And while both medicines had to do with matters that were considered the proper concern of the practitioner, they were rather marginal from the medical point of view. Thus we do not learn here very much about the comparative situation of medicine vis-à-vis quackery, and it does not really matter that the sums of money mentioned probably amounted on an annual basis to more than even a prominent physician could expect to earn.

Numbers and economic rewards are essential factors to be considered for determining the position of physicians in society. A word remains to be said about the role in Muslim society that physicians created for themselves by virtue of the extent and character of their special contribution to it. The Muslim religion is notable for preaching social consciousness and demanding that the believers follow its mandates. We would expect the social consciousness of physicians to have gone beyond ordinary limits. What information do we have on this point?

We have seen that the sources tend to highlight financial and social success. We are more likely to have details about the few physicians who gained access to high society than the many who were not that fortunate. Very rarely do we find an express statement to the effect that a physician kept away on purpose from entering the service of an important personage when the opportunity was offered to him.[27] The rewards of social success were great. Occasionally it provided a valuable sinecure, even though few court physicians could have been as lucky as one of those employed by the Ayyûbid sultan of Egypt, al-Malik al-ʿÂdil, who died in 1218 at the age of about seventy-three years. That physician claimed rather smugly that he had been eating the sultan's bread for many years, and the sultan had made use of his services just once, for a single day.[28] Under normal conditions, fees were tailored to the patient's ability to pay. A prominent eye surgeon was willing to do a cataract operation for practically nothing on the assumption that the patient was unable to pay the full fee. He changed his mind and refused to go through with the operation when he learned that the patient had more money than he had solemnly sworn he had.[29] An early autobiography claiming to embody the wisdom of a pre-Islamic sixth-century Persian, Burzôê, tells us that a man may study medicine for four purposes: to make money, to live the good life, to acquire rank and fame, or to assure for himself lasting bliss in the other world. Such bliss was the greatest reward to come to a physician. One way of assuring it for oneself was to treat all sick persons without accepting any recompensation.[30] Such religious motivation for medical charity was probably less prominent in Islam than it was in medieval Europe, even if the somewhat dubious exception mentioned before was nothing unique, and physicians treating patients, whether in hospitals or elsewhere, sometimes did not expect to be paid. We do not have any concrete information on the amount of charity work done by physicians. However much it was, it must have been little as compared to the needs of the large numbers of those who could not afford medical attention, since the vast majority of the population lived from hand to mouth with little to spare for extra expenditures. It is clear, however, that the moral imperative to treat all sick Muslims without regard to their social or financial status was very much alive. A hospital charter stressed the point that that hospital should be open not only to the poor but also to the rich,[31] though there were precious few rich people ever to be found in hospitals—which certainly leaves no doubt that no financial distinctions should be made.

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[27] Ibn Juljul, *Tabaqât al-aṭibbâʾ wa-l-hukamâʾ*, ed. Fu'âd Sayyid (Cairo, 1955). 104, with reference to Saʿîd b ʿAbd-ar-Rahmân Ibn ʿAbd-Rabbih, a nephew of the famous Spanish littérateur Ibn ʿAbd-Rabbih. Ibn Juljul is quoted by Ibn Abî Uṣaybiʿah (*op cit.*, n 6 above), II, 44.

[28] C. Cahen, *op. cit.* (n 15 above), III

Practically no social distinctions are apparent in the many case histories we hear about. The great ar-Râzî has left us a collection of *mujarrabât,* that is, medicines tested in actual cases. His collection includes some 650 cases of men, women, and children. The patients came in person to see the doctor, or they wrote to him, or they had their complaints transmitted by somebody else, or they complained in a manner not further specified.[32] Presumably only the more interesting cases were listed and, although this is not stated expressly, only those where the prescribed medication proved effective. Thus the number of 650 can be extrapolated into a rather large practice. At any rate, it would seem that practically all of the patients were ordinary people, and we may assume that there were also some really poor people among them, though whether they were charity cases remains doubtful.

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[29] Ibn Juljul, *op cit* (n 27 above), 81 f.; quoted by Ibn Abî Usaybi'ah, *op cit* (n 6 above), I, 230

[30] From Ibn al-Muqaffa''s preface to his translation of *Kalîlah wa-Dimnah,* quoted from T. Nöldeke's German translation by F. Rosenthal, "Die arabische Autobiographie," in *Analecta Orientalia* (Roma, 1937), XIV, 10 f

[31] Ahmad 'Îsâ Bey, *Ta' rîkh al-bîmâristât fî l-Islâm* (Damascus 1347/1929 [rather, 1358/1940]), 138, with reference to the Mansûrî Hospital in Cairo. "Rich and poor" means "everybody " The poor are referred to in the charter more explicitly.

[32] *Kitâb at-Tajârib,* MS Istanbul, Topkapısarayı, Ahmet III, 1975

Drugs appear to have been rather expensive, quite apart from the fact that compound medicines were considered the more likely to be effective, the more complicated and containing rare ingredients, and thus the more expensive, they were. Precise data on the prices charged, especially retail prices, are few. The vast pharmacological literature is not very likely to contain any, although some may show up in future studies. Drugs and spices were important items of commerce, and there is hope that more documents than those known so far will be discovered. There are sad disappointments. An interesting letter from the eleventh or twelfth century deals with the purchase of drugs, but as far as prices are concerned, it restricts itself to giving the good advice to buy the drugs at the proper prices.[33] A papyrus appears to indicate prices but has so far not been satisfactorily deciphered.[34] The Geniza as studied by Goitein will probably remain the most important source of information. It can be assumed that patients often found drugs expensive and beyond their means. Physicians, too, were concerned with the high prices of drugs. Medical writers wrote special treatises on medicines for the poor, offering, it seems, advice on the less costly medicines.[35]

Public health care on a large scale was clearly beyond the resources of individual physicians. Governmental action and public financial support were required. The charitable precepts of Islam, combined with the Classical tradition of the ruler's moral and practical obligation to take care of the health of his subjects,[36] created the necessary climate. It was, however, upon the urging of physicians that major steps to transform theory into reality were undertaken. The famous tenth-century report on bringing medical services to rural areas and to prisoners held in jail is a good example of cooperation between the government and the medical profession.[37] It has always been praised very highly, and rightly so. However, reforms suggested are not always reforms realized, and this appears to apply to this report. The only safe conclusion it allows us to draw is that medical attention was *not* available in prisons, quite apart from the presumably dismal medical situation in rural regions. We have some information about what prison conditions were, or were supposed to be; it tells us about the existence of prison chaplains, but nothing is said about medical services provided for prisoners.[38]

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[33] A. Dietrich, *Zum Drogenhandel im islamischen Agypten* (Heidelberg, 1954, *Veroffentlichungen aus der Heidelberger Papyrus-Sammlung*, Neue Folge, 1). Among the medical documents referred to by Dietrich, one published by A. Grohmann, in *Archiv Orientální*, 1935, 7: 439, merely asks for information about the price of some potion. A papyrus in the Viennese collection gives the price for one drug, another papyrus seems to contain more data.

[34] G. Levi Della Vida, " A druggist's account," *Archaeologica Orientalia* (n. 7 above), 150-155.

[35] For Ibn al-Jazzâr's work on the subject, cf. C. Brockelmann, *Geschichte der arabischen Litteratur*, 2nd ed. (Leiden: Brill, 1943-1949), I, 274, M. Ullman, *op. cit.* (n. 1 above), 148.

[36] Cf. al-Mubashshir, as quoted in F. Rosenthal, *The Classical Heritage in Islam* (London: Routledge & Kegan Paul, 1975), 35.

[37] Al-Qiftî, *op. cit.* (n. 9 above), 193 f.; Ibn Abî Usaybi'ah, *op. cit.* (n. 6 above), I, 221.

The noblest expression of the deep concern of medieval Muslim society with matters of public health was a highly developed hospital system, a network of urban institutions with large staffs, providing numerous services and frequently having teaching facilities attached to them. Here again, it was the physicians who took the initiative and laid the groundwork. The eleventh-century Zâhid al-'ulamâ' tells us, for instance, how he persuaded his ruler to found a hospital as a reward for the successful cure of the ruler's daughter from a severe illness.[39] Hospitals were completely dependent upon outside endowment support. What usually happened was that the initial endowment became insufficient or was dissipated after a rather short time. This made for progressive deterioration of the original lofty conception of what a good hospital should be like. But hospitals continued to be founded wherever government stability and general prosperity permitted. Apparently there was always willing cooperation on the part of physicians, and no dissent on their part on personal economic grounds.[40]

I have tried here to marshall representative samples of the evidence we have, or can expect to obtain, from literary sources, for the position of physicians as a group in medieval Muslim society. Economically, it was, to all appearances, a group comparable to that of the storekeeper/merchant class, with, of course, a number of exceptions. Numerically, medical men naturally constituted a small percentage of the population, numbering, even at the best of times, less than one-third of one percent in some exceptionally favored location. They were a special element also for cultural reasons. Medical theorists were convinced of the truth of the Hellenistic idea of the inseparability of medicine and philosophy. During the first two centuries of

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[38] F. Rosenthal, *The Muslim Concept of Freedom* (Leiden Brill, 1960), 62

[39] Ibn Abī Usaybi'ah, *op cit.* (n 6 above), I, 253 The monograph of the Zâhid al-'ulama' on the history of hospitals is unfortunately not preserved, at least no manuscript of the work has as yet been noted in the literature

[40] The government, however, was advised to avoid spending too much money on public health services. Cf the early ninth-century *fürstenspiegel* still admired by Ibn Khaldûn more than five centuries later, *The Muqaddimah*, trans. F. Rosenthal (New York Bollingen Series XLIII, 1958, Princeton University Press, 1967), II, 153

'Abbâsid rule, "philosophy" was the slogan of the elite, and the intellectual alliance of medicine with philosophy generally added to the reputation of its practitioners. The alliance was always shaky; soon, it became a constantly growing danger to the physician's societal standing. The word "philosophy"—that is, Greek philosophy—was anathema to the masses and their leaders. If nineteenth-century Western medicine had allied itself with "philosophy," then at the pinnacle of the cultural hierarchy, before "science," the current frontrunner, took over, the damage to the standing of medicine in society would no doubt have been incalculable. Yet, it could hardly have been as great as it was in medieval Islam where the very word "philosophy" was in a sense suspect.

Another special aspect of the medical profession was its continuing interfaith character. With the growing retrenchment of the Jewish and Christian positions and the hardening of the Islamic character of Muslim society, this, too, lost more and more of its positive social significance.

Then, since the top of the profession depended upon upper-class patronage, medical prestige and influence contracted with the growing fragmentation of the Muslim world. This applies to the Muslim world before 1500; thereafter, conditions changed again. However, the significance of this factor was less specific since a general lowering of material standards was involved, with the relative position of physicians left largely unchanged.

What influenced that position most in the course of history was the fact that it was not altogether possible or desirable for physicians to fit themselves comfortably into the dominant religious and legal framework of Islam. They tried hard not to sell their souls, and they kept medicine, in the words of the eleventh-century Christian physician Ibn Butlân, "the most useful of crafts and the most profitable of enterprises,"[40] that is, the craft and science most beneficial for individuals as well as a society somewhat ambivalent about the place it had to assign to it.

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[41] At the beginning of his *Da'wat al-aṭibbâ'*.

# ACTIVE EUTHANASIA

## AN HISTORICAL SURVEY OF ITS CONCEPTUAL ORIGINS AND INTRODUCTION INTO MEDICAL THOUGHT

W. BRUCE FYE

This paper attempts to illustrate the circumstances surrounding the introduction of the concept of euthanasia into medical thought in the late nineteenth century. The emphasis is on what has come to be called "active" euthanasia, an intervention which shortens the life of a patient who is suffering from a painful, incurable disease. Although it is likely that the practice has occurred throughout history, it was not until the closing years of the last century that it became a topic of discussion. Open consideration of active euthanasia began outside of the medical profession and only gradually became a subject of controversy among physicians. After a brief review of opinion regarding the hastening of death from antiquity to the middle of the nineteenth century, several factors which I believe fostered the open discussion of active euthanasia will be investigated.

Initially, it is important to clarify what is meant by the term euthanasia.[1] For the first two hundred years of its use in literature and medical writing the term did not imply the shortening of life. I shall call this earlier use of the term "spiritual" euthanasia since the word implied a state of mind at the time of death. The dying person was supposed to be tranquil and the means by which this was to be achieved included primarily physical comfort and moral support. This was essentially a passive process which was largely independent of the physician. The clergy, family and friends shared this process with the physician. I shall show that this word, euthanasia, acquired its modern connotation between 1870 and 1890. It is important to emphasize that this study does not attempt to document the actual practice of "active" euthanasia; rather it presents a set of circumstances which led to a formal

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[1] The term euthanasia is derived from the Greek εὐθανασία which literally translated means "easy death." The *Oxford English Dictionary* cities 1646 as the date of the first use of the term. An early medical definition of the word is "a soft easy Passage out of the World, without Convulsions or Pain." Stephen Blanchard, *The Physical Dictionary. Wherein the Terms of Anatomy, the Names and Causes of Diseases, Chirurgical Instruments, and their Uses, are accurately Described*. 5th ed. (London, 1708), p. 126.

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discussion and serious consideration of the feasibility of the practice for the first time.

Throughout history the moral acceptability and practice of certain acts relating to life and death have been intimately related. The consensus of any culture toward infanticide, suicide and murder during a specific period of its history is influenced by a number of social and moral factors. The concept of "active" euthanasia is closely associated philosophically with suicide. The practice of such acts is influenced largely by the value placed upon an individual human life as compared to the good of the total community.[2] In addition, attitudes toward death are important determinants of behavior of the dying person and those around him.[3]

Whereas suicide and infanticide were at times condoned in antiquity, the Hippocratic Oath forbids the participation of the physician in acts designed to shorten life: "I will neither give a deadly drug to anybody if asked for it, nor will I make a suggestion to this effect."[4] Edelstein asserts that this passage relates to suicide and the physician's responsibility in such cases rather than to murder. He emphasizes the widespread practice of suicide in antiquity as a means of escaping painful death and notes that poison would be most likely available from a physician. Edelstein concludes with regard to the practice of active euthanasia, "Throughout antiquity many people preferred voluntary death to endless agony. This form of 'euthanasia' was an everyday reality . . . [and] many physicians actually gave their patients the poisons for which they asked."[5]

The practice of suicide among the sick became unusual after about the second century A.D. Largely responsible for the change in philosophy toward suicide and its occurrence in this period was the importance placed upon individual life by Judeo-Christian teachings. This influence was especially important and went largely unquestioned until the middle of the nineteenth century. It accounts for the uniformity of Western opinion toward the prolongation of human life and acceptance of human suffering at the time of death.

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[2] An extensive review of the philosophical interpretations of self-death with emphasis on euthanasia has recently been published Historical trends from the Renaissance to the twentieth century are explored An extensive, annotated bibliography is included See Gerald J Gruman, "An historical introduction to ideas about voluntary euthanasia with a bibliographic survey and guide for interdisciplinary studies," *Omega*, 1973, *4* 87-138

[3] See Philippe Ariès, *Western Attitudes toward Death from the Middle Ages to the Present* (Baltimore Johns Hopkins Univ Press, 1974)

[4] Translation Ludwig Edelstein, *The Hippocratic Oath—Text, Translation, and Interpretation* (Baltimore Johns Hopkins Press, 1943 *Bull Hist Med* Suppl I)

[5] *Ibid* , pp 6-18 For a comprehensive and well-referenced review of suicide among the ill in this period see Danielle Gourevitch, "Suicide among the sick in classical antiquity," *Bull Hist Med* , 1969, *43* 501-518

A theoretical discussion of the principle of "active" euthanasia was put forth by Sir Thomas More in his *Utopia*, first published in 1516. In this work he writes, " . . . if a disease is not only incurable but also distressing and agonizing without cessation, then the priests and the public officials exhort the man . . . to free himself from this bitter life . . . or else voluntarily to permit others to free him. . . ."[6] Although the term euthanasia had not yet been coined, More clearly advocates the active form. There is no discussion of the physician's role, however More does outline certain precautions to be taken to avoid possible abuses of the practice, and briefly discusses the philosophical significance of the act. There does not seem to have been any impact on medical practice in this period as a result of More's writings. The physician was not the dominant figure in the death chamber until the nineteenth century when scientific advances began to erode the theological monopoly which had developed over the centuries.

The role of the physician in dealing with the dying patient received a significant amount of attention in Germany in the early nineteenth century. All aspects of German culture in this period were strongly influenced by "romanticism" and German medicine of the late eighteenth and early nineteenth centuries reflects the metaphysical orientation of romanticism.[7] As Temkin has noted, there was a preoccupation with death in German medicine as well at this time.[8] It is not surprising, therefore, that a number of dissertations published by German medical graduates in the early nineteenth century dealt with the physician's responsibility to his patient at the time of death. The role of the physician in promoting "spiritual" euthanasia is the subject of a number of dissertations.[9] Carl F. H. Marx, a medical graduate of Jena (1820) and later an academic physician and medical historian at Göttingen, published a paper in 1826 entitled "De euthanasia medica prolusio."[10] After noting the tendency of the physician to neglect his patient once it has been concluded the illness is terminal, the author urges the doctor to accept responsibility for his patient's "spiritual" euthanasia. This is to be accomplished by providing physical and moral comfort and by avoiding the use of heroic medications which are likely to be worthless. Whereas the author emphasizes the role of the physician in comforting the dying patient and urges the use of narcotics to relieve pain, he condemns the thought of hastening the death of the patient: "It is therefore the physician's duty to extend the span of life in every way, and least of all should he be permitted, prompted either by other people's requests or by his own sense of mercy, to end the patient's pitiful condition by purposely and deliberately hastening death. How can it be permitted that he who is by law required to preserve life be the originator of, or partner in, its destruction?"[11] By including this admonition Marx would imply that the practice of "active" euthanasia must have been contemplated at least, if not actually employed by some individuals. Open advocacy of "active" euthanasia was nearly half a century away at the time of Marx's essay. It is unlikely that there is any direct relationship of this movement in Germany toward a greater awareness of the physician's responsibility in providing "spiritual" euthanasia and the later conceptualization of "active" euthanasia in England and America.

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[6] Sir Thomas More, *Utopia*, from *The Complete Works of St Thomas More*, ed Edward Surtz and J H Hexter (New Haven Yale Univ Press, 1963), IV, p 186

[7] See Fielding H Garrison, "The romantic episode in German medicine," *Bull N Y Acad. Med* , 1931, *7* 841-864 Also W Leibbrand, *Die spekulative Medizin der Romantik* (Hamburg Classen, 1956), also K E. Rothschuh, "Deutsche Biedermeiermedizin Epoche zwischen Romantik und Naturalismus (1830-1850), *Gesnerus*, 1968, *25* (3-4) 167-187

[8] Erwin H Ackerknecht, "Death in the history of medicine," *Bull Hist Med* , 1968, *42* 19-23 See especially p. 22, footnote 9 (Temkin)

[9] More than a dozen dissertations on the subject of (spiritual) euthanasia published in Germany between 1820 and 1840 are listed in the *Index Catalogue of the Surgeon General's Office* (Washington, D C , 1883), 1st ser , IV, p 388

Just as Marx was reacting to the heroic medicine of the eighteenth century, a growing number of British and American physicians preached the doctrine of conservatism during the first half of the nineteenth century. In America Jacob Bigelow was among the earliest and most influential physicians to advocate this philosophy.[12] An important part of the concept of conservatism was the emphasis placed on palliation of disease. With the growing acceptance of this philosophy and the decline in application of heroic therapies, more attention was devoted to the comfort of the patient at the time of death.

In addition to this change in medical and therapeutic philosophy a number of other developments occurred in medical science and practice which set the stage for the open discussion of the concept and practice of "active" euthanasia in the closing decades of the nineteenth century. Among these developments were: the advances in physical diagnosis which combined with a more rational system of pathology to permit more accurate prognosis, the theoretical and practical advances in analgesic therapy, the discovery of anesthesia and its acceptance in relieving the pain of child-birth, and the impact of scientific discovery upon theology. Each of these factors will be considered briefly as they relate to the origins of the conceptualization of "active" euthanasia.

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[10] This paper has recently been translated into English, see Walter Cane, "Marx's 'medical euthanasia,' *J Hist Med*, 1952, 7 401-416 A biographical sketch of Marx with numerous bibliographical citations may be found in August Hirsch, *Biographisches Lexicon der hervorragender Arzte aller Zeiten und Volker*, 2nd ed (Berlin Urban und Schwarzenberg, 1932), IV, pp 105-107

[11] Cane, *op. cit* (n. 10 above), p. 413

[12] See Jacob Bigelow, *A Discourse on Self-Limited Diseases* (Boston N Hale, 1835), and *Brief Expositions of Rational Medicine* (Boston Phillips Sampson, 1858)

The first half of the nineteenth century witnessed a spectacular growth of sophistication in the diagnosis and prognosis of diseases. This was due in large part to advances in physical diagnosis pioneered by the French school, the introduction of statistical methods into medicine by Louis and emphasis on pathological correlations of clinical conditions. With these modalities physicians in the second half of the nineteenth century were equipped to prognosticate more accurately than ever before. To be sure, prognostication was by no means precise, but there was a growing body of scientifically derived information upon which a physician could rely in making diagnostic and prognostic decisions. Only when a patient's condition could be judged incurable with reasonable accuracy would it be feasible to propose a concept as radical as "active" euthanasia.

Drugs with some efficacy in relieving pain have been known and used for centuries.[13] It was not until the isolation of morphine by Sertürner in 1816 and the development of the hypodermic syringe by Pravaz and Wood in the early 1850s that potent analgesics could be administered in accurate doses and by a route where their action was very rapid. Practical experience with the hypodermic administration of morphine was gained in the American Civil War,[14] and a scientific study of the dosage and effects of subcutaneous administration of this drug was sponsored by the Royal Medical and Chirurgical Society of London in 1867.[15] These developments permitted the safe and effective control of pain resulting from illness as well as injury.

The discovery of anesthesia in 1846 was, of course, of monumental importance in the surgical treatment of disease. In addition, this discovery was promoted by a number of physicians for the control of pain in non-surgical conditions. Complete anesthesia was unnecessary for this purpose. Jacob

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[13] For a comprehensive survey of early modalities and drugs for the control of pain see E. S Ellis, *Ancient Anodynes—Primitive Anaesthesia and Allied Conditions* (London Heinemann, 1946).

[14] See *The Medical and Surgical History of the War of the Rebellion (1861-65)* (Washington, 1876), vol 2, part 2, pp 27 and 207.

[15] See "Report of the scientific committee appointed to investigate the physiological and therapeutical effects of the hypodermic method of injection," *Med -Chir Trans* , 1867, *50* 561-643

Bigelow encouraged the use of both analgesics and anesthetics to reduce the pain of disease: "The use of anodynes and anaesthetics, the obviation of various painful and distressing symptoms . . . may well afford employment to the most earnest physician."[16] Many of the physicians who advocated this practice were careful to note that it was not their intention to shorten life. Joseph Bullar, a British physician, reported the efficacy of opium in promoting "euthanasia" in terminal phthisis in 1856, and ten years later advocated chloroform for the alleviation of pain and suffering in the dying patient. In both of these reports Bullar was cautious to note that "a large dose might be hazardous." Furthermore, he declared that these interventions prolonged, rather than shortened, his patients' lives.[17] Here then, are examples of the use of agents to promote "spiritual" euthanasia which were soon advocated by others in the practice of "active" euthanasia.

Thus, several practical obstacles to the practice of "active" euthanasia had been overcome, at least in part. A patient could be judged incurable with greater accuracy than ever before and powerful modalities for the relief of pain and suffering had been developed. It would seem logical that someone should propose the practice of "active" euthanasia. There remain to be discussed, however, the moral and legal aspects of the practice. It is likely that these are largely responsible for the failure of the medical profession to either propose or openly discuss the practice. On the other hand, it would seem likely that some members of the profession actually practiced "active" euthanasia without attempting to advocate it publicly or in the medical press.

Theology and its impact on Western culture entered a difficult period in the second half of the nineteenth century. The growth of scientific knowledge and technology led to a reconsideration of traditional patterns of education and moral thought. There ensued in the second half of the century a period characterized by a decline in the importance of theology. An event which contributed significantly to the re-interpretation of traditional views toward religion and its teachings was the publication in 1859 of Charles Darwin's *Origin of the Species*. Shortly before, the introduction of chloroform anesthesia into childbirth by Simpson had caused major criticism from certain religious circles.[18] This religious criticism regarding the alleviation of pain in childbirth and surgery was successfully answered by Simpson and another potential impediment to the proposal of "active" euthanasia became less important.

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[16] Jacob Bigelow, "Brief expositions of rational medicine," in *Modern Inquiries* (Boston Little, Brown, 1867), pp 224-225

[17] Joseph Bullar, "On the use of small doses of opium in the act of dying from phthisis," *Assoc Med J London*, 1856, p 256, and idem, "Chloroform in dying," *Brit Med J* , July 7, 1866, pp 10-12

[18] See H Laing Gordon, *Sir James Simpson* (London Fisher-Unwin, 1898), pp 123-129, and James Simpson, *Answer to the Religious Objections Advanced against the Employment of Anaesthetic Agents in Midwifery and Surgery* (Edinburgh, 1847)

The initiation of open discussion and frank advocation of the practice of "active" euthanasia originated not from the medical profession, but rather from a school master and essayist in England. Samuel D. Williams, Jr., of Birmingham in 1870 published the first paper to deal entirely with the concept and its application.[19] Williams' essay was overlooked or ignored by the medical profession, at least initially. His formulation is concise and comprehensive, as illustrated by a brief extract from the essay:

> . . . in all cases of hopeless and painful illness it should be the recognized duty of the medical attendant, whenever so desired by the patient, to administer chloroform, or such other anaesthetics as may by and by supercede chloroform, so as to destroy consciousness at once, and put the sufferer at once to a quick and painless death; all needful precautions being adopted to prevent any possible abuse of such duty; and means being taken to establish beyond any possibility of doubt or question, that the remedy was applied at the express wish of the patient.[20]

This proposal requires no clarification, but two points are worthy of emphasis. Williams advocates "active" euthanasia in *all* willing patients with incurable and painful diseases; he does not limit its application to specific conditions. Furthermore, he states that it is the *duty* of the medical attendant, presumably the patient's physician, to provide "active" euthanasia to his patient. Anticipating moral and religious objections, Williams rejects traditional teachings on the value of human life: "It may well be doubted if life have any sacredness about it, apart from the use made of it by its possessor . . . and, indeed, seeing that life is so transitory a thing . . . it is hard to understand the meaning of the word 'sacred' when applied to it . . . life is a thing to be used freely and sacrificed freely."[21]

Another Englishman, Lionel A. Tollemache, an essayist who has been included among the rationalist philosophers,[22] wrote an essay in 1873 entitled "The New Cure for Incurables."[23] This essay was stimulated by Williams' paper as well as by the comments of Sir Thomas More regarding the concept of "active" euthanasia in his *Utopia*.[24] Much of the Tollemache's essay deals with possible moral and religious objections to "active" euthanasia, while little attention is focused on the medical aspects of the issue. The influence of Darwinism[25] and utilitarianism upon the author's conceptualization and rationalization of euthanasia is apparent: ". . . modern science informs us that in an overcrowded population there is a sharp struggle for existence, so that an unhealthy, unhappy and useless man is in a manner hustling out of being, or at least out of the means of enjoyment, someone who would probably be happier, healthier, and more useful than himself."[26]

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[19] Williams' essay entitled simply "Euthanasia" first appeared in a volume of papers published by a philosophical society in Birmingham, England, *Essays of the Birmingham Liberal Club* (Birmingham, 1870). The circulation of this volume appears to have been very small, however Williams essay was reprinted separately and three editions had appeared by 1872 A far wider audience, and one which included physicians and scientists, was reached when an extensive review including lengthy quotations from the original appeared in *Popular Science Monthly*, May 1873

[20] *Popular Science Monthly*, May 1873, p 91.

[21] *Ibid*, p 92

[22] See Joseph McCabe, *A Biographical Dictionary of Modern Rationalists* (London Watts, 1920), p 803

The initial response of the medical community to these essays was negligible.[27] This is partially explained by the limited circulation of the essays until the review in the *Popular Science Monthly* of Williams' article. The concept was revolutionary and may not have been taken seriously by the majority of physicians who happened to read of it. On the other hand, it will be shown that there is evidence that some physicians practiced "active" euthanasia but were reluctant to discuss it.

Some insight into the actions of the American medical profession with regard to "active" euthanasia in this period is provided by an editorial which appeared in a popular Philadelphia medical journal in 1873. The editor notes:

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[23] Originally published in the *Fortnightly Review*, London, Feb 1873, pp 218-230, the essay was reprinted in the author's volume of collected essays, *Stones of Stumbling* (London, 1884) Three subsequent editions appeared in 1885, 1887, 1895.

[24] Tollemache emphasizes this point in the introduction to his volume, *Stones of Stumbling*, and denies that he was advocating the practice as some of his critics had charged See "Letters to the Editor" in *Spectator*, London, Feb. 15 and 22, 1873.

[25] Here I refer to what has been called "social Darwinism" reflecting a trend which followed rapidly upon the publication of Darwin's works on evolution Charles Singer has noted this trend. "Evolutionary doctrine was soon extended also to man s habits, language, customs, religion, social organization, even in his ways of thinking " Charles Singer, *A History of Biology*, 3rd ed. (New York Schuman, 1959), p 316 Other publications include Richard Hofstadter, *Social Darwinism in American Thought*, 1860-1915 (Philadelphia Univ of Pennsylvania Press, 1944), John C Greene, *The Death of Adam Evolution and Its Impact on Western Thought* (New York New American Library, 1961), and Stow Peters (ed ) *Evolutionary Thought in America* (New Haven Yale Univ Press, 1950), especially Robert E L Faris, "Evolution and American Sociology," pp 160-180

[26] Tollemache, *Fortnightly Review* (n 23 above), p. 227.

[27] This conclusion is based upon a thorough review of more than two dozen of the most widely read British and American medical periodicals of the period. One American response which was published in a West Coast medical journal was little more than an announcement that the practice had been proposed (by Williams) See O. M Wozencroft, "Euthanasy," *Pacific Med & Surg Reporter*, 1874, 7 499-502

> Whether, when a patient is past all hope, a victim to a fatal disease, entailing great agony, when, as sometimes happens, he and the family alike beseech us to 'put an end to his misery,' ought we to do so? Generally we dodge the responsibility of answering, and at the same time meet the requirements of the case by prescribing such a dose of opium or other narcotic that will certainly plunge the patient into a profound sleep, whence he may or may not come forth . . . Personally, we have often been asked—and we presume our experience is by no means singular—by friends of the sick and by the patient himself, to administer some potent drug that would close the fearful struggle."[28]

There is no indication that this editorial was written in response to the essays of the English writers.

It would appear that the earliest body of physicians in America to openly deal with this question was the South Carolina Medical Association. In 1879 a committee studied the medical, religious, moral, ethical and legal aspects of "active" euthanasia. A synopsis of their conclusions appears in the Association's proceedings.[29] Among the conclusions of this committee were that there was no medical precedent to justify the sacrifice of life, such a practice could lead to widespread abuses, and "in the present state of society, the practice of Euthanasia was illegal, and could only be regarded as the practice of murder."[30] The response of other members of the Association present at the convention where this report was presented reveals the divergence of opinion with regard to the question. One of the members felt "Euthanasia was as sure to be accepted as was the doctrine of evolution and that would be as surely as the Copernican system in astronomy."[31] This same physician admitted having practiced "active" euthanasia and announced he would resort to it again under certain circumstances. The comments of one member illustrate a possible explanation for the paucity of published opinion regarding "active" euthanasia in this period: "Dr. Buist thought the public should not be enlightened upon this subject. . . . He hoped that nothing of the discussion would be published, as it would be calculated to give rise to wrong opinions."[32]

In the 1880s sporadic articles appeared in some of the more widely read medical periodicals in America. The emphasis of these articles, which were few in number, was on the legal aspects of "active" euthanasia. Although some physicians admitted having practiced "active" euthanasia and others expressed sympathy for the practice, the majority of articles were opposed to it. The concern with the legal aspects of the question continued to rise: " . . . we fear that even the benevolent intentions of the one who offers a patient the happy release will not make killing anything but homocide in the eyes of the law."[33] More formal considerations of the legal aspects of "active" euthanasia appeared in the 1880s. Henry Leffman, a Philadelphia physician, read a paper entitled "Euthanasia: a Consideration of the Permissibility of Terminating Life in Cases of Hopeless and Painful Illness" before the Medical Jurisprudence Society of Philadelphia in 1883.[34] The author of this paper did not draw any conclusions but read the essay in order to stimulate discussion of the practice.[35] A British physician and professor of legal medicine at the London Hospital, Charles Tidy, wrote in his comprehensive text of medical jurisprudence in 1882: "The law knows no such principle as that involved in the term 'Euthanasia.' The words of Mr. Justice Hawkins (R. vs. Paine, tried for the murder of Miss MacClean, C.C.C., 1880) are on this point explicit: 'It is equally criminal to accelerate by one hour the death of a person as to cause it.' "[36]

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[28] "The euthanasia," *Med & Surg Reporter*, 1873, *29* 122-123

[29] *Trans S Carolina Med Assn* , 1879, xiv-xvii See also a review of the *Transactions* which appeared in the Philadelphia weekly, *Med & Surg Reporter*, 1879, *41* 479-480.

[30] *Trans S Carolina Med Assn* , 1879, xv.

[31] *Ibid*

[32] *Ibid* p xvi

It is clear from this discussion that the major arguments in favor and against the practice of "active" euthanasia had been articulated by these few, early writers on the subject. The incidence of the practice is impossible to estimate; it is clear, however, that it did occur. Many of the physicians who wrote on "active" euthanasia in this period expressed concern that the issue could not be settled in light of the legal and moral aspects. In this atmosphere an editorial published in the prestigious *Boston Medical and Surgical Journal* articulated, and even condoned, a practice which may be considered a compromise, that of "passive" euthanasia. This refers to a failure to utilize heroic or extreme measures to prolong life in cases of incurable and painful illness. It represents a natural extension of the philosophy of conservatism which was promoted by Jacob Bigelow and others earlier in the century. The 1884 editorial stated: "Perhaps logically it is difficult to justify a passive more than an active attempt at euthanasia; but certainly it is less abhorrent to our feelings. To surrender to superior forces is not the same thing as to lead the attack of the enemy upon one's own friends. May there not come a time when it is a duty in the interest of the survivors to stop a fight which is only prolonging a useless and hopeless struggle?"[37]

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[33] "Homicide secundum artem," *Phila Med Times*, Dec 1, 1883, p 176

[34] No printed version of this address would seem to exist. See *Medico Legal J* , 1884-1885, *2* 313

[35] "The moral side of euthanasia," *J A. M A* , Oct 3, 1885, *5*, 382

[36] Charles Tidy, *Legal Medicine* (London Smith, Elder, 1882), I, p 279

Interest in the question of "active" euthanasia continued sporadically throughout the closing years of the nineteenth century. There were few substantive changes in the discussions of "active" euthanasia from this point forward. Emphasis continued to shift toward the legal aspects of the practice.[38]

I have attempted to show how the formalization of the concept of "active" euthanasia took place in the second half of the nineteenth centruy. The scientific and philosophical circumstances which fostered this conceptualization have been discussed. In addition, some consideration has been given to the evolution of meanings attached to the term euthanasia. The currently employed distinction between "active" and "passive" euthanasia was utilized in 1884. The controversy over "active" euthanasia which was initiated by Williams and Tollemache more than a century ago continues today with little likelihood of resolution in the forseeable future.

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[37] "Permissive euthanasia," *Boston Med & Surg J.*, Jan. 3, 1884, *110* 19-20

[38] See Louis J Rosenberg and N. E. Aronstam, "Euthanasia—a medicolegal study, *J A M A*, Jan 12, 1901, *36* 108-110. See also Clark Bell, "Has the physician ever the right to terminate life?" *Medico-Legal J*, 1896., *14*· 463-473

# "THE GREENING OF EPIDEMIOLOGY": SANITARY PHYSICIANS AND THE LONDON EPIDEMIOLOGICAL SOCIETY (1830-1870)[*]

DAVID E LILIENFELD

> *The death rate is a fact; anything beyond this is an inference.* William Farr, *Vital Statistics* (1897).

## *Introduction*

In a profound, far-sighted editorial in 1856, the *Lancet* proclaimed:

> The synthesis of population-returns may amuse the pure statistician in his closet; but rigid analysis is the proper task of the sanitary physician [1]

Considering that this statement was written barely one year after John Snow had completed his analysis of the 1853-54 cholera epidemic in England, one may wonder about its basis and the identity of these "sanitary physicians." The currently accepted view of the state of epidemiology between 1830 and 1870 is perhaps best illustrated by the description presented in a recently published introductory epidemiology textbook: there were only three isolated instances of epidemiological field work during that period—those of John Snow, William Budd, and Peter Ludwig Panum.[2] It has become apparent, however, that this "grand triumvirate" of 19th-century epidemiology was neither grand nor were they the sole epidemiologists at that time; rather, they represent the isolated memories of a period which saw epidemiological concepts blossom.

During this 40-year period, 1830-1870, comparative mortality studies, evaluation of smallpox inoculation and vaccination, and morbidity and mortality surveys were all frequently used by physicians. Concerned specifically with diseases and their etiologies, the sanitary physicians were the 19th-

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[*] This paper, which was supported in part by a grant from the Milbank Memorial Fund, was presented on April 13, 1977, at the 50th annual meeting of the American Epidemiological Society, Case-Western Reserve University, Cleveland. Awarded the 1977 Professor's Prize in the History of Epidemiologic Research of the Society for Epidemiologic Research, the paper was also presented at that Society's 11th annual meeting held at the University of Washington, Seattle, on June 15, 1977

[1] "Editorial," *Lancet*, 1856, *1* 161-162

[2] A M Lilienfeld, *Foundations of Epidemiology* (New York Oxford University Press, 1978), pp 20-31

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century counterparts of modern-day epidemiologists. As financial support for such endeavors had not yet been established, these pioneers had to earn their livelihood(s) by clinical practice; hence, the term "sanitary physicians." As a result of their personal efforts and an organization that they formed, the London Epidemiological Society, the eclectic discipline of "epidemiology" was born. In fact, the accomplishments of these sanitary physicians actually constitute the "Greening of Epidemiology."

Because of the inherent relationships between statistical methods and epidemiology, a brief review of relevant developments in medical statistics from 1800 to 1830 is necessary before proceeding further. The general growth of statistical-biometrical ideas during the period between 1800 and 1870 has been generally traced by A. M. and D. E. Lilienfeld.[3] Historically, though, these ideas can be considered to have begun with the work of Pierre Charles-Alexandre Louis in 1830. The use of medical statistics prior to Louis is not to be denied, for several of his predecessors and colleagues had calculated many "death rates." Of course, one cannot be too certain of what was meant by the term death rate, but it does not seem improbable that the modern meaning of the term was applicable. Nor can the theoretical advances in probability and statistics by the 18th-century French mathematicians, e.g. Laplace, be entirely overlooked. Nonetheless, before Louis, there had not been a vigorous application of statistical ideas to medicine; hence, the period of "statistical enlightenment" that Louis' work initiated, and the possible reasons for these developments during this period of time will be given first consideration, as epidemiology "greened" in the "statistical climate" of the early 19th century.

## *The Statistical Climate of the Early 19th Century*

Ackerknecht has noted that many medical advances of the early 19th century were probably the direct result of the French Revolution.[4] Since the Revolution itself symbolized a break with past traditions, customs, and laws, so, too, did the changes in French medical systems represent a break with their antecedents. Old theories were discarded and the "rational-armchair" approach to medicine, dating from Aristotle, was generally dispensed with. An important result was the beginning of the collection and analysis of medical statistics. Indeed, the vacuum in the French medical world created by this break with the past was filled by the statistical approach to medicine. Hence, medical statistics became an important aspect of the research activities of the center of French medicine, the Parisian medical school.

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[3] A M Lilienfeld and D E Lilienfeld, "What else is new?" *Am J. Epidemiology*, 1975, *105* 169-179.

[4] E H Ackerknecht, *Medicine at the Paris Hospital 1794-1848* (Baltimore Johns Hopkins University Press, 1967), p 48

Pinel, famous for his work on insanity and among the first to employ statistical methods in medicine, perhaps best represents this statistical orientation.[5] He was among the first to organize a nosology, as well as to actually collect numerical data for analysis.[6] However, one should not be misled into believing that Pinel was the only one in the Parisian school to use statistics, as Bayle and Laennec were also frequent users.[7] It is highly probable that there were other less-prominent users of statistics during the 25-year (1790-1815) gestation period of the Parisian school.

Although such early utilization of statistical methodology by the Parisian school is well documented in historical analyses, it is clear that it was not an immediately popular technique; rather, it had to await the arrival of Dr. Pierre Charles-Alexandre Louis. Louis, whose epidemiologic endeavors have been almost totally forgotten by both historians and epidemiologists, was a key figure in sowing the seeds of epidemiology. As an epidemiologic pioneer, he, more than any other individual during the 19th century, was responsible for the development of current concepts of epidemiologic reasoning, which have only been refined during the past 150 years. During his 30-year teaching career, Louis, who never held an academic appointment, was to train many of the leaders of the next generation of epidemiologists (Figure 1).[8]

Louis was oriented towards a notion that runs throughout the 19th-century epidemiologic thought, that is, the idea of a "law of nature," or as it became known later in the century, a "law of mortality" or a "law of vitality." These "laws" were not necessarily exact methematical relationships, such as Gauss' Law. Rather, they were generalized expressions of inductive epidemiologic reasoning from observed facts; for example, the germ theory was the generalization based on the fact that several diseases, i.e. syphilis, smallpox, and cholera, had all been shown to be caused by specific contagia viva.[9] Louis best expressed this idea in a letter to the father of one of his students, James Jackson, Jr.:

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[5] *Ibid.*, p 47

[6] *Ibid.*, p 104

[7] *Ibid.*, pp 103-104

[8] *Ibid.*, pp 192-193, Lilienfeld and Lilienfeld, *op cit* (n 3 above), p 176

[9] D E Lilienfeld, "Contagium virum and the development of water filtration," *Preventive Med.*, 1977, 6, 361-375

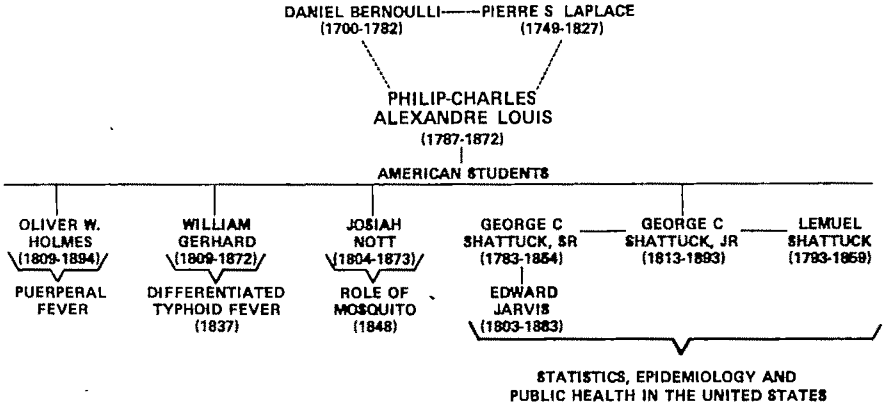
DANIEL BERNOULLI (1700-1782)
PIERRE S LAPLACE (1749-1827)
PHILIP-CHARLES ALEXANDRE LOUIS (1787-1872)
AMERICAN STUDENTS
OLIVER W. HOLMES (1809-1894)
PUERPERAL FEVER
WILLIAM GERHARD (1809-1872)
DIFFERENTIATED TYPHOID FEVER (1837)
JOSIAH NOTT (1804-1873)
ROLE OF MOSQUITO (1848)
GEORGE C SHATTUCK, SR (1783-1854)
EDWARD JARVIS (1803-1883)
GEORGE C SHATTUCK, JR (1813-1893)
LEMUEL SHATTUCK (1793-1859)
STATISTICS, EPIDEMIOLOGY AND PUBLIC HEALTH IN THE UNITED STATES

Figure 1. Louis' Influence on Americans

> Think for a moment, Sir, of the situation in which we physicians are placed. We have no legislative chambers to enact laws for us. We are our own lawgivers; or rather we must discover the laws on which our profession rests. We must *discover* them and not invent them; for the laws of nature are not to be invented. . . .[10]

Louis refused to accept the concept of statistical hypotheses.[11] Indeed, he was very inflexible to adopting any newly developed statistical methods. One example of this is found in his treatment of Jules Gavarret's work on statistical theory, in which 99% confidence intervals were derived and applied to Louis' data.[12] Although criticized by Gavarret for his refusal to use these 99% confidence intervals, Louis held firm.[13] Louis' arguments were:

> The calculus, as I employ it, does not efface differences: it supposes them; it limits itself to combining similar unities in order to compare them with parallel unities, these being subjected to somewhat different influences; that if, after all, as has been before remarked, it should be necessary that facts should be combined, which are not strictly similar; the error will be distributed through the different groups or classes of facts, and will be equalized; so that a comparison can be instituted between several groups without altering the result.[14]

Perhaps Louis had a better understanding of the value of theoretical statistics when applied to epidemiology than we do today!

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[10] A J. Bollet, "Pierre Louis the numerical method and the foundation of quantitative medicine," *Am J Med Sci.*, 1973, *266* 101.

[11] Ackerknecht, *op. cit* (n. 4 above), p 104.

[12] J. Gavarret, *Principes généraux de statistique médicale* (Paris: Bechet jeune et Labé, 1840), pp. 155-157.

[13] Ackerknecht, *op. cit* (n. 4 above), p 104.

[14] L. S. King, *A History of Medicine* (Middlesex: Penquin Books, 1971), p 202.

Louis, then, represents a major root in the "Greening of Epidemiology." His popularization of the numerical method led to a bold new concept within medicine: the epidemiological approach to disease, which may best be illustrated by Louis' comments on the question of inheritance of phthisis, made in 1836:

> . . . to determine the question satisfactorily, tables of mortality [life-tables] would be necessary, comparing an equal number of persons born of phthisical parents with those in an opposite condition.[15]

Louis was not the first to use statistics; rather, he was the first to make them the basis of medicine. Perhaps the best statement regarding his development and use of numerical method was made in 1837:

> The method of analysing medical cases may be said to have originated with him. . . . This system, although strongly opposed by many, is gradually gaining ground, as will appear by referring to several recent publications. It has been adopted by Andral in the last edition of his clinique, as well as by Chomel. Even Bouillaud has given his adhesion, and undertaken its defense. . . .[16]

Two of Louis' students, William A. Guy and William Farr, were responsible for the development of epidemiology in England in the mid-1800s. In fact, both of these men were to strongly influence this development by becoming intimately associated with a relatively young English statistical organization, the Statistical Society of London.

The Statistical Society of London (SSL), predecessor of the present Royal Statistical Society, was formed in 1834 at the suggestion of the British Association for the Advancement of Science.[17] It was organized to stimulate the communication of ideas between members at its meetings. It held three types of meetings: ordinary, which were held monthly between November and July; anniversary, which were held annually on March 15; and special, which were held at the request of ten or more Fellows.[18] The ordinary meetings were "to admit Fellows, to read and hear reports, letters, and papers on subjects interesting to the Society."[19] The anniversary meetings were to elect the officers of the Society and to discuss questions on its rules and management. Special meetings could be devoted to any subject.

The governing body of the SSL was its "Council."[20] The Council's mem-

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[15] P Ch A. Louis, Review in *Pathological Researches on Phthisis, Am J Med Sci.*, 1838, *19* 445-449

[16] *Ibid* , p 449

[17] F. M Mouat, "History of the Statistical Society of London," *J Statistical Soc* , 1885, Jubilee Volume, pp 15-16

[18] *Ibid* , pp 68-69.

[19] *Ibid* , p 68

[20] *Regulations of the Statistical Society of London* (London F W Calder, 1835), p. 7

TABLE 1
Some of the Founding Fellows of the Statistical Society of London (24)

| | |
|---|---|
| 1. Charles Babbage | —inventor of the first mechanical calculating machine |
| 2. Edwin Chadwick | —sanitary reformer |
| 3. James Copland | —a future member of the London Epidemiological Society |
| 4. Benjamin Gompertz | —discoverer of the "Gompertz Curve", which described the mortality experience of a population |
| 5. J. R. McCulloch | —author of *Statistical Account of the British Empire* |
| 6. Robert R. Roupell | —future member of the London Epidemiological Society |
| 7. William Henry Sykes | —future member and vice-president of the London Epidemiological Society |
| 8 M Auguste Quetelet | —eminent French statistician |
| 9. Thomas Watson | —future member of the London Epidemiological Society and eventually the leading physician in England |
| 10. T. R. Malthus | —demographer |

bership reflected both the orientation of the SSL and the current state of the art of statistics as a discipline. For example, Dr. Bisset Hawkins, a physician and author of an 1829 book, *Medical Statistics,* was one of the SSL's founders and a member of its first Council.[21] However, the statistical methods used in *Medical Statistics* were very unsophisticated consisting, generally, of the computation of averages. The original Fellows were statistical pioneers; some of them became prominent in the development of vital statistics, epidemiology, or both (Table 1).[22]

The SSL initially published an annual bulletin, which contained a list of its annual membership, its officers and Council members, who were, like other English societies, elected to their respective positions on an honorary basis.[23] The SSL concerned itself with five major areas of statistics, listed in Table 2.[24] It should be noted that "Medical Statistics" is one of these categories.

The SSL's activities during its first year, 1835, were not extensive. About the only event of note was the death of Malthus.[25] During the next two to

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[21] *Ibid*., p 17

[22] *Ibid*., pp. 13-22.

[23] Mouat, *op. cit* (n. 17 above), pp. 41-42

[24] *Ibid*., pp 43-44.

[25] M. J Cullen, *The Statistical Movement in Early Victorian Britain* (New York Barnes and Noble, 1975), pp. 88-90

TABLE 2
Areas of Concentration for the Statistical Society of London at its Founding in 1834 (26)

I. *The Statistics of Physical Geography Division, and Appropriation,* or, geographical and proprietary Statistics.
II. *The Statistics of Production;* or, agricultural, mining, fishery, manufacturing, and commercial Statistics.
III. *The Statistics of Instruction;* or, ecclesiastical, scientific, literary, university, and school Statistics.
IV. *The Statistics of Protection,* or, constitutional, judicial, legal, military, and criminal Statistics
V *The Statistics of Life, Consumption, and Enjoyment* ; or, of population, health, the distribution and consumption of the commodities of life, and public and private charity

three years, the SSL gradually established itself and began to grow, although it was not until 1838 that any significant activities were begun. At that time, the society discontinued its bulletin and began to publish an annual journal, the *Journal of the Statistical Society* (JSS), which included papers presented at the ordinary meetings.[26] Also, F. G. P. Neison, an actuarian, and W. A. Guy were elected Fellows of the Society.[27] Neison was oriented towards theoretical aspects of statistics; he appears to be the only member with this interest. He was, insofar as the author can ascertain, the first to develop and use the concept of age-standardization.[28] He symbolically derived and demonstrated their use at one of the ordinary meetings; its subsequent publication made the concept of age-adjustment known to all of the Fellows. Neison also symbolically derived estimates of the English population between two censuses; this, too, was demonstrated at an ordinary meeting and, subsequently, published.[29] Probably Neison's most important contribution to medical statistical analysis was his demonstration of the errors in Edwin Chadwick's use of the "mean age at death" as an index of health.[30] The data used in this demonstration were from Bethnal Green and Marylebone, which had mean ages at death of 25.8 and 29.12 years, respectively.[31] Neison age-standardized the death rates of these two regions by applying the age-specific death rates of Marylebone to Bethnal Green's age-specific population. He then calculated the mean age at death for Marylebone, which was 24.52; this was lower than its average of 29.12, thus demonstrating that Chadwick had erred in his statistical methodology. Strangely, Chadwick persisted in his folly even after Neison had demonstrated this error.

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[26] "Introduction," *J Statistical Soc* , 1838, *1* i-ii

[27] Cullen, *op cit* (n 25 above), pp. 102-103, R W. Rawson, "International statistics," *J. Statistical Soc* , 1885, *48* 505-506.

[28] F G. P Neison, "On a method recently proposed for conducting inquiries into the comparative sanatory condition of various districts," *J Statistical Soc* , 1844, *7* 40-68.

[29] Idem, "Contributions to vital statistics," *J Statistical Soc* , 1845, *8* 290-343.

[30] M Greenwood, *Some British Pioneers of Social Medicine* (London Oxford University Press, 1948), pp 55-56.

[31] *Ibid* , p 56.

It is interesting that, in another instance, before Chadwick presented his analysis to the SSL, he had shown it to William Farr for his comments. Farr agreed with both Chadwick's method of analysis and his inferences. After the presentation, Chadwick published the analysis; the *Lancet* published a sharp editorial attacking both Chadwick's methods and inferences. However, Chadwick was later surprised to find out that the editorial was written by Farr![32]

W. A. Guy was able to integrate his medical background with his statistical knowledge in a manner similar to that of Major Greenwood. Guy, born in 1810 and died in 1885, came from a family of physicians.[33] He spent two years as a student at Heidelberg and at Paris before receiving an M.B. degree from Cambridge in 1837. In 1838, he was appointed Professor of Forensic Medicine at King's College, London. A description of Guy at this critical period best summarizes his role as a pioneer of modern biostatistics:

> "And there was Guy, with his wide range of interests and his extensive statistical material, delving into such problems as 'The influence of seasons, and the weather on sickness and mortality', into medical education, into occupational disease."[34]

This last item, occupational diseases, was a major interest of Guy's until the 1850s, as it was for most of the SSL's membership.

The groups which were examined most frequently were military or governmental workers, such as artillery men or policemen; the basic reason for this was that the information necessary for analysis was readily available for such groups. Indeed, it was not until the late 1800s that a sufficient body of data had accumulated for similar analyses of the entire population of England. The designs used in such occupational studies tended to be very simple; and, unless one is willing to refer to one group in the study population as a control group, no control groups, in the modern sense, were used. The general approach was to classify the numbers of deaths (or sicknesses) from a given disease according to certain categories, such as, rank, age, sex, years in service, etc. A tabulation was then made for the population in each stratum. Mortality rates were calculated and compared, both in a relative manner, akin to relative risk, and in a differential manner, similar to excess risk. In general, the cross-classifications used are of a type expected of any modern epidemiologist. Sometimes, life-tables would be computed and compared for the various groupings. In general, when such laborious calculations as those involved in the construction of a life-table were needed, Guy or any other SSL member asked Neison to compute them.[35] Brockington views Guy's work in occupational epidemiology:

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[32] D. V Glass, *Numbering the People* (Farnborough. D C. Heath, 1973), p 149

[33] L Stephen and S Lee, *Dictionary of National Biography*, 1st ed (New York· MacMillan, 1908), vol 8, p 835

[34] F N L Poynter, *Medicine and Science in the 1860's* (London: Wellcome Institute of the History of Medicine, 1968), pp 185-196

> . . . the study of its (occupational) effects (arsenite of copper) . . . was brilliantly undertaken by Dr. Augustus Guy, one of the earliest epidemiologists to use statistical methods.[36]

In fact, these statistical methods, which allowed Guy to study occupational diseases, resulted from his two years in Paris under Louis. Indeed, Guy stated in 1838:

> Does not the single fact, that medicine required a Louis to teach the advantages of the 'methode numerique', and to set an example of its employment, so long after its introduction into the more advanced sciences, prove the necessity of making those sciences an example to our own.[37]

In 1870, Guy also proclaimed, in a book entitled, "*Public Health*" (perhaps the first book of its kind):

> Of this numerical method which the French physician, Louis, so largely applied, and Gavarret so ably explained and illustrated, I have now to treat, as it lends itself to the science of hygiene. . . .[38]

The necessity of the use of the numerical method is clearly expressed:

> As hygiene deals with mankind not one by one, but in masses, its scientific method can be no other than the numerical so often confounded with its leading application—statistics. If this word now meant what it orginally did . . . then hygiene would take rank among its leading subdivisions as applying the great State-policy of prevention to health and disease.[39]

Reflective of his lifetime work, such statements place Guy in the position of being the epidemiological conscience of the 1800s. Hence, it is not too

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[35] W. A Guy, "On the duration of life among English gentry, *J Statistical Soc.*, 1846, *9* 37-49

[36] F Brockington, *World Health*, 3rd ed (Edinburgh· Churchill Livingstone, 1975), p. 81

[37] W. A. Guy, "On the values of the numerical method as applied to science, but especially to physiology and medicine, *J Statistical Soc* , 1839, *2* 45

[38] Idem, *Public Health* (London Renshaw, 1870), p 8

[39] *Ibid*

TABLE 3
Expectations of Life in Years at Various Ages for Four Professions (44)

| Age | Sovereigns | Peerage and Baronetage | Gentry | Professions |
|---|---|---|---|---|
| 20 | 34.3 | 38.5 | 37.3 | .... |
| 30 | 27.3 | 30.9 | 31.2 | 33.9 |
| 40 | 20.9 | 24.4 | 24.9 | 26.0 |
| 50 | 15.0 | 17.9 | 18.4 | 18.9 |
| 60 | 10.5 | 12.6 | 12.8 | 12.8 |

surprising that, in 1843, he was elected an honorary secretary of the SSL, a post he held until 1868.[40] His studies of occupational mortality are worthy of publication today. Indeed, Guy's brilliance, unclouded by meaningless statistical tests of significance, is unmistakable. For example, in comparing the duration of life among sovereigns with that of other professionals, Guy used a life table to note that " . . . sovereigns, as a class, are among the shortest lived of human beings" (Table 3).[41]

Also, in 1856, in *"On the Nature and Extent of the Benefits Conferred by Hospitals on the Working Classes and the Poor,"* Guy raised the question of what modern epidemiologists refer to as a "Berksonian bias":

> There are two questions especially to which I am not aware that any answer has yet been given; nor has any collection of facts been made with a view to furnish a reply. The first question refers to the class of persons who resort to hospitals; the second to the proportion which that class forms of the population to which they belong.[42]

Lastly, Guy was to conduct the first known Monte Carlo simulation experiment to empirically determine whether the statistical theories developed by Gavarret on the probabilities of an event occurring by chance had any practical application to the numerical method.[43] The simulation used a known population from which samples were randomly drawn. The samples were then analyzed. The inferences concerning the population based upon the analyses were then compared with the population, in an effort to ascertain the quality of the method of analysis used. Concerning this experiment, Guy said:

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[40] Stephen and Lee, *op. cit.* (n. 33 above), p. 836.

[41] W. A. Guy, "Duration of life of sovereigns," *J. Statistical Soc.*, 1847, *10* 62-69.

[42] Idem, "On the nature and extent of the benefits conferred by hospitals on the working classes and the poor," *J. Statistical Soc.*, 1855, *5* 315-323; J. Berkson, "Limitations of the application of fourfold table analysis to hospital data," *Biometrics*, 1946, *2* 47-53.

[43] W. A. Guy, "On the analogy between the aggregate effects of operation of the human will and the results commonly attributed to chance," *J. Institute of Actuaries*, 1855, *5* 315-323.

> Now, unless I am greatly mistaken, no attempt of any kind has yet been made to show that rules and calculation derived from abstract reasonings upon probabilities, backed a few experiments on occurrences brought about by what is commonly designated 'chance', are applicable to events of a totally different order, brought about by the operation of the human will or by the multitudinous external influences, which, acting on the human frame, preserve it in health or give rise to the diseases which impair its vigour and ultimately destroy it.[44]

Guy served as Dean of the medical faculty at King's College from 1846 to 1858.[45] He was, however, intimately involved with the SSL throughout his life, serving as editor of the JSS from 1852 to 1856, vice-president from 1869-72, and as president from 1873-75. The medical profession recognized his contributions by inviting him to give three lectures at the Royal College of Physicians; his election as vice president of the Royal Society in 1876 was the final mark of honor in a truly illustrious career.

The best illustration of Guy's influence on hygiene can be seen from the academic positions that he held. Although Guy was never to leave King's College in London, his professorship was to be changed in name many times; in the 1830s when the SSL was founded, Guy was a "Professor of Forensic Medicine."[46] In the 1840s he became the "Professor of Forensic Medicine and Hygiene," and by the 1870s his title had again been changed to "Professor of Hygiene."[47]

Along with Guy, William Farr was among the titans of mid-19th-century epidemiology. Farr, the mastermind of the Registrar General's Office, has been characterized by Eyler as a "social pathologist."[48] His work, though, did not confine itself to the limits of any given discipline. In addition to establishing the first modern national registration system, analyzing data collected by that system and serving as a member of the SSL Council, Farr, together with Guy, must be considered among the founders of the English school of statistics. His influence extended beyond his written contributions, through his personal relationships with Galton and Florence Nightingale.[49]

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[44] *Ibid.*, pp 316-317

[45] Stephen and Lee, *op cit* (n 33 above), p. 835.

[46] *Ibid.*, p 835, Cullen, *op cit* (n 25 above), p 102

[47] "Miscellanea," *J Statistical Soc.*, 1885, *48* 650

[48] J. M Eyler, "William Farr, An Intellectual Biography of a Social Pathologist" (Ph D Thesis, University of Wisconsin, 1971)

[49] *Ibid.*, pp. 262-271, F Galton, *Memories of My Life* (London Methuen, 1908), p 292

However, Farr's work has not yet been placed in the perspective that it deserves.

Farr, like Guy, was a product of the Parisian school of medicine, a student of Louis.[50] As a result of that experience, he apparently was also instilled with Louis' beliefs in the concept of "law of nature" as indicated in his following statement, which so closely resembles Louis' thinking:

> Thus we learn in the same circumstances the same number of people die at the same ages of the same diseases, year after year; organized bodies governed by laws as fixed as those which govern the stars in their courses.[51]
>
> The deaths and causes of death are scientific facts which admit of numerical analysis; and science has nothing to offer more inviting in speculation than the laws of vitality, the variations of those laws in the two sexes at different ages, and the influence of civilization, occupation, locality, seasons, and other physical agencies, either in generating diseases and inducing death, or in improving the public health.[52]

Indeed, it was this belief in "laws of vitality" that enabled Farr to study disease phenomena in a statistical manner, searching for these laws by constructing life tables, etc.[53] For it was Farr who referred to a life table as the "biometer" because of its ability to measure life.

Farr returned to England from Paris in 1831. After his unsuccessful attempt to establish a private medical practice, he presented, in 1832, a series of lectures on medical statistics which were poorly received.[54] Farr published the lectures in the *Lancet* in 1833, where they were favorably received. In the same year, Farr wrote an article on vital statistics for McCulloch's *Statistical Account of the British Empire*.[55] This article was very favorably received and placed Farr in the forefront of English statistics. In 1839 he was appointed to the Registrar General's Office as the Compiler of Abstracts.[56] In 1837 he stated that sickness should be studied by "taking one hundred thousand persons of given ages, indiscriminately, and observing them for one, two, three, etc. years."[57] This is a clear statement of what is today regarded as a prospective study. Indeed, Farr's grasp of epidemiologic concepts was extraordinary as is presented in Table 4.[58]

[50] Lilienfeld and Lilienfeld, *op cit* (n. 3 above), pp. 175-176

[51] William Farr, *Vital Statistics*, 2nd ed (New York New York Academy of Medicine, 1975), p 142

[52] *Ibid* , p 213.

[53] Eyler, *op cit* (n 48 above), p 30

[54] L. Stephen and S. Lee, *Dictionary of National Biography*, 1st ed. (New York MacMillan, 1908), vol 6, p. 1090, Farr, *op cit* (n. 51 above), pp. xi-xii

[55] Farr, *op. cit* (n. 51 above), p xii

[56] Eyler, *op. cit* (n. 48 above), p. 30, Stephen and Lee, *op cit* (n. 54 above), p 1090

[57] J. R McCulloch, *A Statistical Account of the British Empire* (London Charles Knight, 1839), p. 546

[58] Farr, *op cit* (n 51 above), pp 127, 128, 129-130, 142, 169, 187, 320, McCulloch, *op. cit.* (n. 57 above), p. 571, Glass, *op. cit* (n 32 above), p 148

TABLE 4
Examples of William Farr's Understanding of Epidemiologic Concepts

| Epidemiologic Concept | Farr's Statement |
|---|---|
| Scope of epidemiology | "The causes that make the rates of mortality vary may be considered under two heads—<br>(1) Causes inherent in the population itself, such, for exple, as *sex* and *age*<br>(2) Causes outside the population, such as air, water, food, clothing, dwellings, or such groups of causes as are involved in residence, and relation of the several parts to each other in time and space." (66) |
| Person-years | "A year of life is the lifetime unit. It is represented by one person living through a year, or by two persons living through half a year." (67) |
| Relationship of death rate and probability of dying (or living) | ". . . the rate of mortality serves to give the probability of living a year . . ." (68) |
| Standardized mortality rate | "[If] the number of boys under 5 years of age was 147,390; the annual rate of mortality in the healthy districts [the standard population] was .04348, . . 6367 deaths which would have happened in London . . . continuing the process . . . the mortality in London should be 15 in 1,000 . ." (69) |
| Dose-response effect | ". . . the effects are in some regulated proportion to the intensity of the causes . . ." (70) |
| Need for large numbers of population and biological inferences | ". . . When the number of cases is considerable the relative mortality is most correctly expressed and . . . slight differences deserve little attention." (71) |
| Prevalence = incidence × duration | ". . . in estimating the prevalence of disease, two things must be distinctly considered; the relative frequency of their attacks, and the relative proportion of sick-time they produce. The first may be determined at once, by a comparison of the number of attacks with the numbers living; the second by enumerating several times the living and the actually sick of each disease, and thence deducing the mean proportion suffering constantly. Time is here taken into account and the sick-time, if the attacks of two diseases be equal, will vary as their duration varies, and whatever the number of attacks may be, multiplying them by the mean duration of each disease will give the sick-time." (72) |
| Herd immunity | "The small-pox would be . . . sometimes arrested, by vaccination, which protected a part of the population . . ." (73) |
| Retrospective and prospective studies | "Is your inquiry to be retrospective or prospective? If the former, the replies will be general, vague, and I fear of little value . . ." (74) |

The initial organization of the English vital registration system was one of Farr's greatest accomplishments. Among the first of the many tasks necessary for establishing such a system, which he performed in 1839, was developing a nomenclature of diseases.[59] One should not forget Farr's relationship to Louis, whose meticulousness with nomenclature led to his identification of typhoid fever. Farr's own view may be seen by his 1841 comparison of nomenclature to physical units: "The nomenclature is of as much importance . . . as weights and measures in the physical sciences. . . ."[60] In 1853, with d'Espine, Farr developed the anatomically-based system that is the basis of today's International Classification of Diseases.[61]

The immediate effect of Farr's association with the SSL was to change the SSL's orientation from matters of political arithmetic to those of medical and vital statistics. This can best be seen in the contents of the society's journal, the JSS. However, one should not infer that no previous medical statistical work had been done by the SSL, as William Augustus Guy had, for the previous two years (1837-39), been presenting papers at its meetings on such subjects.

Two other Fellows of note were Col. William Henry Sykes and Dr. James Bird. Col. Sykes was the SSL's first vice-president.[62] He was also closely associated with the British Association for the Advancement of Science, serving from time to time as its vice-president.[63] Sykes presented an occasional analysis of mortality experience in the Army to the SSL.[64]

Dr. James Bird served in the British East Indian Company until 1847.[65] During that time, he found sufficient free time to master Persian and then translate, from Persian to English, a book, *Political and Statistical History of Gujerat*. In 1847, he retired from the Army, returning to London as the "chief authority" on "diseases of Europeans in tropical climates."[66] In that same year, he joined the SSL and shortly thereafter became a Fellow.[67]

By the mid-19th century, the SSL was the thriving center of biostatistical and epidemiological studies. During the late 1840s epidemiology had begun to emerge as a separate discipline, distinct from biostatistics. The SSL and its activities were well known to medical men; medical journals consistently reported on any medically relevant paper that was presented at an SSL meeting.[68] Further, many physicians were members of the SSL.

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[59] Eyler, *op. cit.* (n. 48 above), p. 224.

[60] Farr, *op. cit.* (n. 51 above), p. 234.

[61] *Ibid.*, p. 250; Eyler, *op. cit.* (n. 48 above), pp. 226-227.

[62] W. Farr, "President's inaugural address," *J. Statistical Soc.*, 1872, *35*: 429-430.

[63] "Twelfth meeting of the British Association for the Advancement of Science," *J. Statistical Soc.*, 1843, *6*: 76.

[64] Cullen, *op. cit.* (n. 25 above), pp. 85-104.

[65] D. Power, *Plarr's Lives of the Royal College of Surgeons of England*, (London: John Wright, 1930), vol. 1, p. 100.

[66] *Ibid.*, p. 100.

[67] "Proceedings of the Statistical Society of London," *J. Statistical Soc.*, 1849, *12*: 80.

One interesting paper brought to the medical world's notice by the *London Medical Gazette* in 1850 was one of Alexander MacLaren, entitled "Origin and Spread of Cholera in the Eighth District of Plympton St. Mary, Devon."[69] The *Gazette* stated that:

> . . . Mr. MacLaren's inquiry illustrated the advantage of adopting the statistical method of submitting to numerical tests the various and disputed facts relative to cholera.

This indicates that the eipdemiologic approach to disease phenomena had been established.

In 1848-49, England, with the rest of Europe, was bracing itself for its second cholera epidemic. The time had come for the sanitary physicians to apply, in an epidemiological manner, the statistical methodology developed by the SSL to epidemic diseases, especially cholera. An association was needed both to bring the sanitary physicians into contact with one another and to co-ordinate their activities. In 1850, in response to the cholera epidemic, the London Epidemiological Society was formed.

### *The Epidemiological Atmosphere of 1850-1870*

By 1850, epidemiological concepts had been elucidated sufficiently for most medical men to use them in day-to-day research. For example, in 1851, Dr. John Webster delivered a paper to the Medical Society of London, of which John Snow was a member, "On the Health of London during the Six Months Terminating March 29, 1851."[70] Webster discussed, in part, the mortality in the surgical and medical services of nine London hospitals (generally those for which such data was available). His findings are presented in Table 5.[71] Referring to the mortality rates for the six-month period (column c, Table 5), Webster noted for the six-month mortality rate:

> Respecting the proportion of deaths recorded, speaking relatively, the ratio was higher amongst medical patients, at all the metropolitan hospitals, than in persons treated by surgeons . . . and notwithstanding, the amount of deaths (column b) was

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[68] Poynter, *op cit* (n 34 above), pp 298-299

[69] "Statistical Society of London," *London Med Gaz.*, 1850, *45* 254-255

[70] "Medical Society of London," *Lancet*, 1851, *1* 516-518

[71] *Ibid*, p 603.

TABLE 5
Statistics of In-patients of Nine General Hospitals in London, During Six Months Terminating at the End of March, 1851 (88)

| Hospital | | (a) Admitted | (b) Died | (c) Death rate per 100 | (d)* Difference in death rates | (e)* Ratio of death rates | (f)* Ratio of deaths |
|---|---|---|---|---|---|---|---|
| St. Bartholomew | Medical | 891 | 121 | 13.58 | 8.57 | 2.71 | 1.34 |
| | Surgical | 1,796 | 90 | 5.01 | | | |
| St Thomas | Medical | 913 | 92 | 10.07 | 5.99 | 2.47 | 1.84 |
| | Surgical | 1,224 | 50 | 4.08 | | | |
| London | Medical | 528 | 63 | 11.93 | 7.31 | 2.58 | 1.00 |
| | Surgical | 1,362 | 63 | 4.62 | | | |
| St. George | Medical | 691 | 82 | 11.87 | 6.14 | 2.07 | 1.82 |
| | Surgical | 785 | 45 | 5.73 | | | |
| Middlesex | Medical | 627 | 51 | 8.13 | 4.07 | 2.00 | 1.96 |
| | Surgical | 640 | 26 | 4.06 | | | |
| Westminster | Medical | 458 | 40 | 8.73 | 5.28 | 2.53 | 3.64 |
| | Surgical | 318 | 11 | 3.45 | | | |
| Charing-Cross | Medical | 179 | 26 | 14.52 | 10.91 | 4.02 | 1.63 |
| | Surgical | 443 | 16 | 3.61 | | | |
| Royal Free | Medical | 95 | 7 | 7.05 | 3.39 | 1.93 | .88 |
| | Surgical | 219 | 8 | 3.66 | | | |
| King's College | Both | 659 | 59 | 8.96 | — | — | — |
| Grand Total | Both | 11,838 | 850 | 7.18 | — | — | — |

* Computed by the author

identical in both categories, at the London (Hospital), the comparative result (column e) was similar to that elsewhere observed.

"Comparative results" refers to the relative risk, i.e. the ratio of two mortality rates. Note that neither the differences in the death rates (column d) nor the ratio of deaths (column f) show the noted consistency that the ratio of rates (column e) does. Webster's approach, which was epidemiological, did not pass unnoticed, as the *Lancet* published it with an editorial lauding his findings:

> . . . the general average of deaths being about twice as numerous, speaking comparatively, in the medical, as in the surgical department.[72]

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[72] "Editorial, *Lancet*, 1851, *1* 630.

TABLE 6

Biases Noted by Dr. Routh in the Homeopathic Analysis of Medical Practice (92)

1. Differences in the location of treatment, including variable treatments, diagnoses, etc.
2. Changing predispositions to diseases over the last 30 years.
3. Severity of cases were different between homeopathic patients and regular patients.
4. "The numbers were small"
5. Homeopathic patients were "younger and of a better class"
6. No sex distinction was made among the homeopathic patients
7. No age distinctions were made among the homeopathic patients

Also, a letter to the editor of the *Lancet* dealing with possible reasons for the apparent differences, referred to the "relative risk of mortality."[73]

In the same year, Dr. C. H. F. Routh presented a paper entitled "On Some Fallacies in the Modern Science of Medicine, More Specifically Homeopathy, and in Estimating the Results of Practice by Imperfect Statistical Inquiry."[74] The paper addressed itself to the practice of homeopathy (in which cases were treated with drugs that would produce in healthy persons symptoms like those of the disease to be treated, the drug being administered in small doses), which was then arousing a great deal of concern among English physicians. The homeopaths attempted to justify their approach to disease and medicine by statistical comparisons of their patients' recoveries with those under the care of other physicians. Routh, however, noted several flaws in their analyses (Table 6).[75] As a result, the homeopaths were discredited. This epidemiologic approach served the London Medical community well in its battle against the homeopaths.

In a letter to the *Lancet* in 1851, by "A Statist," the relationship "prevalence equals incidence times duration" is noted:

> But then six percent ill for a whole year is equivalent to at least *seventy-two* percent of sick cases in the year—i.e. 6 x 12 for the average duration of cases does not exceed a month.[76]

Lastly, in 1852, Dr. George Gregory, who, three years later would lead the English medical world in its campaign to have Parliament enact compulsory vaccination legislation, presented at a meeting of the Royal Medical and

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[73] G Houseman, "Relative rates of mortality in hospitals," *Lancet*, 1851, *2* 85

[74] "Medical Society of London," *Lancet*, 1851, *1* 516-518

[75] *Ibid.*, p 517.

[76] Statist, "Self-supporting dispensaries and Dr. Moore s statistics," *Lancet*, 1851, *1* 170-171

TABLE 7
Gregory's 1849 Smallpox Vaccination Data (95)

| | Total | Deaths | Percentage of Deaths |
|---|---|---|---|
| Unprotected Cases | 254 | 103 | 40 |
| Vaccinated—with Cicatrices | 365 | 38 | 10 |
| —without Cicatrices | 63 | 25 | 39 |
| Total Vaccinated | 428 | 63 | 14 |
| Previously Vaccinated | 3 | 1 | 33 |

Chirurgical Society, a paper on "Vaccination Tested by the Experience of Half a Century."[77] After the presentation, Mr. Richard Dugard Grainger, Chairman of the LES's Committee on Smallpox and Vaccination, brought attention to some of Dr. Gregory's own data, originally presented in 1849 (Tables 7 and 8).[78] It should be noted that the format used for the presentation of such data reflects an epidemiologic approach in its collection and tabulation. Remembering that these events occurred at the Royal Medical and Chirurgical Society, one may conclude that many physicians were exposed to the epidemiologic approach to disease by the early 1850s at the latest.

The application of such epidemiologic approaches to disease, was, however, uncoordinated until 1850. This created many problems for the English medical community, as it was unable to deal with cholera, which first struck England in 1832. Cholera was a terrifying disease; in the 1832 epidemic most persons simply left their urban residences for rural ones. Hence, physicians were quite panicky when, in 1848-49, cholera again struck England; the public began to question the competency of English physicians, due to their helplessness in dealing with the disease. One such physician, J. Tucker, expressed his concern in an 1848 letter to the *Lancet* under the pseudonym of "Pater," suggesting the formation of a society to direct the anti-cholera effort, somewhat similar to the modern National Cancer Institute and the "war on cancer."[79] Pater wrote a similar letter in 1849, after the cholera epidemic had ceased.[80] Two months later, Pater suggested the formation of an organization to study epidemic diseases, to be called the "Epidemic Medical Society."[81] In December 1849, barely three months after his last

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[77] "Royal Medical and Chirurgical Society," *Lancet*, 1852, *1* 285-289.

[78] *Ibid*, p 286

[79] Pater, "Letter to the editor," *Lancet*, 1848, *1* 242

[80] Pater, "Letter to the editor," *Lancet*, 1849, *2* 107

[81] Pater, "Letter to the editor," *Lancet*, 1849, *2* 301-302

TABLE 8
Gregory's 1849 Smallpox Vaccination Data (96)

| | Total | Deaths | Percentage of Deaths |
|---|---|---|---|
| Natural Smallpox in Unprotected | 1,731 | 361 | 20 85 |
| Smallpox after Vaccination | 929 | 32 | 3 44 |

letter, Tucker identified himself as "Pater" and noted that a few gentlemen had formed a committee, held meetings and decided to form such an organization.[82] On March 6, 1850, the London Epidemiological Society (LES) was born.[83]

In July 1850, the LES held its first meeting. Dr. Benjamin Guy Babington, translator of Hecker's *Epidemics of the Middle Ages,* was elected president. Among its founding members were: Thomas Addison, Richard Bright, John Simon, John Snow, Charles Hastings, R. Gordon Latham, Colonel William Henry Sykes, J. Haviland, and Thomas Watson.[84] It is quite probable that Sykes and Bird, as members of the SSL, were the chief resources for statistical methodology in the early days of the LES. However, between 1850 and 1860, one finds many members of the LES, such as John Simon, being elected as Fellows of the SSL. Medical statistics had become accepted in England in the late 1840s and early 1850s, possibly because William A. Guy, as the Dean of the medical faculty at King's College between 1846 and 1852, was able to exert a great deal of influence in favor of the statistical approach to medicine. Statistics had, by the mid-1800s, "assumed for many people the status of a god whose twin shrines were the Statistical and Epidemiological Societies."[85] Further, the influence of the Parisian school, specifically of Louis, can be clearly seen by Babington's remarks shortly after being elected president of the LES:

> Statistics, too, have supplied us with a new and powerful means of testing medical truth, and we learn from the labours of the accurate Louis how appropriately they may be brought to bear upon the subject of epidemic disease.[86]

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[82] J M. Tucker, "Letter to the editor," *Lancet,* 1849, 2 592.

[83] Poynter, *op cit* (n 34 above), p. 187. D Thomson, "Changes in epidemiology and preventive medicine," *Proc Roy. Soc. Med* , 1965, *58* 83.

[84] Commemoration Volume. *Transactions of the Epidemiological Society of London* (London Shaw & Sons, 1901), p. 5.

[85] Poynter, *op. cit.* (n 34 above), p 299.

[86] "Epidemiological Society," *Lancet,* 1850, *2* 641

The purposes of the LES were:

> To institute rigid examination into the causes and conditions which influence the origin, propagation, mitigation, and prevention of epidemic diseases . . . to institute . . . original and comprehensive researches into the nature and laws of disease . . . to communicate with government and legislature on matters connected with the prevention of epidemic diseases . . . to publish original papers; to issue queries; to publish reports; to form statistical tables: to prepare illustrative maps; and to collect works relative to epidemic diseases.[87]

Five committees were formed to carry out these aims:

1. The Committee on Smallpox and Vaccination
2. The Epizootic Committee
3. The Cholera Committee
4. The Hospitals Committee
5. The Continued Fevers Committee

In 1851, the Committee on Smallpox and Vaccination began a survey of 2,200 English physicians to determine the efficacies of both vaccination and inoculation, two preventive measures which were then being scrutinized. However, after an initial response from less than half of these doctors, the *Lancet* began an editorial campaign, chastising those who had not responded to the Committee's questions (Table 9.)[88] Dr. Edward C. Seaton, the committee's secretary, required two years to both collect and analyze the data. The resulting exhaustive report, "On the State of Smallpox and Vaccination in England and Wales, and on Compulsory Vaccination," was quite thorough in its recommendations.[89] The Parliament subsequently published it, and the Compulsory Vaccination Act of 1853 was, in large part, based upon this report.

There are numerous additional examples of the LES's epidemiologic activities. For example, in 1851, Dr. Thomas Hunt presented a paper on the use of statistics and statistical methodology in the study of epidemic diseases.[90] Between 1851 and 1853, one finds that the LES's monthly meetings were devoted to a considerable discussion about whether or not a specific disease was caused by a contagion; yellow fever was a common example cited by both opponents and proponents of the contagion theory. In 1853, one finds John Snow presenting a paper on "The Comparative Mortality of Large Towns and Rural Districts, and the Causes by Which it is Influ-

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[87] "Transactions of the Epidemiological Society of London," *J. Public Health*, 1855, 1: 1-17

[88] "Editorial," *Lancet*, 1852, *1*: 382-383.

[89] E. C. Seaton, *Handbook on Vaccination* (London: MacMillan, 1868)

[90] "Epidemiological Society," *Lancet*, 1851, *2*: 35.

TABLE 9

Questions Posed by the London Epidemiological Society, Committee on Smallpox and Vaccination in its Survey of English Physicians

1. If vaccinated, at what age?
2. Ditto, the number of cicatrices?
3. Ditto, the special character of ditto?
4. Ditto, have you been much exposed to smallpox?
5. Ditto, have you taken smallpox, and when?
6. Ditto, and taken smallpox, was it mild or severe?
7. If inoculated, at what age?
8. Ditto, have you taken smallpox, and when?
9. Ditto, and taken smallpox, was it mild or severe?
10 Ditto, have you taken cowpox accidentally or experimentally?
11. If neither vaccinated nor inoculated, have you taken smallpox?
12. What is your opinion of the efficacy of vaccination as a protection against smallpox?

enced."[91] Having seen for three years (1850-52) the application of statistics to epidemiology, Snow decided to become involved in analyzing epidemiologic data. The resulting analysis was equal, if not superior, to his cholera work two years later. Indeed, 1853 was the first of two turning points for the LES, for it was then that cholera struck Europe, including England, for the third time and provided the LES with its first test of coping with an actual epidemic, or rather, pandemic.

In 1853, as part of its regular procedure of tabulating epidemic data, the Registrar-General's office took note of one suggested method of transmission of cholera, viz. water. It published tabulations of death rates in London during the epidemic, classified by water companies (Table 10).[92] It noted that those water companies with higher death rates from cholera had sources of water of poorer quality and that this relationship was independent of the previously observed relationship of cholera mortality with elevation above sea level. This analysis resulted in strong editorials by both the *Lancet* and the *Medical Times and Gazette* concerning the quality of water sold in

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[91] J. Snow, "On the comparative mortality of large towns and rural districts, and the causes by which it is influenced," *J Public Health*, 1855, *1* 16-30.

[92] Farr, *op cit.* (n 51 above), pp 357-363

TABLE 10
Mortality from Cholera in Districts Supplied by Water Companies, 1853

| | | Aggregate of Districts Supplied Chiefly by the Respective Water Companies | | | |
|---|---|---|---|---|---|
| Water Companies | Source of Supply | Elevation (in feet) above trinity high water mark | Population | Deaths from cholera in 13 weeks ending Nov 19 | Deaths per 100,000 inhabitants |
| London | — | 39 | 2,362,236 | 744 | 30 |
| Hampstead & New River | Springs at Hampstead and Kenwood, two artesian wells and New River | 80 | 166,956 | 8 | 5 |
| New River | Chadwell Springs in Hertsforshire, from River Lee, and four wells in Middlesex and Herts | 76 | 634,468 | 56 | 9 |
| Grand Junction | Thames, 360 yards above Kew-Bridge | 38 | 109,636 | 15 | 15 |
| Chelsea | Thames at Battersea | 7 | 122,147 | 22 | 18 |
| Kent | Ravensbourne in Kent | 18 | 134,200 | 31 | 23 |
| West Middlesex | Thames at Barnes | 72 | 277,700 | 89 | 32 |
| East London | Lee at Lee Bridge | 26 | 434,694 | 162 | 37 |
| Lambeth & Southwark | Thames at Thames Ditton and at Battersea | 1 | 346,363 | 220 | 64 |
| Southwark | Thames at Battersea | 8 | 118,267 | 121 | 102 |
| Southwark & Kent | Thames at Battersea, Ravensbourne in Kent, Ditches and wells | 0 | 17,805 | 19 | 107 |

London.[93] They also raised the question of what the water companies were planning to do to prevent a future epidemic. It is highly probable that John Snow either read or heard of these editorials and the data upon which they were based.

The LES was oriented towards the previously mentioned concept of a "law of mortality," as is clearly shown in one of its stated purposes: "To institute . . . researches into the . . . laws of disease. . . . "[94] Further, in 1855, Dr. James Bird presented an invited address entitled "On the Laws of Contagious and Epidemic Diseases."[95] The "law of mortality" concept was one of the underlying ideas used by LES members in the analysis of epidemiologic data.

The 1850s can be remembered as the period when Dr. Benjamin Ward Richardson, a close friend of Snow's, and Dr. Gavin Milroy became members of the LES and began to conduct the epidemiologic studies that would place them among the leaders of epidemiology until 1900.

Benjamin Ward Richardson became a member of the LES in 1852. As a friend of John Snow, he supported Snow in his theory of water propogation of cholera, which, judging from both Richardson's remarks in 1855 and 1856 and Guy's remarks in 1870, was quickly accepted by the medical community.[96] One example of Richardson's understanding of epidemiologic concepts is a paper presented at a LES meeting in 1858, entitled "The Investigation of Epidemics by Experiments," in which he called for the use of animal models in order to further the study of epidemic diseases then afflicting mankind.[97] The *Lancet* summarized Richardson's aims in using such models:

> 1. That by experiment it might be ascertained in what excreta the poisons of certain of the epidemic diseases are located.
> 2. By what surfaces of the body such poisons may be absorbed so as to produce their specific effects.
> 3. Whether the virus of a disease in reproducing its disease in a healthy body, acts in the development of the phenomena by which the disease is typified primarily or secondarily—i.e., by its own reproduction and presence, or by the evolution of another principle or product.

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[93] "Editorial," *Med Times & Gaz* , 1853, n s 7 558-559; "Editorial," *Lancet*, 1853, 2 419-420

[94] "Origins and objects of the Epidemiological Society," *Transactions of the Epidemiological Society of London* (London T. Richards, 1856), p 2

[95] J Bird, *On Laws of Contagious and Epidemic Diseases* (London John Churchill, 1854), pp 1-32

[96] B. W Richardson, "Propagation of cholera," *J Public Health*, 1855, *1* 402-403, idem, "Report, *J Public Health*, 1856, 2 192, Guy, *op cit* (n 38 above), p 215.

[97] "Epidemiological Society," *Lancet*, 1858, *1* 349

4. Whether climate, season, or other external influences modify the course of epidemics, by reproducing modifications of the epidemic poisons, or modifications in the system of persons exposed to the poisons.

A publisher-editor throughout his life, Richardson founded the *Journal of Public Health* in 1855, where the transactions of the LES were first published.[98] However, this particular journal was discontinued in 1859 for financial reasons. Despite Richardson's non-acceptance of the germ theory, his 45 years in epidemiology saw the establishment of epidemiology as an accepted scientific discipline, to which his leadership greatly contributed.[99]

Gavin Milroy was one of the founding members of the society. Attending most of its meetings between 1850 and 1860, Milroy would not display until the 1860s the brilliance that would make him, along with Richardson, the leading spokesmen for epidemiology in the latter one-third of the 19th century. Such works as "The Influence of Contagion on the Rise and Spread of Epidemic Diseases" presented in 1861, in which he dismissed the miasmatic theory of disease causation because of its inconsistency with the spread of epidemics; "Address at the Opening of the Session, 1864-65"; and the "Valedictory Address" in 1866, all attest to his comprehension of epidemiologic principles.[100] It was in 1864 that Milroy, on Babington's retirement, would become the second president of the LES.[101] Indeed, in the 1860s a new generation of epidemiologists were to take over the LES and all those institutions then associated with epidemiology.

The LES entered the 1860s very inauspiciously. Meetings were held as usual. However, by 1863, epidemiology had, according to Richardson, come "into its own": "Let us then, as *scientific epidemiologists,* join hands with the sanitarian . . . " (italics by the author).[102] This is the first known reference to "epidemiologists." A year later, as has been noted, Babington retired from the LES presidency to allow "a change in government," which would pursue new ideas, new notions, etc. In 1866, Babington would die. Between 1860 and 1870, most of the founding members of the LES would meet the same fate, and with their deaths, an era in the history of epidemiology would end. By 1870, Milroy was one of the few surviving founding

[98] B G Babington, "Address on vacating the chair," *Trans Epidem Soc. London*, 1864, *2* 168.

[99] A Newsholme, *Fifty Years in Public Health* (London. George Allen and Unwin, 1935), p 102.

[100] G Milroy, "The influence of contagion on the rise and spread of epidemic diseases," *Trans Epidem Soc London*, 1861, *1* 172-180, idem, "Address at the opening of the session, 1864-1865," *ibid* , 1864, *2* 247-256, idem, "Valedictory address," *ibid.*, 1866, *2*· 435-489.

[101] Commemorative Volume, *op cit* (in 84 above), p. 7

[102] B. W. Richardson, "The present position and prospects of epidemiological science," *Trans Epidem Soc. London*, 1863, *2* 127

members; but the birth and establishment of epidemiology had been accomplished. Epidemiology, germinating from Louis' seeds, had "greened." It was stated in 1860: "The science of hygiene may almost be said to have come into existence during the last twenty years," and the LES certainly played a major role in this development.[103]

Beginning in 1870 and until 1910, the Bacteriological Era overshadowed epidemiology. During these 40 years, epidemiology hibernated in Francis Galton's and Karl Pearson's biostatistical laboratory. Here, the next generation of English epidemiologists were trained. It was to be under the leadership of Major Greenwood and Percy Stocks that "epidemiology" was to undergo a Renaissance in England.

## *Conclusion*

The beginning of the Bacteriological Era signaled the end of the "Greening of Epidemiology." However, the concepts that were developed during this period were either continued until the present, or rediscovered in the 20th century. Indeed, the development of such concepts and methods in the 1800s is not too surprising, especially when one considers that the same underlying "epidemiological logic" used today was available then.

This logic was first used by P.-C.-A. Louis, who was a popular teacher in Paris and who trained many students, imbuing some of them with this logic. Further, Louis' influence was not confined to his own students' research but was constantly reported in the leading medical journals of the day, including the *Lancet* and the *American Journal of Medical Sciences,* which circulated throughout both Europe and the United States. Some of his students, particularly Guy and Farr, returned to their native lands and spread Louis' ideas among their fellow countrymen. In other instances, physicians on the European continent, having read of Louis in the literature, attempted to implement his ideas. Some are known to us, other are not. One who is well known to us is Semmelweis, and the fact that Semmelweis' work was done in 1847 suggests consistency with this pattern.[104]

Guy and Farr were able to influence a young organization, established for the collection of statistics, the SSL, to such an extent that Louis' ideas began to be implemented on a broad scale in epidemiological studies. Admittedly, statistical methods at this time were very crude. However, statistical techniques became sophisticated very quickly. Further, the English medical community, threatened by a cholera epidemic in 1849, turned to these techniques, known to them as a result of their associations with members of the SSL, in an attempt to determine the cause of cholera. This effort was quickly extended to other epidemic diseases, coordinated by an association, the LES. Epidemiology became recognized as a distinct science.

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[103] Poynter, *op cit* (n 34 above), p. 297.

[104] Lilienfeld and Lilienfeld, *op cit* (n 3 above), pp. 169-174.

By 1860, the LES had established itself. It had begun to provide a forum to consider the issues for which it was formed. As the original members began to die, the LES directed its efforts toward other diseases, some of which were not necessarily "epidemic." By 1870, the leadership of the LES was in the hands of a new generation of epidemiologists. The work of the 1850s and 1860s had constituted the "Greening of Epidemiology," that period when epidemiology began to flourish as a separate discipline. With the advent of the Bacteriological Era, epidemiology was forced to "hibernate." But when it became apparent that bacteria were not responsible for all diseases, epidemiology reappeared, flourishing again (under Major Greenwood and others). We, today, are still in that era.

Lastly, one problem that has plagued the study of the history of epidemiology is that too much emphasis has been placed on "WHO believed WHAT" and "WHEN did WHO believe this"; however, the critical question is "WHY did WHO believe WHAT." The underlying reasoning processes used by a specific individual to arrive at an epidemiological inference is the key to tracing the development of epidemiological methods and concepts; for it is the methods and concepts, rather than the specific inferences that one derives from them, that make epidemiology the unique discipline that it is today.

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# MEDICINE AND BUREAUCRACY: JEAN COLOMBIER'S REGULATION FOR THE FRENCH *DÉPÔTS DE MENDICITÉ* (1785)*

THOMAS M. ADAMS

It is increasingly evident from recent research that royal administrators in the last decades of the Old Regime in France esteemed highly the expert advice of doctors in many areas of social policy and institutional reform. It is also evident that doctors gave advice willingly, and that as a result of their activities they came to define some of the goals that policy would prescribe, as well as serving to elaborate technique and procedure.[1]

Jean Colombier was one of the most active doctor-bureaucrats. Born in 1736, he began his career as a military surgeon, like his father. Eventually he published major treatises on military medicine and its administration. He studied medicine at the Faculties of Caen and Paris, defending his last thesis in 1768. He then attempted to launch a practice open to annual subscription by the citizens of Paris. The Faculty of Medicine forced Colombier and his associates to abandon this group health plan. The reform initiatives of Jacques Necker, who was in charge of French royal finances from 1777 to 1781, drew Colombier into a consulting role in the royal bureaucracy.[2]

In September 1777, Colombier was consulted as an expert on "matters of

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[1] See especially Louis S. Greenbaum, "Scientists and Politicians: Hospital Reform in Paris on the Eve of the French Revolution," in *The Consortium on Revolutionary Europe, 1750-1850 Proceedings 1973* (Gainesville University of Florida Press, 1975), pp 168-191, which presents a general argument elaborated in many other articles, Jean-Pierre Goubert, *Malades et médecins en Bretagne, 1770-1790* (Paris, 1974), and Caroline C. Hannaway, "The Société Royale de Médecine and epidemics in the Ancien Régime," *Bull Hist Med* , 1972, *46* 257-273 I am indebted to these authors for ideas exchanged in conversation See also Harold T Parker, "French administrators and French scientists during the Old Regime and the early years of the Revolution," in Richard Herr and H T. Parker, eds , *Ideas in History. essays presented to Louis Gottschalk by his former students* (Durham, N C Duke Univ Press, 1965), and the standard work of Camille Bloch, ***Assistance et l'état en France à la veille de la Revolution. Généralités de Paris, Rouen, Alençon, Orléans, Soissons, Amiens (1764-1790)*** (Paris, 1908)

[2] Pierre Gallot-Lavallée, *Un hygiéniste au XVIIIe siècle, Jean Colombier, rapporteur du conseil de santé des hôpitaux militaires, inspecteur général des hôpitaux et prisons du royaume (1736-1789)* (Paris, 1913), p 26.

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salubrity" by a special commission that was investigating ways to improve conditions in the hospitals of Paris.[3] He then agreed to work on the establishment of a clinic for children born of venereal patients at the Hospice de Vaugirard.[4] In January 1780, he presented a report on the state of the royal prisons of Paris to the Académie Royale des Sciences.[5] In that year he gave up his remaining private practice to assume responsibility for a vast bureaucratic domain, being named "inspector of hospitals, prisons and *maisons de force* of the kingdom."[6]

The range of Colombier's activity was immense; much of it remains to be documented. Colombier's work in regulating civil hospitals is one strand in Professor Louis S. Greenbaum's forthcoming study of French hospital reform.[7] Here, we shall limit our view to Colombier's role in devising and implementing a regulation for the *dépôts de mendicité*. These institutions, designed as a network of detection centers for the repression of public begging, represent a notable step in the bureaucratization of social policy in eighteenth-century France. It will be interesting to see how the doctor, Jean Colombier, acquired the power of prescribing their methods of operation, and to observe the efficacy of his prescriptions. This leading instance may illuminate the process of bureaucratization, and the role of doctors in promoting it.[8]

## *In Search of a Rule*

The French word "*bureaucratie*" first appears in print in July 1764 in Grimm's *Correspondance littéraire*.[9] Fittingly, the *dépôts de mendicité* were officially conceived in the following month, veiled in the language of a new royal decree on vagabonds and shiftless persons.[10] The royal commission which prepared this measure and later completed it was determined to supplant the desultory efforts of municipal authorities with a uniform royal policy of arrest, detection and correction of beggars. In order to stabilize costs and forestall the budgetary problems that had wrecked earlier schemes, it was necessary to prescribe a uniform workhouse standard of treatment, set between that of soldiers and that of prisoners, and far more austere and utilitarian than that of charity hospitals. A simple instruction was drawn up, reflecting the practical wisdom of royal intendants and the experts who served them.[11]

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[3] Memoir of 14 January 1784 reviewing Colombier's services in support of fixing his yearly compensation at 12,000 *livres* (probably by La Millière), Archives Nationales (hereafter "A. N."), $F^{15}$ 2866.

[4] Gallot-Lavallée, *Colombier*, p. 52.

[5] Antoine Laurent Lavoisier, *Oeuvres de Lavoisier*, 6 vols. (Paris, 1862-1893), III, 465-6.

[6] His first official emoluments as inspector were authorized 7 January 1781, A. N., $F^{15}$ 2866. Gallot-Lavallée, *Colombier*, p. 75.

[7] Of particular interest in the context of this paper is Colombier's role in devising the regulation of 1786 for the Hôtel-Dieu of Paris.

[8] This study of Colombier's role is part of a work in progress, "Beggars and Bureaucrats: a history of the French *dépôts de mendicité* from 1764 to the Revolution."

[9] Friedrich Melchior Grimm, *Correspondence littéraire*, 16 vols. (Paris, 1877-1882), VI, 30. cf. Paul Imbs, ed., *Trésor de la langue française: Dictionnaire de la langue du XIXe et du XXe siècle (1789-1960)*, Vol. 4 (Paris, 1975), s. v. "bureaucratie."

[10] Bloch, *Assistance et l'état*, pp. 162-3.

It proved no easy task to maintain inmates in health, given the sources of infection and debility concentrated within the narrow walls of the depots. The option of transferring the ill to hospitals was costly, limited, and not always satisfactory. Infirmaries had to be installed in the depots themselves, and regular medical visits were required. Whatever measures royal intendants and their subordinates attempted, they were still exposed to challenge by those who regarded them as usurpers of judicial and municipal authority. The taint of scandal enveloped the gloomy beggars' prisons, especially after it became known that the majority of them were provisioned by general contract let with a company of Parisian bankers in 1773.[12] The administrator responsible for the overall coordination of the depots, Bertier de Sauvigny, believed that the general contract could be enforced by proper supervision, and that it had the merit of providing incentives to devise work for inmates and to keep them healthy. The contractors, after all, received a flat rate for each inmate, including the sick who required costlier rations and medical care. Critics still saw the contract as a stark incitement to exploit the helpless poor.[13]

Faced with challenges to their authority, administrators clamored for expert assistance and advice. Bertier, however, was reluctant to add staff. Only the personal intervention of Necker made it possible for the intendant of Soissons to appoint, in 1781, a special "inspector," the abbé de Montlinot, who attempted to turn the depot of Soissons into a showcase of economy, health and industry.[14] Administrators also asked for clarification of their authority. The intendant of Brittany found that it was not enough to inspect conditions in person and to make public a new regulation for the depot at Rennes. The subdelegate responsible for the depot was still subject to endless challenges from the provincial Estates, and threatened to resign. Urging the need for "uniform and certain rules," the intendant wrote to the minister in 1782, "It is absolutely necessary either to destroy these sorts of establishments, or to give them a solidity that will secure the confidence of their administrators."[15]

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[11] The text of the instruction is reproduced in the manuscripts of the Bibliothèque Nationale (hereafter "B.N."), *fonds français* 8129, folios 149-153.

[12] There is a copy of the general contract in Archives départementales (hereafter "A.D.") Indre-et-Loire, C 307. I have described the dilemmas of administrators in "Niveau de vie et correction dans les depôts de mendicité au XVIIIe siècle," *Bulletin de la Société française d'histoire des hôpitaux*, 1976, *33*, 53-72

[13] See for example the complaints of the Archbishop of Aix, 12 December 1776, A.N., H 1417, and those of Dupont de Nemours in *Carl Friedrichs von Baden Brieflicher Verkehr mit Mirabeau und Dupont*, ed Carl Knies (Heidelberg, 1892), II, 100 Turgot shut down most of the depots after a review of royal policy on mendicity, but the system was largely restored after his fall from office in 1776 (see Bloch, *Assistance et l'état*, pp. 193 and 220).

Insistent challenges from the provincial estates—those of Brittany and Languedoc especially—reached the royal council, where the policies governing the depots were explicitly reaffirmed in 1783. The general regulation for the depots drawn up by Jean Colombier two years later refers to these challenges and to the king's intention to prescribe a new general regulation.[16]

Jean Colombier had already begun to lay the groundwork for a regulation since his appointment as inspector of hospitals, prisons and *maisons de force* in 1780. A memoir composed in Jaunary 1784 credits him with carrying out "the most useful changes in the prisons of Paris, and in the different *dépôts de mendicité,* notably in that of St. Denis under the orders of the intendant of Paris."[17] The first trace of his involvement at St. Denis is a record of payment to him for the provision of dressing gowns to inmates there in 1781.[18] Then, in February 1782, Bertier, intendant of Paris and coordinator of the depots, answered a complaint from Soissons concerning the miserable condition of inmates transferred there from the depot of St. Denis, by saying that Dr. Colombier had certified their fitness to depart. All had been examined by the "health officers" of the depot, and all had departed with sufficient clothing. Dr. Colombier preferred fine cold weather *("un beau froid")* for such transfers, in any case.[19]

Colombier may have assisted Bertier in planning new constructions at the depot of St. Denis from 1782 onward. In 1783, accounts include payments for two ovens, presumably connected with measures of hygiene, and for a pharmacy to be established at the veterinary school of Alfort for the provisioning of the *dépôts de mendicité*.[20] A ventilator was installed in 1783, another in the following year. Such changes occurred in the context of a regulation that was recommended as a model for other depots. The Comptroller General of Finances suggested to the intendant of Rouen, following an inspection there by Colombier in March 1783, that problems of negligence on the part of staff at the depot might have been prevented by the enforcement of "a regulation similar to the one in use in the depot of St. Denis."[21]

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[14] Necker to intendant of Soissons, 13 March 1781, A.D Aisne, C 749

[15] Caze de la Bove to Joly de Fleury, 13 March 1782, A N , H 556

[16] The text of the regulation used here is from A N , H 556

[17] A N , $F^{15}$ 2866

[18] A N , $F^{15}$ 2806, 45 *livres* for "robes de chambre pour les malades "

[19] Bertier to the intendant of Soissons, 26 March 1782, A D Aisne, C 714

In 1783, Colombier also had a part in devising a new regulation for the royal Dépôt de la Quarantaine at Lyon, following his controversial decision, given effect by a royal decree of September 9, to eliminate the municipal workhouse-jail for beggars attached to the historic Hôpital Général de la Charité.[22] The regulation, in printed form, was preceded by an account of the formal meeting of December 4, 1783, attended by Colombier and the intendant of Lyon, at which it was adopted. A preamble refers to previous efforts to reform abuses that had arisen under the general contract at the royal depot. The many articles following included provisions that spelled out the role of Colombier, as Inspector-General, in making decisions, jointly with the doctors of the depot, relating to the remedies to be stocked in the pharmacy, and the methods to be used in treating venereal disease, the mange, and insanity.[23]

Bertier mentioned recent visits by Colombier to "seven or eight principal depots" in a letter of July 1784 requesting new appropriations of funds for repairs, construction, and an increased budget for food.[24] This justification of expenditure seems to represent a commitment to newly defined general standards. At Rouen, for instance, the intendant had explained to Bertier that the increased expense of a proposed new contract for supplying the depot reflected in part Colombier's advice to add meat and vegetables to beggars' rations.[25]

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[20] A. N., $F^{15}$ 2806. Colombier did some teaching at Alfort, according to Paul Delaunay, *La vie médicale aux xvie, xviie et xviiie siècle* (Paris: Éditions Hippocrate, 1935), p. 271. On links between human and veterinary medicine, see Caroline C. Hannaway, "Veterinary medicine and rural health care in pre-revolutionary France," *Bull. Hist. Med.*, 1977, *51*: 431-437.

[21] A. D. Seine-Maritime, C 1035 (letter of D'Ormesson to intendant of Rouen, 13 April, 1783). Colombier was in Rouen in March according to travel expenses listed in A. N., $F^{15}$ 2866.

[22] Jean-Pierre Gutton, *La société et les pauvres: l'exemple de la généralité de Lyon, 1534-1789* (Paris: Société d'Édition "Les Belles Lettres," 1971), p. 461. A. D. Rhône, C 175.

[23] *Règlement concernant le dépôt royal de mendicité de Lyon, lu et arrêté au Bureau d'Administration dudit Dépôt Royal de la Quarantaine, le 4 Décembre 1783* (Lyon, 1783). A. D. Rhône, C 175.

[24] Bertier to Gojart, 10 July 1784, A. N., $F^{4}$ 1026.

[25] Intendant of Rouen to Bertier, 10 May 1783, A. D. Seine-Maritime, C 1035. The contract is discussed in the useful study by Marie-Odile Deschamps, "Le dépôt de mendicité de Rouen," Diplome d'études supérieures, Faculte des Lettres, Caen, 1965 (copy at Bibliothèque de l Assistance Publique, Paris).

If a draft regulation was already in existence, its final adoption was delayed by other demands on Colombier's time in 1784. In May, he was at Auxerre, in Burgundy, complaining of overcrowding at the Hôtel-Dieu and noting that poor records made it impossible to calculate unit cost.[26] In October, he apologized to the intendant of Lyon that he had been preoccupied by "the great ills of our prisons of the Abbaye, of the Conciergerie and of the Châtelet, which (said he) have obliged me to give myself over entirely to the dispositions that the Parlement has prescribed in order to prevent new catastrophes."[27]

## *The Regulation of 1785*

When the draft of a general regulation was ready, an advance copy was sent to Caen. The subdelegate as well as the doctor and the surgeon who served the depot of Beaulieu were invited to suggest changes or additions.[28] Amiens was not equally favored. The intendant there, who had not long previously received Colombier's sharp criticism of the depot at Amiens, noted to his subdelegate that no comments were invited. In a further letter, Colombier added, in a general vein, that the regulation would not appear in printed form "before the end of the year, in order to profit from the observations that might be made."[29]

Measures of hygiene practised in military hospitals, prisons, civil hospitals and *dépôts de mendicité* converged in the regulation of 1785. These measures included the use of bathing pools and of a small oven "for destroying the infection and vermin of clothing and linen." Various depots already had such facilities; they were prescribed in the Lyon depot in 1783.[30] Military hospitals were supposed to have them, according to a regulation of 1780.[31]

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[26] Marcel Bolotte, *Les hôpitaux et l'assistance dans le province de Bourgogne au dernier siècle de l'ancien régime* (Dijon. Association Bourguignonne des Sociétés Savantes, 1968), p. 81

[27] Letter of 15 October 1784, A D. Rhône, C 165

[28] The intendant relayed the invitation to the subdelegate in a letter of 6 January 1785, and referred to M Chibourg, the doctor, and M Amiel, the surgeon, by name (A.D Calvados, C.718)

[29] Letters to intendant from Calonne (20 March 1785) and from Colombier (27 May 1785), A D Somme, C.1624.

[30] A D Rhône, C 175; cf Article 2 of the contract of 1783 at Rouen with Simon Heutte, A D Seine-Maritime, C 1035. There is provision for baths and ovens in a contract signed 30 January 1782 by the intendant of Alencon, A.N., $F^{15}$ 2800.

[31] *Ordonnance du Roi concernant les hôpitaux militaires et ceux de charité au compte de S M du 1er Janvier 1780* (Paris, Imprimerie royale, 1780), p. 116ff. (copy at National Library of Medicine, Bethesda, Maryland).

The commission that had investigated the prisons of Paris, following Colombier's report to the Academy of Sciences, had recommended that prisons be so provided.[32]

Articles concerning infirmaries paralleled the language of the regulation for the depot at Lyon. Patients were to be strictly separated by sex and by category of illness; beds were to be constructed with a partition allowing separate space for two per bed. Every pair of beds was to have a sick robe, two pairs of sandals, two bowls for infusions, two ladles for bouillon and two chamberpots. There was to be a toilet seat for every four beds. The provision of food and medication was to be strictly regulated by the doctor's prescriptions recorded by name of inmate in a *cahier de visite;* a clerk would transcribe the amount of food to be distributed by the head nurse. Standard types of diet were spelled out in the regulation.[33]

General rules of hygiene also parallel the Lyon regulation, with added details on bedding and linen, stuffs from which clothes should be made, and the prohibition of copper vessels for kitchenware.[34] Colombier's influence extended to prescribing for the well-being of inmates in the broadest sense. His concern for motivating inmates to work was reflected in the provision that they would receive from one-half to two-thirds of usual wages, depending on the type of work and the output.[35]

Like the Lyon regulation, that of 1785 promised a forthcoming instruction on the means for treating the insane.[36] The instruction drawn up for this purpose by Colombier, with the collaboration of François Doublet, was published in 1785.[37] Doublet had performed "special treatments" in the depot of St. Denis in 1784. Traces of clinical experimentation with electric therapy at St. Denis include payment to Mauduit under the heading "infirmary," and for "an electric machine" in accounts for 1785.[38] In July 1785, a ninety-page booklet was dispatched in order to assist standardization in another area, the provisioning of pharmacies.[39]

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[32] Lavoisier, *Oeuvres*, III, 474

[33] A.N , H.556, "Règlement concernant la constitution et le régime intérieure des dépôts de mendicité du Royaume," Titre III, art. 47 (further references by section only) Articles pertaining to the regime of the infirmeries are excerpted in an appendix to my article cited above, note 12

[34] Titre III, art 19, the *lieutenant de police* of Paris, Lenoir, had forbidden the use of copper containers for keeping milk in order to prevent cases of verdigris, according to Alan Williams, "The police and public welfare in eighteenth-century Paris," *Social Science Quarterly*, December 1975, *56* (3) p 408

[35] Titre III, art 79

[36] Titre III, art. 67.

[37] [Jean Colombier and François Doublet], *Instruction sur la manière de gouverner les insensés et de travailler à leur guerison dans les asiles qui leur sont destinés* (Paris, 1785), cf Gallot-Lavallée, *Colombier*, p 65

[38] A N , F$^{15}$ 2806

An "Instruction on the correspondence of doctors and surgeons attached to the *dépôts de mendicité,*" dated 13 February 1788, formally organized the function of the depots as observation posts for social medicine.[40] The Inspector General wished to receive information on the disposition of each depot, on the state of the infirmaries, on the illnesses of the inmates and upon the "medical topography" of the depot itself. Military hospitals had provided such information under a plan drawn up in 1763, and Colombier argued in his *Code de médecine militaire* of 1772 that they should regularly perform such a role.[41] The collection and tabulation of medical data was, of course, a major activity of the Société Royale de Médecine from its inception as a commission on epidemics in 1776, and the Society extended its supervision over a renewed correspondence with military hospitals in 1781.[42] Since 1781, on the other hand, Colombier and his superior, the intendant of finances Chaumont de la Millière, were responsible for collecting observations made in civil hospitals and prisons.[43]

Before the appearance of any official circular on medical reporting, François Doublet, Colombier's assistant, had published notable medical observations from the depots along with other reports from civil hospitals. In 1787, for example, he published "Observations made in the *dépôt de mendicité* of Rouen on uncommon maladies, and on venereal diseases by M. Marc, surgeon of the depot, under the direction of M. Le Pecq de la Clôture."[44]

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[39] Bertier sent the intendant of Rouen a printed *Matière médicale des dépôts de mendicité du royaume* with a letter of 26 July 1785. A D Seine-Maritime, C 1035

[40] A.D Indre-et-Loire, C.315

[41] Jean Colombier, *Code de médecine militaire pour le service de terre* . 5 vols (Paris, 1772), Vol II, p 122 I am grateful to Caroline C Hannaway for comments which led me to make some revisions in this paragraph and the next. See her thesis, *Medicine, Public Welfare and the State in Eighteenth Century France the Société Royale de Médecine of Paris (1776-1793)*. The Johns Hopkins University, Ph D 1974 (Ann Arbor, Michigan Xerox University Microfilms, 1976), p 71, on the correspondence of military hospitals published in 1766 and 1772

[42] Hannaway, *Medicine, Public Welfare and the State*, p 167, and idem, "The Société Royale de Médecine and epidemics in the Ancien Régime," *Bull. Hist Med.*, 1972, *46*, p 267.

[43] Louis S Greenbaum, "Scientists and Politicians Hospital Reform in Paris on the Eve of the French Revolution," in *The Consortium on Revolutionary Europe, 1750-1850 Proceedings 1973* (Gainesville University of Florida Press, 1975), p 174 and Camille Bloch, *Assistance et l'état*, p 319.

[44] Francois Doublet, *Observations faites dans le département des hopitaux civils*, Vol III (Paris, 1787), p 73 A decade before, Le Pecq de la Clôture published observations on epidemic conditions dating from 1773 in the dépôt de mendicité of Rouen and the unhealthy quartier de Martainville in which it was situated, in his *Collection d'observations sur les maladies et constitutions épidemiques* . (Rouen, 1778), pp 117, and 942

Although there was some conflict between the Société Royale de Médecine on the one hand and Colombier and La Millière on the other over the supervision of medical reporting, the two information networks overlapped. Doctors and surgeons who served as the medical officers of the depots were often the same ones who served at the request of the intendants in combatting epidemics.[45]

The administrative organization of the depots was at least as significant as the medical and sanitary regulations of 1785. The kingpin of Colombier's staff model was the overseer, responsible for maintaining the standard of provisioning and diet prescribed in the regulation, and for giving an account of expenses in a strictly standardized form. The wording of the regulation underscores his key role: "The police, the subsistence, and the regime of the interior of the depot will be confided to an overseer, having a commission from the Council, and the said overseer will alone command in the depot under the orders of M. the Intendant, as far as concerns the duties that he will be charged with."[46] The significance of this reform was remarked upon in 1791 by Bannefroy, a former inspector of the depots who had worked under Colombier at St. Denis and elsewhere. In the depots, he noted, "the life of these unfortunates has long been calculated by bidding and contracting. Companies had made speculations on the bread of these wretches; and it was but recently that the administration at last established, in the depots, an overseer accountable to the King."[47]

Colombier had argued, in his *Code de médecine militaire,* that provisioning by contract invited abuse, and suggested establishing a corps of "capable people" as overseers in military hospitals.[48] An abortive attempt had been made to establish the principle in the depots in 1769, but the task devolved to already overburdened subdelegates, not specifically trained for the task.[49]

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[45] Colombier describes the work of Davan in reporting on epidemics and combatting them while continuing his rounds at the depot of St. Denis, in his *Description des épidémies qui ont régné depuis quelques années dans la généralité de Paris. Publié par ordre de M. l'intendant* (Paris, 1783), premier cahier, p. 96. La Hardouyère appears in accounts for the *dépôt de mendicité* at Rennes in 1785 (A. D. Ille-et-Vilaine, C 1301) and is active in the combat against epidemics (Jean-Pierre Goubert, *Malades et médecins en Bretagne,* p. 232n.). Dupichard at Tours is ubiquitous. Hannaway describes conflict over control of correspondence in *Medicine, Public Welfare and the State,* pp. 174-177.

[46] Titre I, art. 3.

[47] Bannefroy, *Mémoire sur la mendicité par M. Bannefroy, ancien inspecteur des maisons de force et des dépôts de mendicité* (Paris, 1791), p. 26; Bannefroy appears on the accounts of St. Denis for visits in 1783 (A. N., $F^{15}$ 2806).

[48] Colombier, *Code de médecine militaire,* Vol. II, p. 91.

[49] See for example the correspondence of Turgot and his subdelegate at Angoulème, A. D. Haute-Vienne, C 364.

Under Colombier's regulation, the depot of St. Denis served as a school for overseers.[50] In 1791, the Minister of the Interior explained the practice of requiring a period of training there, "in order to maintain a necessary uniformity, be it in the interior regimen, in the use of funds, or in the keeping of records and accounts."[51] The accounting system used, with its standard printed forms for calculating daily expenses and unit cost per inmate, was referred to as "*la régie économique.*"

*Implementation and Its Problems*

Colombier's regulation was never printed. No doubt he wanted it to be tested and fully implemented first. Implementation of the regulation was gradual for several reasons. Considerations of cost and convenience persuaded Colombier in at least one case to allow an old contract to expire before introducing the *régie économique.*[52] He chose to force implementation of the new system where his inspections gave proof of an urgent need. The Auvergne offers an example of a contest between Colombier and an intendant in which Colombier and his regulation won out. The intendant at Riom, Chazerat, on receiving Colombier's inspection report and notice of the arrival of an overseer, replied with the classic lament of a local official confronted by guidelines from a distant bureaucrat. The building used for the depot was small, he said, and it could not be converted to allow for "the prodigious number of apartments" required by the regulation. The expansion of staff alone would swallow the entire current annual budget for the depot at Riom.[53]

The inspector spoke through the minister, who replied that it was necessary to establish "greater order and salubrity" in the depot. "The remedy," he concluded flatly, "lies in the general regulation." Here, the range of Colombier's inspection activities allowed him to match the intendant's knowledge of local circumstances. The minister suggested that the intendant might either have the depot enlarged or rent space for workshops and infirmaries from the *hôpital-général* of Riom. Colombier had noted, in a report on his visit to the latter establishment, that its workshops were losing money. Such enterprises were not generally suited to hospital establishments, in his view. From such responses it is clear that Colombier's inspection over several categories of institutions gave him the authority of generalized expertise, coupled with the knowledge of specific cases.[54]

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[50] A N., $F^{15}$ 2806 Accounts for 1786 include payments to three "surnuméraires." Henrion de Bussy, who becomes *régisseur* (overseer) at Riom, received payments at St Denis in 1785 The overseer at Bourges demanded payment of sums due to him "pour le tems que j'ai régi par interim le dépôt de St Denis en 1787 et 1788," and he mentions having been responsible for "une école des surnuméraires" there (A.N , $F^{15}$ 2787).

[51] Minister of the Interior to administrators of the department of the Orne, 20 April 1791, A N , $F^{15}$ 2800 The minister's reference to "l'ordre des écritures et de la comptabilité" is one of many points where Michel Foucault's comments on the "disciplinary" functions of observation and record-keeping seems applicable. see his *Surveiller et punir naissance de la prison* (Paris, 1975), pp 145 and 191ff.

[52] Report of Colombier on the depot at Alençon, May 1789, A N , $F^{15}$ 2800

[53] Intendant of the Auvergne to the Comptroller General, 12 January 1786, A D. Puy-de-Dôme, C 1188

As the regulation gained ground, Colombier had to secure his conquests. Personnel had to be habituated to new tasks. At Soissons, the abbé de Montlinot was not easily persuaded that the forms used for the "*tableau de régie économique*" were practical, having evolved his own system for reckoning unit cost of bread rations.[55] At Châlons, Colombier expended great efforts, ultimately successful, to oust an overseer who had protection at Court but who was only fit, in his words, "to bungle the simplest matters."[56] At Amiens, a concierge carelessly allowed an insane inmate to follow him into the bedding stores with a lit taper: the depot burned down. Colombier publicized the dismissal of the concierge with posters in every depot.[57]

A gradual pace of implementation was dictated as well by the initial costs of change, attested in many cases. The state's fiscal crisis, meanwhile, cut directly into Colombier's work of inspection. Following the breakdown of Calonne's projects for fiscal reform, the new minister Loménie de Brienne made sweeping budget cuts in 1787. Attacking excess administrative overhead for the *dépôts de mendicité,* he cut allotments for "Under-Inspectors-General" and their travel expenses, cut salaries for clerks and office costs, cut the budget for the depot at St. Denis, and, for good measure, cut out Bertier's position as coordinator of the depots.[58]

Actual progress in improving health conditions in the depots was impeded by the difficulty of controlling conditions in the prisons where beggars were often kept before they reached the depots. The inspector Bannefroy observed that the beggar arrived in the depots "with the germ of contagious illness caught in the prisons during a long stay; there, probably, lay the principal cause of the high mortality that often occurs in such houses."[59]

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[54] Comptroller General to the intendant of the Auvergne, 19 January 1786, A.D. Puy-de-Dôme, C 1188, Colombier's strictures on the workshop in the *hôpital général* of Riom are in a report of 18 November 1785, A.D. Puy-de-Dôme, C 1043

[55] Letter of 20 January 1788, A.D. Aisne, C.733.

[56] Letter of 30 January 1788, A.N., F$^{15}$ 2796

[57] Report of 28 January 1789, A.N., F$^{15}$ 2808

[58] *Compte rendu au Roi au mois de mars 1788 et publié par ses ordres* (Paris, 1788), p. 125

[59] Bannefroy, *Mémoire sur la mendicité*, p. 26

Intendants were reminded that the regulations prescribed suitable rations and conditions in "prisons of passage," and forbade long periods of detention under pretext of waiting to form a convoy. Such problems were compounded by the fact that transfers occurred not only on the way to a depot, but from one depot to another as beggars were returned to their places of origin. Chronic problems ensued from this policy; in 1788, La Millière raised hopes that it would be changed.[60]

Colombier's regulation had stipulated that transfers would only be made between March 1 and November 1, but Colombier himself allowed exceptions in order to relieve severe overcrowding of the depot at St. Denis. As a result of being required to absorb large numbers of the "dangerous" poor arrested by the police of Paris, St. Denis became an overcrowded transit camp and a breeding ground for illnesses that were carried to other depots.[61] Many "abuses" arose when inmates were herded into carts and escorted by poorly paid constabulary from one day's jail to the next. Investigation of one complaint revealed that inmates in transit had sold their officially provided clothing (no doubt receiving some tatters into the bargain), and had purchased "strong liquors;" the guards had not interfered.[62] From St. Denis, inmates frequently journeyed to Soissons, where the abbé de Montlinot, appalled by their miserable condition, attempted to accommodate the motley crew in his own model depot. On one occasion, he wrote angrily to his friend Favart, secretary to the intendant of Soissons:

> If I held by the chignon the medico-chemico-bureaucracy of St. Denis, I would put them in the way of their patron, so wroth am I. . . . I know no one so badly served as M. B[ertier]. with his great and little inspectors; I for one know six of them who *belabor* St. Denis, may the Good Lord keep them!
>
> "O Wisdom of the gods, I trust thou art most deep,
> But to such shallow sirs thou giv'st the world to keep?"[63]

Colombier was aware of the problems of transfers, but could not relieve overcrowding of the depots otherwise. He seems to have sensed the political danger of allowing the depots to be seen as agents of despotism. In the Spring of 1789, he complained to La Millière that the night searches for beggars authorized by the *lieutenant de police* of Paris were illegal.[64] Soon afterwards, true to the ideal of the patriotic doctor, Colombier fell ill and died, exhausted by his labors, on an inspection tour in Alsace, August 4, 1789.[65]

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[60] Circular of La Millière to intendants, 16 May 1788, copy in A D Calvados, C.657.

[61] Comment by Colombier on complaint from intendant of Limoges, 23 December 1787, A N , F$^{15}$ 2810, Colombier spoke of the large numbers transferred to St Denis by the Paris police in a letter of 24 July 1788, A D. Aisne, C 710

[62] Bertier to intendant of Soissons, 26 March 1782, A D Aisne, C 714.

[63] The undated note to Favier is from about 1785, A D Aisne, C 744, Montlinot uses the spelling "medico-chemico-burocratie," I have not located the source of the two lines he cites "O sagesse des dieux, je vous crois très profonde/Mais à quels plats messieurs livrez vous le monde!"

## *Conclusion*

Colombier is remarkable above all for the range of his institutional experience. He was thus placed in a unique position to integrate the expertise of several distinct domains. The bureaucratic cast of his thinking was mature in the *Code de médecine militaire* of 1772. It was clear to him that individuals trained in rational procedures could be forged into a complex machine obedient to state direction. Institutional involvement reinforced his belief in the validity of scientific prescriptions for administrative practice. Irrational methods of administration revealed their shortcomings in failures of hygiene and health. Rational bureaucracy, on the other hand, had the advantage of reducing morbid statistics, and it created a system of observation posts for empirically based improvements in the medical art.[66]

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[64] A N , $F^{15}$ 2811. Colombier probably inspired the sharp letter of La Millière to De Crosne on this subject, 3 July 1789

[65] Gallot-Lavallée, *Colombier*, p 95

[66] Cf Toby Gelfand, "A clinical ideal Paris 1789," *Bull Hist Med*, 1977, *51*, p 401, on the clinic as written observation," and Chambon's debt to Colombier

# THE MUNICIPAL CLINIC OF SAN FRANCISCO: A STUDY IN MEDICAL STRUCTURE

NEIL LARRY SHUMSKY

The world of contemporary medicine is one of structure, bureaucracy, and organization. At the federal, state, and local levels, large impersonal institutions determine the nature and quality of health care, and the way in which it is provided. In Washington, the National Institutes of Health employ more than 10,000 people and have an annual budget exceeding two and a half billion dollars. Scattered across the country, an enormous research establishment seeks new cures and methods of treatment, and, in every state, medical associations and societies judge the qualifications of individual doctors. No major city in the country is without its multifloor hospital employing hundreds of individuals, and even private physicians have discovered the advantages of group practices with their own laboratory and nursing staff in addition to several doctors.

Unfortunately, we know far too little about the emergence and development of today's highly-structured medical institutions. We do not know how, when, where, or why medical organization and bureaucracy began to appear and change the delivery of health care, the nature of the doctor-patient relationship, or the means of paying for medical aid, just to give a few examples. Historians of medicine have generally tended to study treatments and cures rather than the institutions within which treatment has been provided, and those who have studied institutions have generally written internal administrative histories without reference to larger issues about the relationship between structure and function.

However, it is probably safe to say that modern medical bureaucracy began to develop during the late nineteenth and early twentieth centuries. During this era, a general organizational revolution took place in America as depersonalized, bureaucratized organizations began to replace unstructured, spontaneous groups. As the nation grew in size and population, institutions generally ceased to function on a small-scale informal basis and became large-scale and formal. One especially important aspect of this transformation was a change in the internal structure of institutions. As their nature changed and they became larger, more impersonal, and more com-

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plex, they needed new forms. Businessmen in particular recognized this need, and historians have analyzed many of their attempts to restructure firms. Daniel C. McCallum, superintendent of the Erie Railroad, devised one of the first organization charts ever printed by an American company and instituted new ways of administering field units. A few years later, managers of the Pennsylvania Railroad carefully defined the lines of communication and authority between various units of the transportation department, and also between the transportation, traffic and accounting departments. After about 1890, administrators in the steel, meatpacking, electrical manufacturing and agricultural implement industries began to design new systems of managing their plants and offices. They had also started to establish structures to administer field units and had begun to create central offices to run their companies as a whole. The appearance of great integrated enterprises in the last decade of the nineteenth century produced the first sizeable administrative structures in American industry. Gustavus Swift, James B. Duke, James S. Bell and Andrew Preston reorganized the meatpacking, tobacco, flourmilling and banana businesses respectively. They built nationwide distributing and marketing organizations with district offices, warehouses, and retail outlets. In order to run these enormous companies, they established centralized headquarters which directed nearly all of the activities of the firms' various divisions. This organizational structure allowed economies of scale and enabled production to be consolidated in a few large factories. They could reduce unit costs, specialize further, and subdivide the manufacturing process.[1]

Business institutions were not the only ones to be reorganized around the turn of the century, although they are probably the best known. The development of social work as a profession between 1880 and 1930 reveals the importance of such organizational attributes as specialization and the idealization of expertise, the growth of an occupational subculture, and bureaucratization.[2] In more recent decades, the nation's churches have also undergone a kind of organizational revolution. New bureaucracies have exerted an influence on all denominations as religious groups have assumed special functions related to human rights, economic life, political development, international concerns, welfare structures, educational activities, and mass media.[3] In fact, there is hardly an area of American life which has not become part of the organizational society.[4]

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[1] Alfred D Chandler, Jr , *Strategy and Structure Chapters in the History of the Industrial Enterprise* (Cambridge MIT Press, 1962), 19-51

[2] Roy Lubove, *The Professional Altruist The Emergence of Social Work as a Career, 1880-1930* (Cambridge Harvard University Press, 1965)

In order to place contemporary medicine within this context, we need case studies of specific institutions which are organized around four key questions. What was the structure of the institution? Why was that particular structure created? Did the structure have any identifiable precedents? And, does the study provide any insights about the development of medical institutions in general? The present study attempts to answer these questions by analyzing the Municipal Clinic of San Francisco, an unusual health care facility which opened its doors on March 25, 1911. The purpose of the Clinic was to lessen the incidence of venereal disease in the city, and the San Francisco Board of Health required that every prostitute be examined twice a week here for venereal infections. If a woman was found to be healthy, she was allowed to practice her profession without official harassment; if she was found to be diseased, she was given free medical care and forbidden to practice prostitution until pronounced cured.[5]

Today, the Clinic has been almost completely forgotten despite its historical significance. For its history indicates the extent to which even small health care facilities had been influenced by the organizational revolution then taking place in American society and were being structured in accordance with modern bureaucratic principles. This Clinic was not simply a doctor's office in which physicians examined and treated their patients. It was a complex organization characterized by a system of abstract rules, a

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[3] Gibson Winter, "Religious Organizations," in W Lloyd Warner et al , The *Emergent American Society,* I (New Haven Yale University Press, 1967), 408-91

[4] See, e g , Samuel Haber, *Efficiency and Uplift Scientific Management in the Progressive Era, 1890-1920* (Chicago University of Chicago Press, 1964), Samuel P Hays, *Conservation and the Gospel of Efficiency The Progressive Conservation Movement, 1890-1920* (Cambridge Harvard University Press, 1959), Louis Galambos, "The emerging organizational synthesis in modern American history, *Business History Review,* 1970, *44* 279-90, Jerry Israel, ed , *Building the Organizational Society Essays on Associational Activities in Modern America* (New York Free Press, 1972), George Rosen, "The efficiency criterion in medical care, 1900-1920 an early approach to an evaluation of health service " *Bull Hist Med* , 1976, *50* 28-44.

[5] The history of the Clinic is presented in several articles and pamphlets The most important of these are "Regulations of the Municipal Clinic ' in [San Francisco, Board of Health], *Annual Report for the Fiscal Year . 1910 to . 1911* (San Francisco n publ , 1911), 2-6, Julius Rosenstirn, 'The Municipal Clinic of San Francisco,' *Transactions of the Fifteenth International Congress on Hygiene and Demography,* IV (Washington GPO, 1913), 93-115, idem, *The Municipal Clinic of San Francisco* (New York William Wood and Company, 1913); and *Our Nation's Health Endangered by Poisonous Infection through the Social Malady, the Protective Work of the Municipal Clinic of San Francisco and Its Fight for Existence* (San Francisco Town Talk Press, 1913) See also Testimony of Julius Rosenstirn in the case of *Rosenstirn v Westenberg,* Franklin Hichborn Papers (Ms , Public Affairs Service, UCLA Library, Los Angeles), Commonwealth Club of California, "The Red Plague, ' *Transactions*, 6 (1911), Commonwealth Club of California, "The Red Plague, Second Report,' *Transactions*, 8 (1913)

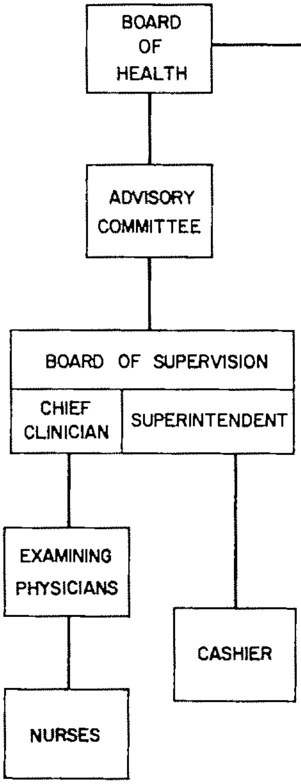

Figure 1. The Structure of the Clinic

clearcut hierarchy of authority, the specialization and division of labor, and a spirit of formalistic impersonality.[6] Moreover, these traits did not evolve over time; the founders of the Clinic seem to have had a clear sense of structure and built these features into the original regulations which they issued in February of 1911.[7] They created a highly structured organization characterized by discipline, regularity, routine, and order.

At the top of the Clinic's organization chart was the Board of Health which established the Clinic and had ultimate responsibility for making regulations and setting policy. Immediately below the Board itself was an Advisory Committee which the Board appointed and to which the Health Commissioners delegated most of their authority. They gave it full charge of the administration of the Clinic and its finances and staffed it with prominent San Franciscans whose reputation was beyond question. The members of the first Advisory Committee were five of the city's most prominent clergymen, and the second committee included the President of the State Board of Health, two noted businessmen, the Superintendent of Public Schools, a prominent lawyer, and a Paulist Father.[8]

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[6] These characteristics of bureaucracy are taken from Peter M. Blau, *Bureaucracy in Modern Society* (New York: Random House, 1956), 28-31.

[7] [San Francisco: Board of Health], Minute Books: Minute Book, October 1–December 31, 1910, p. 749, Minute Book, January 1–March 31, 1911, p. 813 (Mss., San Francisco Department of Public Health, San Francisco), [San Francisco: Board of Health], *Annual Report, 1910 to 1911*, 1-2.

The reason for establishing the Advisory Committee and appointing these noted citizens had to do with reputation and respectability. In most turn-of-the-century American cities, prostitution was a major source of graft and corruption. In return for a regular system of tribute, judges, policemen, and city officials of every level looked the other way and did not enforce state and municipal laws. In San Francisco, the situation was no different. Policemen on the take received regular donations from every prostitute, and Mayor P. H. McCarthy was said to be an intimate associate of Jerome Bassity, one of the city's most notorious pimps. Several years before, in an investigation into municipal corruption, the grand jury had revealed the existence of the so-called municipal crib. Located at 620 Jackson Street, this brothel housed up to 120 prostitutes a night and had a weekly income of nearly $4000 from room rents alone. It received its colorful, and accurate, name because Abe Ruef, San Francisco's political boss, extorted one-fourth of the establishment's profit and divided it with his cohort in graft, Mayor Eugene Schmitz.[9]

Given this historical background, the Board of Health had to place the Clinic above reproach immediately. If it did not, the institution was almost certain to be seen as another means of exploiting the women of the tenderloin and lining the pockets of officeholders. In order to meet this problem head on, and demonstrate the Clinic's honesty and uprightness, the Board of Health created the Advisory Committee and filled it with thoroughly reputable San Franciscans. However, the appointment of this committee also began the creation of a hierarchical structure for the Clinic. First came the

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[8] Rosenstirn, *Municipal Clinic*, 3, Rosenstirn, "Municipal Clinic," *Fifteenth Congress on Hygiene and Demography*, IV, 95-6, Rosenstirn, *Our Nation's Health*, 19, George E Malsbary, "The Municipal Clinic of San Francisco," *Southern California Practitioner*, 1913, *28*, 162.

[9] San Francisco *Bulletin*, May 6-31, 1911, Anonymous, The Child, Unpublished Manuscript, Hichborn Papers, [San Francisco Board of Supervisors], *Report on the Causes of Municipal Corruption in San Francisco* . (San Francisco The California Weekly, 1910), 19, [San Francisco Grand Jury], Session of the Grand Jury, November 30, 1906 1 30 PM, Investigation in re 620 Jackson Street, Hichborn Papers, Walton Bean, *Boss Ruef's San Francisco* (Berkeley University of California Press, 1967), 45-6

Board of Health and just below it the Advisory Committee. But this rudimentary framework left unsettled questions about the day-to-day operation of the Clinic. Neither the clergy nor the businessmen were competent to supervise or conduct the health examinations of the prostitutes. The businessmen, who almost certainly could have looked after the Clinic's affairs, were probably reluctant to invest the time and energy that such a task would have entailed. Therefore, the Health Board created a second body, the Board of Supervision, to "have complete control of the operation of said clinic"[10] and be subject only to the regulations enacted by the Board of Health or the advisory committee. This third level in the chain of command thus established a clear distinction between ultimate responsibility and daily administration. The Public Health Commissioners and the Advisory Committee made major policy decisions, and the Board of Supervision managed the Clinic's everyday workings.

The Board of Supervision was appointed by the Board of Health upon the nomination of the Advisory Committee, and it consisted of two members, a Chief Clinician and a Superintendent.[11] While these two men had joint responsibility for hiring all employees of the Clinic and signing all orders covering the working of the Clinic, they had essentially separate spheres of power. The Chief Clinician supervised the health examinations themselves, and the Superintendent attended to the business affairs of the Clinic. Thus, not only was there a division between policy-making and daily management, but also between the medical and business aspects of day-to-day operations.

The Chief Clinician was "a reputable duly registered physician" and was to determine "the arrangement of service at the Clinic."[12] On his staff he had two Examining Physicians, a Chief Nurse, and an Assistant Nurse. Presumably, he was also in charge of the laboratory and bacteriologist. The Clinic regulations required the examining physicians "to make a complete physical and microscopical examination of each and every individual who may present themselves for such purpose" and to "administer the necessary medical treatment to render" every diseased individual "a healthy person."[13] The Chief and Assistant Nurse assisted the three physicians. They prepared the women for examination or treatment, were present at examinations, and "executed all orders for such attending physicians in matters relating to the purposes of . . . [the] regulations."[14] Obviously, the bacteriologist ran the laboratory. He performed Wasserman tests whenever they were indicated and examined all slides prepared by the doctors during each examination.[15]

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[10] "Regulations of the Municipal Clinic [San Francisco Board of Health], *Annual Report, 1910 to 1911*, 3

[11] *Ibid*

[12] *Ibid*

[13] *Ibid*, 4

[14] *Ibid*

While the Chief Clinician and his subordinates conducted the Clinic's health examinations, the other member of the Board of Supervision, the Superintendent, managed its business affairs. He was primarily responsible for record-keeping and watching over the clinic's finances.[16] One of the problems faced by the Board of Health was that of supervision and control; how to make sure that the city's prostitutes actually came to the Clinic and had their examinations. As a solution, the Health Commissioners designed a complicated system of records, forms and charts, and they placed the Superintendent in charge of it. Very briefly, the system worked as follows. Every prostitute was required to "submit to a medical examination" every four days. When she reported to the Clinic for the very first time, the Superintendent prepared three cards: a master card, a patient's certificate, and a control card. On the master card, he recorded her real and assumed names, places of work and residence, place of birth, age, race, height and weight. He also noted the color of her eyes and hair, her general build, any physical peculiarities, and her marital status. Then, he assigned her a number and filed the master card in numerical sequence and kept it in his office.[17]

At the same time, the Superintendent prepared a patient's certificate which contained all the same information, and he gave it to the woman. When she came for each subsequent examination, she surrendered the certificate and received a "complete physical and microscopical examination . . . particularly with a view to determinig the existence of diseases of a venereal character."[18] If the results of the examination were negative, the patient's certificate was marked accordingly and returned to her. She could then practice prostitution until the next examination four days later. In the meantime, the Superintendent also recorded the examination results on the master card in his office and on the control card. While the master cards were filed numerically and kept in the Superintendent's office, the control cards were filed according to the day when a prostitute was due for her next examination. This file remained at the front desk and, after each negative examination, the Superintendent replaced the control card in the file for four days away. This way, he always knew when each woman was scheduled to report next.[19]

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[15] *Ibid*

[16] *Ibid*., 4-5.

[17] *Ibid*., Rosenstirn, *Municipal Clinic*, 6-8, Rosenstirn, "Municipal Clinic," *Fifteenth Congress on Hygiene and Demography*, IV, 95-100, Rosenstirn, *Our Nation's Health*, 22-23, Malsbary, "Municipal Clinic," 161-64.

[18] Regulations of the Municipal Clinic, [San Francisco Board of Health], *Annual Report*, 1910 to 1911, 4

If however, the results of an examination were positive and a woman was found to be infected with syphilis or gonorrhea, the procedures were different. In that case, the Superintendent did not return her card. Instead, he gave her a treatment card which entitled her to free medical care at the Clinic or, if necessary, the City and County Hospital. In the meantime, she was forbidden to work as a prostitute until the Clinic physicians declared her cured, at which time the Superintendent returned her patient's certificate.[20]

In addition to handling the paperwork and maintaining the Clinic's system of records, the Superintendent also kept track of the Clinic's finances, another of the problems confronted by the Board of Health when it established the Clinic. Although the San Francisco Board of Supervisors allowed the Board of Health to create the Clinic, it refused to appropriate funds for it As a result, the Board had to charge the prostitutes and set a fee of fifty cents for each examination.[21] Once again, the problem was one of reputation. The charge could all too easily be interpreted as a form of graft rather than a legitimate fee to help defray the costs of running the Clinic. In order to forestall this kind of accusation, the Health Commissioners devised a detailed system of bookkeeping and auditing. According to the Clinic regulations, the Superintendent was to appoint “a proper person,” to be confirmed by the President of the Board of Health, who would receive all fees and issue receipts for them. These receipts would be “in duplicate, appropriately numbered, the original to be handed to the individual paying the fees, the duplicate to remain in a bound book kept for such purpose.” The regulations also demanded that “a full, detailed and accurate record . . . be kept for all fees collected” and that these fees “be turned over to such custodian in such manner and at such times as may be hereafter designated.”[22] As it worked out, the Advisory Committee asked the Union Trust

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[19] *Ibid* , [San Francisco Board of Health], Minute Books Minute Book, October 1 to December 31, 1910, p. 749, Minute Book, January 1 to March 31, 1911, p 813 (Mss , San Francisco Department of Public Health San Francisco), Rosenstirn, *Municipal Clinic*, 6-8, Rosenstirn, “Municipal Clinic,” *Fifteenth Congress on Hygiene and Demography*, IV, 95-100, Rosenstirn, *Our Nation's Health*, 22-3, Malsbary, “Municipal Clinic, 161-64

[20] *Ibid*

[21] Rosenstirn, *Municipal Clinic*, 8, Rosenstirn, “Municipal Clinic, *Fifteenth Congress on Hygiene and Demography*, IV, 99. Rosenstirn, *Our Nation's Health*, 23, Malsbary, “Municipal Clinic, 164 Regulations of the Municipal Clinic,” [San Francisco, Board of Health], *Annual Report*, 1920 to 1911, 5 [San Francisco Board of Health], Minute Books Minute Book, January 1 to March 31, 1911, pp 813, 839

[22] “Regulations of the Municipal Clinic, [San Francisco Board of Health], *Annual Report, 1910 to 1911* 4-5

Company to audit its accounts every month. The Trust Company did so by balancing the amount of money it received from the Superintendent against the receipt book and the doctors' records of how many women they had examined each day.[23]

As a means of solving problems of reputation and supervision, the Board of Health, perhaps inadvertently, created for the Clinic a hierarchical structure characterized by a chain of authority. Management was separated from day-to-day responsibility, and the provision of health care was distinguished from the conduct of business affairs. Moreover, the Clinic's founders revealed precisely how important they considered its business aspects when they set the salary scale. The Superintendent received $150 a month, but the Chief Clinician only $100. The cashier and stenographer were paid $60 a month, but the two Examining Physicians only $50.[24] At the very least, then, the Health Commissioners realized that, for the Clinic to be successful, its business affairs needed the same close attention to detail as the examinations themselves. But, most important of all, they understood that the Clinic would not be successful unless its regulations were enforced. The Clinic could only reduce the incidence of venereal disease if the prostitutes showed up regularly for their examinations. The question was how to ensure the women's compliance.

The answer was to use the San Francisco Police Department. According to the Clinic Regulations, the police were "sanitary officers," and it was their duty "to cooperate with the Board of Health in the enforcement of the . . . regulations."[25] In order to carry out this provision, the Chief of Police permanently assigned two officers to the Clinic, George J. Ohnimus and Nicholas Barron, and they maintained "a vigilant . . . supervision and inspection . . . over all persons known to be or suspected to be of a questionable character." Because of the way in which the Clinic kept its records in general, and the system of control cards in particular, the two policemen could determine daily if every women who was supposed to have had an examination had actually shown up. At the end of each day, they checked the residence of every delinquent to find out why she had not appeared, arrest her, or make sure that she was no longer a prostitute. They were able to enforce the Clinic rules because they had the cooperation of the Police Court Judges who sent all women arrested for prostitution to the Clinic for an examination before settling their cases. If a woman did cooperate and agree to subsequent regular examinations, the judge suspended her sentence; if she did not, he fined or imprisoned her, or both.[26] Therefore, the prostitute had a strong motive for submitting to the Clinic rules.

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[23] Testimony of James Maguire, Supt. of the Municipal Clinic, in the case of *Rosenstirn v. Westenberg*, Franklin Hichborn Papers (ms., Public Affairs Service, UCLA Library, Los Angeles)

[24] [San Francisco Board of Health], *Annual Report*, 1910 to 1911, 6-7

[25] *Ibid.*, 6

The regulations of the Clinic, its organization and structure as well as its various levels of responsibility, satisfied three of the four criteria of a modern bureaucracy: a set of abstract rules, a hierarchy of authority, and the specialization and subdivision of labor. One of the Clinic's other procedures met the fourth criterion, a spirit of formalistic impersonality. The Clinic regulations required that no one other than the Clinic employees, the Chief of Police, and the President of the Board of Health have access to the Clinic records. In order to prevent collusion, even the Examining Physician did not know the names or addresses of their patients, only their numbers. The doctors never saw the patients' certificates, only examination cards. Each time a woman left the Clinic, she received back her certificate and one of these cards which told her when to return. When she came back the next time, she surrendered them both to the Superintendent. He detached a coupon from the examination card, inserted it in the certificate, and filed it to wait for the results. The rest of the card, identified only by the woman's number, he sent to the doctor who used it to report his findings. After the examination, the physician sent the results back to the Superintendent who recorded them on the patient's certificate and master card. Then he returned her certificate (assuming that the tests were negative) and gave her a new examination card to bring back at her next visit.[27] The result was a high degree of anonymity in a very intimate transaction. As one contemporary put it, "according to this plan, the personal equation that under ordinary circumstances must enter into the relationship between the doctor and his patient, and which might sometimes color his judgment, has been eliminated to a very large extent, and each patient is considered more as a designated number than as an individual."[28]

It seems quite clear that the founders of the Clinic intended it to function bureaucratically and in accordance with modern managerial principles of administration then developing and applied them to this small health care facility. But it is also evident that the managers of the new Clinic understood the same principles, and they arranged the Clinic's physical layout and system of examinations in accord with the same ideas. The Clinic was not simply a room in which a doctor examined a patient. It would be much more accurate to compare San Francisco's Municipal Clinic with an assembly line for venereal disease inspections or an army physical line-up. A woman coming to the Clinic for her regular check-up passed from station to station much as an automobile in the process of assemblage. Because each case was identical and the same kind of examination was being performed over and over, the work could be subdivided and parcelled out to an entire staff of doctors, nurses, microbiologists, and matrons. The doctors themselves did not do all the work and performed only those aspects which required their specialized training. The result was a high degree of efficiency.

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[26] *Ibid* , Rosenstirn, *Municipal Clinic*, 4, Rosenstirn, "Municipal Clinic, *Fifteenth Congress on Hygiene and Demography*, IV, 95, Rosenstirn, *Our Nation's Health*, 20, [Commonwealth Club of California], "The Red Plague—Second Report," 342-43

[27] Rosenstirn, *Municipal Clinic*, 6-8, Rosenstirn, "Municipal Clinic," *Fifteenth Congress on Hygiene and Demography*, IV, 96, Rosenstirn, *Our Nation's Health*, 28, Malsbary, "Municipal Clinic, " 163

[28] [Commonwealth Club of California], "Red Plague—Second Report, 350

When a prostitute arrived at the Clinic for her examination, she presented herself at the front desk, gave the clerk her certificate, and entered the first waiting room. When her turn came, the matron called her by number and escorted her to one of the four examination rooms. There a nurse helped her undress, prepared her for the gynecological examination, and remained with her until one of the physicians was available. After a doctor arrived, he performed an examination which took between two and four minutes. He inspected the woman's external genital organs, squeezed the glands of Bartholin at each side of her vulva, and stripped her urethra. He examined

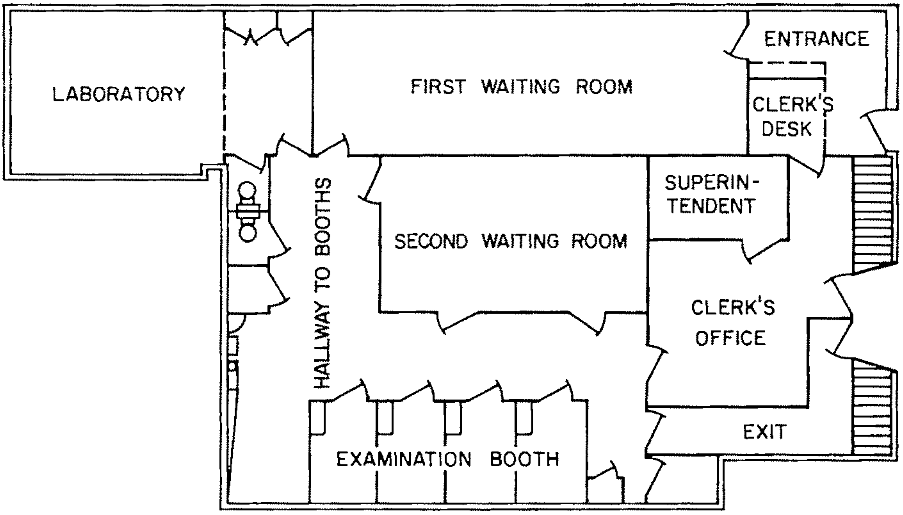

Figure 2. The Arrangement of the Clinic
[Source Rosenstirn, *Our Nation's Health*, 21; Rosenstirn, *Municipal Clinic*, 5]

the vagina and cervix through a speculum and finally glanced over her abdomen and thighs. If, during the course of this examination, the physician perceived any suspicious signs or discovered any discharge or secretion, he prepared a glass slide and ordered a microscopical examination. If he found an ulcer or any other clinical symptom of syphilis, he took a blood sample and called for a Wasserman to be performed. (Wasserman tests were routinely performed on all women at their first examinations.) Then he moved on to the next woman in the next booth, and his just-examined patient got dressed and went to the second waiting room to get the results of her inspection.[29]

If the clinical and laboratory examinations were both negative, she was free to practice prostitution for the next four days, but if any of the tests were positive and the presence of either syphilis or gonorrhea was confirmed, the woman's certificate was not returned and she was forbidden to practice prostitution until the Clinic doctors declared her cured. In the meantime, the woman had a choice; she could either select a private physician to treat her or choose to go to the Clinic. If she took the latter alternative, there was no charge for her treatment. The Clinic generated enough revenue from the fifty-cent examination fee to treat diseased women without charge, and it even purchased special considerations for them at the City and County Hospital. Under no circumstances were patients at this public facility ever charged, but, in return for free treatment, they were normally used for demonstrations before student classes. Since such a procedure would have been extremely embarassing to the prostitutes, the Clinic paid the Hospital one dollar a day to treat them, and they were excused from the objectionable practice.[30]

Unfortunately, none of the remaining literature describes the treatment which the Clinic provided for gonorrhea, but it did use the latest available drugs for treating syphilis. If a woman was found to have syphilis, she was given two injections of neo-Salvarsan (neo-arsphenamine) a week apart "and as many subsequent injections as . . . [were] considered necessary." When "clinical evidences of disease under this and subsequent treatment" disappeared, the woman's certificate was returned, and she was allowed to resume her business.[31]

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[29] Rosenstirn, *Municipal Clinic*, 6-8, Rosenstirn, "Municipal Clinic, *Fifteenth Congress on Hygiene and Demography*, IV, 95-100, Rosenstirn, *Our Nation's Health*, 22-3, Malsbary, "Municipal Clinic, 161-64

[30] Rosenstirn, *Municipal Clinic*, 8, 10, Rosenstirn, "Municipal Clinic," *Fifteenth Congress on Hygiene and Demography*, IV, 99, 102, Rosenstirn, *Our Nation's Health*, 22-3, Malsbary, "Municipal Clinic," 164-65

[31] [Commonwealth Club of California], "Red Plague—Second Report," 410, Rosenstirn, *Our Nation's Health*, 22

Once again, procedures at the Clinic probably developed out of specific problems and concerns. No one knew exactly how many prostitutes lived in San Francisco but, between March of 1911 and December of 1912, the Clinic doctors examined 60,736 women, an average of 116 per day. Moreover, the magnitude of the number increases when it is realized that the Clinic was only open for examinations between one and five o'clock in the afternoon, merely four hours.[32] Because of this large caseload, the examinations had to be performed as quickly and expeditiously as possible, with no wasted time. At the same time, the prostitutes themselves did not want to spend any more time than necessary at the Clinic. They worked during the night, slept during the day, and frequently had only the afternoon hours to themselves. In some houses, they were on call from two in the afternoon until three in the morning. Therefore, they were unwilling to spend hours in a doctor's waiting room. The solution which the Clinic devised—the various waiting rooms, the examination booth routine, and so on—solved the problem of time and grew out of the nature of the work.

Thus, the structure and functioning of the Clinic seem to have developed in response to various problems which the founders of the institution recognized, at least implicitly. Problems of reputation, supervision, control and efficiency produced a system characterized by a chain of responsibility, the subdivision and specialization of labor, the separation of policy-making from daily operation, and the separation of the medical from the business affairs of the organization. In addition, the Clinic possessed a highly rationalized physical layout which enabled the doctors to perform a large number of examinations very quickly. The question which then naturally arises is the source of these notions. How did the men who created the Clinic come to these solutions? How did they happen to realize that they could solve many of their problems through creating structure and organization? Did they derive their ideas independently or were they essentially imitators?

Answering these questions is quite challenging. None of the founders of the Clinic seems to have left any papers. The records of the Clinic itself have disappeared, and contemporary pamphlets and articles simply describe the Clinic's structure without explaining its origins. Therefore, in seeking sources for the Clinic's organization, it becomes necessary to deduce what knowledge the founders may have had when they established the new institution. According to Dr. Julius Rosenstirn, the originator of the Clinic idea, the Civic League of San Francisco conducted an "extensive correspondence" and obtained "thorough information on the various systems in vogue" before the Clinic was set up.[33] Although that correspondence no longer exists, the League's Report makes it clear that the men who created the Clinic knew about the systems of regulation in a number of major European cities. They received information from Antwerp, Hamburg, Berlin, Bremen, Vienna, Budapest, and St. Petersburg, as well as from St. Louis.[34] Although none of these cities had a system with quite the same degree of structure and bureaucratization as that of San Francisco, the Clinic's founders did seem to borrow elements from several other cities, most notably St. Louis and the Central European capitals, and combine them to create the Municipal Clinic.

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[32] "Regulations of the Municipal Clinic," [San Francisco. Board of Health], *Annual Report*, 1910 to 1911, 3-4, Rosenstirn, *Municipal Clinic*, 16, Rosenstirn, "Municipal Clinic," *Fifteenth Congress on Hygiene and Demography*, IV, 105, Rosenstirn, *Our Nation's Health*, 27, Malsbary, "Municipal Clinic," 166

It was only natural for the Clinic's organizers to rely on the St. Louis experience of the early 1870s as a precedent for their own work. St. Louis was, after all, the only other American city to institute regulation of prostitution for a lengthy period, and the experiment was well known throughout the country. In fact, one member of the 1871 San Francisco Board of Health, a physician named Gustavus Holland, visited St. Louis that same year. He had been studying systems of regulation in Europe and ultimately introduced a measure requiring San Francisco prostitutes to have regular medical examinations. The bill, which eventually reached the Board of Supervisors and state legislature before dying, was explicitly based upon the St. Louis ordinance. Although the Clinic of 1911 to 1913 did not draw as heavily upon the St. Louis experiment as did Holland's earlier measure, certain of its organizational features may have been derived from the experience of the mid-Western city. In St. Louis, as in San Francisco, the records were secret; only the Board of Health, Board of Police Commissioners, Chief of Police, and examining physicians had access to them. Furthermore, there were also close accounting procedures in St. Louis. Prostitutes paid $1.50 a week for their examinations, and triplicate receipts were issued: one copy to the city comptroller, one to the auditor, and one to the Board of Health.[35]

Despite these similarities, one fundamental difference distinguished the

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[33] Rosenstirn, *Our Nation's Health*, 18, Rosenstirn, *Municipal Clinic*, 1

[34] George Frink, Report of the Chairman of the Committee on Education and Public Morals of the Civic League in Conjunction with a Committee on Public Health, Pure Food and Tenement Legislation (Ms , Red Plague Committee Papers, Commonwealth Club of California, San Francisco)

[35] For information on the St Louis experiment, see John C Burnham, "The Social Evil Ordinance—A social experiment in nineteenth century St Louis," Missouri Historical Society *Bulletin*, 1971, *27* 203-17, and John C. Burnham, "Medical inspection of prostitutes in America in the nineteenth century the St Louis experiment and its sequel," *Bull Hist Med*, 1971, *45* 203-18

San Francisco and St. Louis experiences. In the Missouri city, there was no actual clinic. The Board of Health simply divided the city into six districts and appointed a like number of physicians to inspect the prostitutes in a particular part of town once a week. In this regard, St. Louis resembled European cities such as Paris, Brussels, Rome and Vienna. However, by the time San Francisco established its system of regulation in the early twentieth century, the inadequacies of examining the women at their lodgings had been recognized. Ehrlich had propounded the germ theory of disease, Neisser had discovered *Neisseria gonorrhoeae* and Schaudinn and Hoffman had found *Treponema pallidum*. As a result, physicians understood that examinations had to be made under sanitary conditions and that an accurate diagnosis often depended on a microscopical examination. Since neither cleanliness nor laboratory facilities were generally available in brothels, a number of cities began establishing separate clinics to perform the inspections. Among the cities having the best facilities were Berlin, Budapest, and Vienna; and San Francisco seems to have derived various aspects of the Municipal Clinic, especially the use of police and system of record-keeping, from their systems.

Perhaps the most significant borrowing was the relationship between the police and health officers. Historically, European police departments had treated prostitutes as a threat to public decency and morality; but now they began to perceive the women as a threat to public health. In Vienna, examination of prostitutes was "considered a sanitary measure and not a police measure," and the "morals police control [was converted] into a sanitary control." One prominent official of the Berlin morals police remarked that his bureau "should certainly now be called the health police," and, in that city, the medical policy of the police department was directed by a physician, the only European "instance . . . of expert medical control of what is admittedly a sanitary matter."[36]

In addition to providing a precedent for the San Francisco system of having the police assist the Board of Health (instead of direct it) and enforce regulations issued by that body (instead of make them), the Central European cities also pioneered in record-keeping. In both Berlin and Budapest, as in San Francisco, prostitutes were indexed at police headquarters according to the days of the week on which they were scheduled to be examined. At the end of the day, the remaining cards provided a list of those who had failed to keep their appointments. It is interesting to note that Berlin also had a logistics problem. Each of the eight physicians at the Clinic made between

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[36] Frink, Report, Red Plague Papers.

1500 and 2000 clinical examinations per month, and each of the laboratory assistants performed about 100 microscopical examinations a day. One can certainly speculate that Berlin also devised its system of records as a response to the need for speed and efficiency.[37]

It seems fairly clear that the founders of the Municipal Clinic were influenced by the systems of reglementation used in Central European cities. Not only are there marked similarities between the organization of the clinics in San Francisco and Berlin, Budapest, and Vienna, but there are other more subtle indications as well. In the actual report of the Civic League recommending the Clinic, far more space is devoted to a discussion of these cities than others. Moreover, in Rosenstirn's first article about the work of the Clinic, the bibliography includes several books and articles about the regulation of prostitution in Austrian and Prussian cities.[38]

However, to say that San Francisco's Municipal Clinic was influenced by various European precursors does not fully explain its high degree of structure and bureaucracy. None of these earlier systems possessed nearly the same degree of organization as the Clinic, and to say that they provided useful precedents does not adequately reveal the sources of the Clinic's structure. It may be that the only way to comprehend the Clinic is within the larger context of medical history during the late nineteenth and early twentieth centuries.

The experience of the Clinic strongly suggests that the development of the germ theory, the emergence of medical paraprofessionals, and the advent of medical specialization were all major factors which contributed to the structuring of modern medical institutions. At the Clinic, the rational physical facility and the highly efficient system of examinations were almost certainly related to the high degree of specialization. The Clinic doctors essentially performed the same task hundreds of times a week; they gave each patient precisely the same examination. Therefore, it was possible to develop a routine. Because each case did not require special treatment and handling, the staff knew in advance what would be done for each patient and could develop rational and efficient procedures. At the same time, specialization seems to have been related to record-keeping and depersonalization. When physicians began to specialize, the nature of their relationship to patients changed. Previously, they had seen their patients in a variety of contexts. Whenever a patient became ill, regardless of the malady, they saw him. But, under conditions of specialization, the doctors began to see each patient much less frequently—for only certain ailments. Therefore, each specialist could have many more patients. But, because the doctor treated more individuals, he came to know less about each of them. Their personal relationship changed, and it became necessary to keep records since doctors could no longer carry each case history in their heads.

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[37] *Ibid*

[38] *Ibid* , Rosenstirn, *Municipal Clinic*, 33-4.

Specialization is also related to a second factor which seems to have influenced medical organization and bureaucracy, the development of paraprofessional services. The structure of the Clinic would have been impossible without the existence of trained nurses and laboratory technicians to assist the physicians. As the doctors' work became, at one and the same time, more routine and more specialized, the appearance of paraprofessionals enabled physicians to perform only those tasks which required their particular expertise. Other members of the staff could do work which did not demand the doctor's presence and which might require a different sort of skill or training. Doctors had better things to do with their time than assist patients on and off the examining table, and they were generally not equipped to do laboratory work. Therefore, the emergence of nursing and microbiology were crucial preconditions to the bureaucratization of health care.

The development of microbiology and the appearance of trained laboratory technicians provides still another insight into the origins of structure at the Clinic in particular and medical facilities in general. Organization and bureaucratization would almost certainly not have been possible without the prior development of the germ theory. The isolation of specific microorganisms as the cause of disease obviously led to the use of laboratories as a diagnostic tool and thus contributed to structure. But more than that, the germ theory and its implications about the way in which disease spread made it clear that the health of any one individual was not simply a personal matter but rather one which could affect all of society. Therefore, it became clear that government had to play an ongoing role in the provision of health care. However, since government officials generally knew little about actually giving medical attention, and since most doctors were neither politicians nor managers, several crucial distinctions emerged—between policy-making and daily management on the one hand and between medical and business affairs on the other. Once again, the history of the Clinic illustrates this point.

In conclusion, then, San Francisco's Municipal Clinic is significant not only as an example of what did not generally happen in American medical history (the legalization of prostitution and compulsory medical examination of prostitutes) but also as an example of what did happen (the organization and bureaucratization of health care facilities). The history of the Clinic reveals that, by the early years of the twentieth century, even small medical institutions were being formally structured and suggests some reasons for the process to have occurred: the specialization of medical institutions, the development of paraprofessional medical services, and the development of the germ theory of disease.

# THE NEW JERSEY RADIUM DIAL PAINTERS: A CLASSIC IN OCCUPATIONAL CARCINOGENESIS*

WILLIAM D. SHARPE

Luminescence, including radioluminescence, had been known for centuries when Henri Becquerel's investigation of this natural phenomenon led to the discovery of radioactivity. The Curies isolated radium at the turn of the century, and among the earliest uses of naturally occurring radioactive substances was the preparation of radioluminous compounds. The most efficient phosphor—zinc sulfide—seems to have first been synthesized by Sidot in 1866. It was very soon appreciated that trace metals were required for an efficient phosphor and Giesel, in 1902, prepared radioluminous material by precipitating radium-226 with barium hexacyanoplatinate. In 1903 he demonstrated zinc sulfide's practical potential as a detector for alpha particles emitted by radioactive substances. The commercial potential of radioluminescence was soon appreciated and although a vast literature exists, much of the early history of radioluminous material preparation and handling remains obscure, many formulae and processes remaining trade secrets to this day.[1]

About 1910, radium-activated luminous paint was used on some of the more expensive Swiss and German watches but, toward 1913, a cheaper paint was developed in the United States. This paint was probably—documentation is lacking—invented by Sabin A. von Sochocky, a Viennese who took a degree in medicine at Prague and later studied atomic physics

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* Read before the Beaumont Medical History Club of Yale University, November 21, 1975. Manuscript prepared during tenure of Contract E(11-1)-3377, Medical Research Branch, Division of Biology and Medicine, United States Energy Research and Development Administration.

[1] The literature of radium and mesothorium injury in man is voluminous. Although incomplete, bibliographies are provided in A. A. Moghissi and M. W. Carter, *Public Health Implications of Radioluminous Materials* (Rockville, Md.: U. S. Department of Health, Education and Welfare, PHS, Food and Drug Administration, Bureau of Radiological Health, July 1975, DHEW Publ., FDA, 76-8001), and in W. D. Sharpe, "Chronic radium intoxication—clinical and autopsy findings in long-term New Jersey survivors," *Environmental Research*, 1974, *8*: 243-383. Dr. Samuel Berg, of Newark, a former resident and for some years assistant to Dr. Harrison S. Martland at Newark City Hospital, has recently brought out a book dealing with Martland and his era—*Harrison Stanford Martland: The Story of a Physician, a Hospital and an Era* (New York: Vantage Press, 1978). Dr. Berg was kind enough to permit me the use of the manuscript before its publication. I thank him for this, and for many past courtesies, as well as for a critical reading of this manuscript.

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with Lord Rutherford at the Cavendish Laboratory. (It will be recalled that Rutherford had identified the alpha particles of radium as helium nuclei in 1899.) This water-based paint contained radium-226 and radium-228 (mesothorium) in a zinc phosphor with trace metals added to provide color. The zinc sulfide itself decomposed because of alpha particle bombardment, and the cheaper mesothorium proved an acceptable luminizer despite its relatively short half-life, although better grades of paint continued to contain radium-226. Dr. von Sochocky was physicist and part owner of the radium company whose plant in Orange, New Jersey, was the largest firm of its kind in America. He later cooperated fully in studies of the radium workers until he died on November 14, 1928, of aplastic anemia.

Many radioluminous dials were used during World War I, chiefly for aircraft and submarine instruments and for watches. An industry developed and, after the war, new markets were sought so that door knobs, light switches, novelties and relatively inexpensive watch and clock dials were widely sold, despite a minority voice which deplored the frivolous use of a rare substance with considerable potential for medical use. Although some early colleagues of Roentgen and the Curies had warned of danger, it was not until the New Jersey disaster that it was appreciated just how dangerous internally deposited radioactive substances could be. The use of radioluminous paint proved to be a fad which, by 1926, had run its course. Fears of radium, publicity given to the injured workers and litigation killed the industry. The company moved from Orange in 1926, and its plant was torn down in 1930.

Between 1917 and 1924, when the industry in New Jersey began its collapse, something like a thousand people worked at radium extraction and dial painting in northern New Jersey, most of them in Orange. Many worked only a few days and at no one time do more than about three hundred seem to have been so employed. Jobs were scarce, especially for relatively unskilled women; the pay was good for that time and place; work was light and working conditions seemed pleasant. Workers interviewed a generation later agreed that "it wasn't like the usual factory job at all." Yet, from a population not much larger than a thousand, by 1925 five had unequivocally died of radium intoxication. In 1931, Dr. Harrison S. Martland, the Essex County Medical Examiner, reported eight autopsies from eighteen deaths between September 1922 and June 1931 which he considered due to radium.[2] Ten had jaw necrosis and aplastic anemia, and five had osteogenic sarcomas. He then regarded the former as *early* and the latter as *late* manifestations of radium intoxication.

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[2] H S Martland, "The occurrence of malignancy in radioactive persons," *Amer J Cancer*, 1931, *15* 2435-2516

### *The Radioluminous Paint*

Exact written formulae of the radioluminous paints have not survived but retrospective analyses suggest that the typical paint used about 1920 consisted of between 0.7 and 0.4 mg. of a natural radioactive substance (the "luminizer") for each 100 grams of dry, finely powdered, heat crystallized zinc sulfide containing traces of metallic salts—cadmium 0.05%, copper 0.001%, manganese 0.002%. Zinc sulfide, by itself, fluoresces poorly and the trace metals were required to produce a commercially useful paint. Alpha particles released during radioactive decay activated the zinc sulfide-trace metal phosphor to produce innumerable flashes of light which the human eye fused into a steady glow whose color depended on the trace metals.

Until about 1918, only radium-226 (as the insoluble sulfate or bromide) was used as the luminizer but, after 1918, mesothorium (radium-228) and possibly radiothorium (thorium-228) were added to or replaced the much more expensive radium-226. These cheaper elements were byproducts of the local gas mantle manufacturing industry and, although their availability varied, they yielded a cheaper and commercially acceptable radioluminous paint used in New Jersey and sold to manufacturers in other areas for use with inexpensive watches, clocks and novelty items.

Dry paint used in the New Jersey plants was weighed out in small glass vials, and that intended for watch dials was ground in small mortars just before use with distilled water, gelatin and gum arabic. This slurry was diluted and issued in 10 ml. lots. The water-based paint required brush application whereas most radioluminous paint used elsewhere had an oil or varnish base, and was applied using glass rods or metal pen nibs. Characters were painted, in the New Jersey plant, using very fine camels' hair brushes which most workers pointed by drawing against the corners of their mouths. Payment was by "piece work," usually $1.44 for 24 dials, and a dextrous worker could earn a substantial income in those lightly taxed days. However, the more characters she painted, the more frequently she pointed her brush and the more paint she swallowed. Dr. Frederick L. Hoffman, a Prudential Life Insurance Company statistician, calculated that if a worker painted 250 dials daily, and many did, licking fourteen times for each dial, she would swallow about 1.75 grams of paint daily, representing a weekly dose of 15 to 125 micrograms of radioactive material.[3] Martland's 1929 paper refers to a *New York Journal* reporter's interview with Madame Sklodowska-Curie during which she expressed surprise that the government would permit such careless handling of radium and mesothorium, regarding swallowing such materials as "criminal."[4]

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[3] F L Hoffman, "Radium (mesothorium) necrosis," *J.A M A* , 1925, *85* 961-65

### *Jaw Necrosis*

In October 1922, an Orange physician reported what he called phosphorus necrosis of the jaw in a radium dial painter who had quit work two-and-a-half years earlier in good health after working two-and-a-half years. Because phosphorus poisoning was, at the time, a compensable disease, both local and state health departments investigated the plants, but found no phosphorus in use and therefore noted no violation of state laws. Martin Szamatolski, Ph.D., of the New Jersey State Department of Health, wrote in a letter dated January 30, 1923:

> As far as I know, luminous compounds do not contain phosphorus, but are usually compounds of zinc to which are added about 1/1,000th part of radium and mesothorium compounds. It is my belief that the serious condition of the jaw has been caused by the influence of the radium. . . .

Samples of the radioluminous paint were analysed, and on April 6, 1923, he wrote: " . . . such trouble as may have been caused is due to the radium contained therein. . . . "[5] This was an unsupported, but accurate, opinion.

Between 1922 and 1924, seven radium dial painters died. No investigations were conducted, and their death certificates list a variety of causes: ulcerative stomatitis, syphilis, gingivitis, primary anemia, sepsis, Vincent's angina and jaw necrosis due to phosphorus poisoning. Autopsies were not performed because there was no evidence of foul play, and because the State Health Department had passed the plants as safe. Radium osteonecrosis and radium-induced aplastic anemia were at that time unknown. The first radium dial painter known to have died (September 12, 1922) was exhumed in 1927, and 48.4 micrograms of radium-226 were found in her body, approximately five thousand times the presently accepted maximum permissible body burden for an industrial worker using radioactive materials.[6]

Local dentists had many young women under treatment for stubborn, refractory mandibular and maxillary osteomyelitis which resisted the usual treatments and which was known locally as "jaw rot." So poorly did this behave that many dentists refused to perform extractions or oral surgery on dial painters for fear of initiating or aggravating jaw necrosis. In September 1924, Dr. Theodor Blum reported an unusual mandibular osteomyelitis in one patient. Uncertain as to etiology but sure that he had not before encountered a similar case, he mentioned "radium jaw" in a footnote to his published article.[7]

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[4] H. S. Martland, "Occupational poisoning in manufacture of luminous watch dials, *J A M A* , 1929, *92* 466-73, 352-59

[5] These quotations are from Samuel Berg, *Harrison Stanford Martland* (New York Vantage Press, 1978), p 169.

[6] A W St George, A O Gettler and R H Muller, "Radioactive substances in a body five years after death,' *Arch Pathology*, 1929, *7* 397-403

Meanwhile, the industry grew: 8500 dials were painted during 1913, and 2,200,000 during 1919. By 1924, the New Jersey Consumers' League for the Improvement of Working Conditions was complaining about objectionable fumes from the plant in Orange, and was concerned about possible occupational poisoning. This concern seemed well-founded: "acid fumes ate clothes right off the line" and an aspiring opera singer claimed that his voice had been "ruined" by the same fumes. Neither the State Department of Labor nor the United States Public Health Service acted, but the Consumers' League remained interested.

Noting the incidence of jaw necrosis, anemia and mouth lesions, Dr. F. L. Hoffman, in 1925, described five dead and twelve living patients with jaw osteomyelitis.[8] He suggested that this was due to mesothorium intoxication acquired by licking the brushes, and the *New York Times* duly reported "New Radium Disease Found" on May 30, 1925. After this statistical definition of jaw necrosis and anemia as an industrial disease, Dr. H. S. Martland entered the picture as County Medical Examiner and his 1929 paper credits Hoffman with the clinical definition of radium osteonecrosis.[9]

Although under New Jersey's Workmen's Compensation Law, as in most jurisdictions, only specifically named occupational diseases were compensable, the factory owners were seriously disturbed. They turned to Harvard Medical School, and invited W. B. Castle, Katherine R. Drinker and C. K. Drinker, who were environmental health and safety experts rather than radiation scientists, to visit the plant on April 16-18, 1924, but because of cases before the courts, their report was not published until August 1925.[10] They had been asked about *zinc* intoxication from the phosphor, and apparently not about radium or mesothorium. Local folklore has it that the workers' clothing, underwear and skin shone in the dark, but the radioactive luminizer and binding vehicles were added to the phosphor just before use, and this "shining in the dark" was probably the zinc sulfide which can be briefly activated by sunlight. The presence of radioactivity was established in some of the workers, and Castle, Drinker and Drinker suggested that the portals of entry were the skin and gastrointestinal tract. They recommended that dial painters refrain from pointing their brushes in their mouths on general principles and, although they did not draw a causal relationship between radioactive material and the injuries in question, they concluded that such a relationship could be assumed.

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[7] Theodor Blum, "Osteomyelitis of the mandible and maxilla," *J Amer Dental Assoc* , 1926, *11* 802-5

[8] Hoffman, *op cit* (n 3 above)

[9] Martland, *op cit* (n 4 above)

[10] W B Castle, Katherine R Drinker and C K Drinker, "Necrosis of the jaw in workers employed in applying a luminous paint containing radium, *J Industrial Hygiene*, 1925, 7 317-82

### *Aplastic Anemia*

On June 5, 1925, an East Orange physician reported the death of a 36-year-old chemist who had worked fourteen years at the radium plant Dr. Martland, as Medical Examiner, performed an autopsy the following day which revealed aplastic anemia and terminal bronchopneumonia. Fatal aplastic anemia had been reported among radium workers in Europe, but without postmortem confirmation, and this was apparently the first case to be published. Howard Baker, the radium company's chief chemist, cooperated fully and postmortem assays yielded 14 micrograms of radioactive material in the body, estimated to be five percent radium-226 and the remainder radium-228 (mesothorium). The report, delayed by litigation, appeared in August 1926.[11] Exactly one week after this first autopsy, Dr. Philip Conlon asked Dr. Martland to see in consultation a 35-year-old dial painter in St. Mary's Hospital, Orange, who had painted dials from 1917 until 1923. Her hemoglobin was 25 percent of normal and her leukocyte count less than 1000 c.mm. She died on June 17, 1925, and is reported in a paper by Martland, Conlon and Knef.[12] Drs. Conlon and Martland each had one additional case still alive, and the histology of aplastic anemia due to radium was first reported at a meeting of the New York Pathological Society in October 1925.[13]

In May 1925, Martland had examined two young dial painters, both of whom had necrosis of the jaw and severe anemia. Both soon died, and he performed autopsies. At about the same time, he examined the company's chief physicist, Dr. von Sochocky, and made the diagnosis of radium-induced anemia. Dr. von Sochocky had extracted approximately 30 grams of radium between 1913 and 1921, and will be recalled as the inventor of the water-based radioluminous paint used in New Jersey. He had chronic radiation dermatitis on his fingers and, while helping Dr. Martland assay radioactivity of the radium workers' exhaled breath, discovered that his own breath was more radioactive than theirs. He died in 1928 of the consequences of his invention.

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[11] G S Reitter and H S Martland, "Leucopenic anemia of the regenerative type due to exposure to radium and mesothorium, *Amer J Roentgenology*, 1926, *16* 161-67

[12] H S Martland, Philip Conlon and J P Knef, "Some unrecognized dangers in the use and handling of radioactive substances, ' *J A M A.*, 1925, *85* 1769-76

[13] H S Martland, "Some unrecognized dangers in the use and handling of radioactive substances, *Proceedings of the New York Pathological Society*, 1925, n s *25* 88-92

In October 1925, Dr. Martland defined radium intoxication as a new occupational disease, basing his report on the first two cases who came to autopsy and on five workers still alive whose expired air contained measurable radioactivity. He described how the radionuclides entered the body, the form in which they were ingested, their storage in the reticuloendothelial system, how they could be recovered, their demonstration by photographic and electroscopic methods, their presence in exhaled air, the characteristics of the anemia which occurred, the autopsy findings and the hopeless prognosis.[14]

In December 1926, Dr. Frederick B. Flinn, a physicist on the faculty of Columbia University's Institute of Public Health, had commented:

> An industrial hazard does not exist in the painting of luminous dials. The only evidence contrary to this conclusion rests in the fact that five employees at the plant in Orange, N.J., have died from some cause that cannot be determined at this date.[15]

Flinn believed that the jaw necrosis was due to bacterial invasion complicated by syphilis, but from the beginning had conceded all of Martland's data and observations. In May 1927, Flinn described a dial painter from Connecticut who had antral sinusitis, a spontaneous fracture of the femur and later died of aplastic anemia.[16] In 1928, he published his agreement that Martland had been correct.[17]

### *Cancer*

In September 1927, Dr. Martland performed an autopsy on a 35-year-old woman who had painted dials eight years earlier and had left work in good health. Autopsy revealed both osteosarcoma of the scapula and aplastic anemia, and postmortem assays demonstrated that her body contained about 50 micrograms of radioactive material, chiefly radium-228 (mesothorium). When he reported this in July 1929, he noted:

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[14] *Ibid*

[15] F B Flinn, "Radioactive material an industrial hazard?" *J A M A* , 1926, *87* 2078-81

[16] Idem, "A case of antral sinusitis complicated by radium poisoning," *Laryngoscope*, 1927, *37* 341-49

[17] Idem, "Some of the newer industrial hazards," *Boston Med & Surg J* , 1928, *197* 1309-14

> I interpret this sarcoma as possibly arising in a bone in which a previous radiation osteitis had existed for some time as a result of deposits in the skeleton. In reviewing the 15 fatal cases I had found evidence that one of the earlier cases had been diagnosed as sarcoma of the femur from the clinical course and the roentgen examination, and had been treated by high voltage roentgen therapy. This was never proved, however, at autopsy. The incidence of two osteogenic sarcomas in 15 patients dying as a result of an occupational poisoning by radioactive substances is too high to be mere coincidence.[18]

The earlier patient, a twenty-year-old dial painter, died in April 1924. Martland's 1931 review reports five osteosarcomas among eighteen dead radium workers.[19]

For some years during the middle twenties, many radium workers complained of "rheumatic pains" in their joints, especially in their hips, sometimes severe enough to interfere with sleep or walking. Then, in 1926 and 1927, and continuing thereafter, some had spontaneous, non-traumatic fractures of their long bones, especially the femur, which healed poorly.

Radium workers continued to die. From 252 death certificates dated from 1922 to 1966, sixteen are recorded for the decade ending in 1930: eight died of aplastic anemia, four from osteonecrosis and sepsis, and four of osteosarcoma. During the decade ending in 1940, two more died from bone necrosis or sepsis, and twelve died of cancer. Six were osteogenic sarcomas, two of the bronchus or lung and one each of breast, ovary, an unspecified sarcoma of the orbit and an unspecified sarcoma of the neck. During the decade ending in 1950, one died of osteonecrosis and sepsis, but 22 died of cancer: three osteosarcomas, and two carcinomas of the paranasal sinuses. In two cases, no primary site is recorded and the other fifteen deaths were from cancers arising in eleven distinct primary sites.

Twenty-eight cancer deaths occurred during the decade ending in 1960: three were primary in the paranasal sinuses, three osteosarcomas, two of uncertain origin and the remainder from thirteen primary sites. The New Jersey Radium Research Project closed in 1967, and for the six years ending in 1966, twenty-seven cancer deaths are recorded: two osteosarcomas, two from uncertain primaries and the others from fourteen primary sites.

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[18] Martland, *op cit* (n. 4 above).

[19] Martland, *op cit* (n 2 above).

*Litigation*

In 1926, suits involving the first two known fatal cases were settled out of court, without assumption of liability by the company, for approximately what their estates would have received under the Workmens' Compensation Act had the deaths been covered, but this Act then included only specific, named occupational diseases and radium intoxication was not among them. The general rule governing industrial accidents was that " . . . where no specific time or occasion can be fixed upon as the time the alleged accident happened, there is no injury by accident within the meaning of the act."[20] This was reaffirmed by the decision when the company sued its insurance carriers for reimbursement of the claims it had settled, which held, "Poisoning resulting from industrial use of radium is not an accidental injury within the coverage of the particular employer's liability insurance policy."[21] The judge ruled that "radium poisoning is an occupational disease and not an injury by accident," and although radium and mesothorium intoxication were made compensable in 1926,[22] a law may not constitutionally be made retroactive, and the disability had to commence within five months of termination of exposure. Adverse publicity for the company was enormous and worldwide, fair trials were impossible and the statute of limitations held off bankruptcy only for a while.

The company's insurance carriers paid maximum benefits within the maximum limits of coverage, but cases went to common law demanding damages. In May 1927, five workers joined in a suit before the Supreme Court of New Jersey asking $1,250,000 because they had been "given a year to live." This attracted much attention and came to be known as "The Case of the Five Women Doomed to Die." The defense pleaded both that the company had not been negligent and that the statute of limitations (at that time, in New Jersey, two years) barred recovery, so in July 1927 the suit was transferred to the Court of Chancery as a case in equity. However, Federal Judge Clark, acting as a private citizen and for humanitarian reasons, urged the disputants to compromise the suit so that the plaintiffs could obtain some relief while they were still alive, even though Vice-Chancellor Backes, sitting in Newark, had ruled that the statute of limitations would not debar the damage action as a case in equity.

A preliminary hearing took place on January 12, 1928, but after three days adjourned to April 25, 1928, when again after three days it adjourned to

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[20] Liondale Bleach Works vs Riker, *N J L* 426

[21] U S Radium Corp vs Globe Indemnity Co , 13 *N.J Misc* 316 178A 271, affirmed in 116 N J.L. 90, 182A 626 (1935).

[22] P L 1926, Chap 31, p 62

September 24, 1928. The Chancery Court returned the case to the Supreme Court for trial, but before it was heard, it was settled out of court in June 1928 through Judge Clark's intervention. Each plaintiff got a cash settlement of $10,000 and a yearly pension of $600 to continue as long as a panel of experts (Drs. Lloyd F. Craver, Edward B. Krumbhaar and James Ewing) found that they suffered from the effects of radium intoxication. The company agreed to pay for future medical care; each got $7500 for her past medical treatment; and legal fees were paid. Publicity then went wild, and many additional suits were filed against the company—at least six reached the docket between 1930 and 1933. Substantial settlements were made to many of the affected women who, feeling that they had not long to live, were remarkably prodigal in their expenditures, again fell upon hard times and again became objects of sensational journalism.[23] (The five women died in December 1929, February and October 1933, March 1938, and November 1946. The first and last of the five were sisters, and a third sister had been the first recorded victim in September 1922.)

Generally, earlier cases against the company claimed that the work was extremely hazardous and that improper precautions had been taken for the workers' protection. Later cases added that the company had been negligent in failing to search out and to treat former employees. The statute of limitations and the company's bankruptcy precluded compensation for many cases, and the law was reaffirmed on appeal from a decision in a suit brought by one of the dial painters:

> Only fraudulent concealment will stop the running of the time permitted for filing of suit by the Statute of Limitations; in absence of any such fraud, a claim for injuries arising from tipping brushes with tongue to paint radium dials is barred by failure to bring suit within two years (the statutory period) of the causal episode even though plaintiff was ignorant of the harmful potentialities of ingesting radium.[24]

Neither the company, nor the workers, nor the medical community knew how dangerous radium and mesothorium were, nor did they dream that people would die of their effects half-a-century after a few months of exposure.

## *Conclusion*

Occupationally absorbed radium and mesothorium produced unfamiliar patterns of disease during the third and fourth decades of this century.

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[23] For a detailed medico-social account of one case, see W D Sharpe, "Radium osteitis with osteogenic sarcoma," *Bull New York Acad Med*, 1971, *47* 1059-82

[24] LaPorte vs U S Radium Corp, 13 *F Supp* 263 (1936).

Although aplastic anemia, osteonecrosis and osteogenic sarcoma were known long before the isolation of radium, the recognition of radium intoxication as a cause in these patients depended upon association of clinical findings with the presence of these new and, in those days, unique substances which could then be identified and measured. The result was the delineation of a new disease and the definition of new safety precautions—too late, to be sure, to help early cases but soon enough to forestall repetition of the disaster—with radium. Probably this forewarning that very minute quantities of radioactive materials deposited in the human body can be disastrous to the individual helped to avert a much more overwhelming series of radiation accidents during later work with nuclear devices, reactors and sources.

In the New Jersey episodes, Federal, State, County and City Health Departments did what they were established to do, and could not assume responsibilities which had not been assigned by law. Universities and medical schools were of no particular help even at a time when more interested in human disease than now seems to be the case. The Medical Examiner's and Coroner's Offices, in most parts of the United States, were then still in the eighteenth, if not the seventeenth, century; but Essex County, New Jersey, was lucky enough to have a good Medical Examiner, Dr. Harrison S. Martland, who took jurisdiction as soon as the radium-related deaths seemed suspicious. The *second* death reported as radium-related was ruled to be a Medical Examiner's case, and all of those subsequently reported were investigated and examined.

It might now take longer to identify such an industrial mishap than during the twenties. The neighborhood was small and homogenous, people knew one another, and many of the radium workers walked to work. Their physicians knew each other and talked among themselves. They knew the medical examiner well; indeed, Dr. Martland had a busy consulting practice as a syphiliologist. But who would now first suspect an industrial intoxication, with our fragmentation and professional isolation? And which of us can be sure either that we are not now incubating a similar horror story, or that we could identify it as promptly as did the general practitioners and County Medical Examiner fifty years ago?

# CHANGING PRACTICES AND DISEASE PATTERNS AT THE UNIVERSITY OF VIRGINIA HOSPITAL SINCE 1907

GEORGE MOORE AND WILHELM MOLL

An article published in 1975 on the early years of the University of Virginia Hospital presents an attractive opportunity to study historical trends in hospital treated diseases and conditions since 1907.[1] Most diseases and disorders are not reportable by law. Death certificates describe terminal conditions which do not necessarily relate to incidence of disease. Hospital admissions, however, are of interest in that both serious and familiar disorders can be identified. Thus, a comparison of hospital admissions over a period of 60-65 years should be of value in spite of frequent changes in disease classification and coding, advances in diagnostic methodology, changes in medical practice resulting in new patterns of referrals and a changing perception by the public of hospital admissions.

The University of Virginia Hospital was built in 1901 with a bed capacity of twenty-five. By 1907 it had been enlarged twice and could accommodate one hundred patients. The statistical data of the period 1907-1911 provide a unique insight into the social history of hospital care in a small, southern rural community.

Between September 1, 1907, and August 31, 1911, 5,083 patients were admitted to the University of Virginia Hospital. Outcome was known on 4,555 of these. For comparison, the Medical Records Department of the University of Virginia Hospital provided information on hospital discharges from February 1, 1973, through December 31, 1973.[2] The number of hospital discharges was 18,012.

## *Geographic Area and Demographic Factors*

The University of Virginia Hospital serves patients today from the approximate geographic area that it served at the turn of the century. In 1910, 96.5% of patients were resident Virginians and in 1973, 92.0% were residents. The percentage of patients admitted from Charlottesville and Albemarle County

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[1] Wilhelm Moll and Todd L Savitt, "The early years of the University of Virginia Hospital, *Virginia Med Monthly*, 1975, *102* 542-545, 715-724

[2] Data compiled by Department of Medical Records, University of Virginia Hospital (courtesy of Doris D Harlan, Director)

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has remained at a consistent level of 30%. This is of note since the population of the city-county in 1910 was 36,636 and in 1973 the population had grown to almost 84,000. For the eleven rural counties surrounding the University Hospital, the percentage of people served was also the same in 1973 as it was in 1910. Admissions from Augusta and Roanoke Counties showed increases of patients in 1973, probably as a result of extension of University of Virginia teaching programs in those areas. The consistency of geographic source of patients over 65 years of time is remarkable when one considers the transportation difficulties of 1910, the rapid growth of urban populations, and the increase of hospital beds in the Charlottesville area. Three institutions now provide 990 beds. The University Hospital alone has grown from 100 to 550 beds.

The city-county population has also undergone changes in racial composition and life expectancy. In 1910, 33.3% of the Charlottesville-Albemarle population was nonwhite. This percentage had decreased to 14.6% by 1970. From state statistics, the estimated average length of life in 1910 was 50 years, and in 1971 the average life span had increased to 71 years. In 1900, there was a two- or three-year difference in life expectancy between men and women; in 1973, the difference had widened to eight years. There has been little change in hospital admission by sex. In 1910, 49.2% of patients were female, and in 1973 this had increased to only 52.5% in spite of the eight-year difference of life expectancy of females. In 1910, females comprised 49.7% of the population and 51.3% in 1973.

### *Length of Hospitalization, Hospital Deaths, and Ratio of "Public" vs. "Private" Patients*

The average hospital length of stay decreased from 21.2 days in 1910 to 9.3 days in 1973 (exclusive of nurseries). Hospital deaths decreased from 6% of admissions in 1910 to 3.1% in 1973. Whereas 80% of the hospital admissions in 1910 were public or staff patients (generally the poor without health insurance) and 20% were private, the percentage of private patients increased to 48.8% in 1973. Public or staff patients had declined to 51.2%. A staff or public patient in 1973, however, was one who had not selected an attending faculty member as his physician and who came under the care of the house staff. This group included many whose hospitalization costs were paid by Medicaid, Medicare and other private or public insurance programs. Similarly, private patients included the indigent if, for example, an attending faculty member selected was a Medicaid provider. The point is not that the University Hospital was serving fewer indigents in 1973, but that third-party payments have enabled the public to select an attending faculty member as their physician without financial embarrassment.

In 1910, the University Hospital provided services free if the patient could not pay. The Medical Center was considered a teaching hospital and indigents were expected to serve as teaching cases for students and residents in return for free care. In addition, the family physician saw most of the public and private patients in homes and offices and referred those for definitive diagnosis, specialist care and surgery to the teaching hospital. This form of practice has diminished appreciably. The incidence of physician home visits was only 2.3% in 1968-69.[3]

### *Hospital Admissions by Service*

It is apparent that the surgeon was supreme in the 1910 teaching hospital setting (see Table 1). Without chemotherapy and antibiotics, the scalpel held most hope for cure of serious disorders referred by family practitioners. The advance of laboratory methodologies in later years, the discovery of broad spectrum antibiotics, and increased longevity raised public expectations and acceptance of the hospital as a general diagnostic and treatment center for all manner of signs and symptoms, real and imagined illness, disorders and even obstetrical deliveries. The advent of public and private insurance programs accelerated the trend of changing hospital utilization, particularly since the patient himself could appear at the hospital without referral from a family practitioner.

The University Hospital suffered financially in 1910 as it does today but, in the early years, a teaching hospital was expected to provide free care based on its overriding teaching responsibilities. In 1910, no fees were collected for 41% of the patients and private patients were charged only $10-20 per week for inpatient care. Today, the cost of a semi-private room is $132 per day but the University Hospital is forced to write off almost $7 million a year on bad debts and free service to indigent patients. Paradoxically, the greatly increased effectiveness of medicine achieved by replacing the former family doctor with highly trained teams in the medical center has not produced corresponding public approval or satisfaction with the health care delivery system.[4]

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[3] National Center for Health Statistics, *Age Patterns in Medical Care, Illness, and Disability, United States, 1968-1969* (Vital and Health Statistics, ser 10, no 70, Rockville, Md Health Services and Mental Health Administration, 1972)

[4] Robert F. Rushmer, *Humanizing Health Care Alternative Futures for Medicine* (Cambridge, Mass MIT Press, 1975)

TABLE 1
University of Virginia Patients by Service

| Service | 1907-11 | 1973-74 | Percent Change |
|---|---|---|---|
| Surgical | 70% | 42%* | − 40% |
| Medical | 20% | 35%** | + 75% |
| Obstetrics and Newborn | 5% | 15.5% | +210% |
| Eye, Ear, Nose and Throat | 5% | 7.5% | + 50% |

| *INCLUDES | | **INCLUDES | |
|---|---|---|---|
| Surgery | Pediatric Surgery | Medicine | Pediatrics |
| Gynecology | Plastic Surgery | Dermatology | Psychiatry |
| Neurological Surgery | Urology | Neurology | Radiology |
| Orthopedic Surgery | | | |

*Diseases or Conditions on Admission*

Table 2 depicts the most commonly treated diseases and conditions by percentage of all cases during 1907-11 and 1973.

Advances in public health and chemotherapy have reduced hospital admissions for communicable diseases. An older population in 1973 accounts for increases in neoplasms, pneumonias, and diseases of the circulatory and musculoskeletal systems. Advanced methods of diagnosis may account for variations in such categories as appendicitis, inflammatory diseases of female genitalia, and complications of pregnancy and delivery. Mental disorders and accidental injuries show an upward trend.

It is interesting that the percentage of hospital admissions for malignant neoplasms, now a leading cause of death, did not increase markedly over the 60-year period even as life expectancy rates were rising. Cancer death rates in Virginia, however, rose from 48.4 in 1913 to 152.9 per 100,000 in 1973.

The rank of hospital admissions for carcinoma in 1973 may be compared with the five leading causes of mortality for malignancies.[5] First is lung carcinoma, followed by intestinal (colon-rectum), breast, prostate and cervical (uterine).

As noted from Table 3, no case of lung cancer was listed as a hospital diagnosis during 1907-11. This is remarkable and deserves further discussion. Death certificates were first used in Virginia about 1912 but early records have been lost. Rigdon reported that the International Lists of

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[5] Based on rates from National Cancer Institute Biometry Branch, *Third National Cancer Survey Incidence Data, 1969-71* Ed Sidney J D. Cutler and John L Young (Monograph no. 41, Bethesda, Md., National Cancer Institute, 1975)

TABLE 2
Most Commonly Treated Diseases and Conditions
University of Virginia Hospital—1907-11 and 1973

| Disease or Condition | 1907-11 Percent of All Cases | 1973 Percent of All Cases |
|---|---|---|
| I. Infective & Parasitic Diseases | 9.7 | 1 68 |
| II. Neoplasms | 9.8 | 10.78 |
| Malignant | 5.7 | 9 08 |
| Benign & Unspecified | 4.1 | 1.70 |
| III. Mental Disorders | 2.2 | 4.41 |
| IV. Diseases of Nervous System & Sense Organs | | |
| Diseases of Eye (Inflammatory & Other) | 2.5 | 2 52 |
| Diseases of Ear | 1 0 | 1.25 |
| V Diseases of Circulatory System | | |
| Rheumatic Fever & all Forms of Heart Disease | 2 2 | 6.97 |
| VI. Diseases of Respiratory System | | |
| Pneumonia | 0.9 | 1 10 |
| Tonsillitis, Hypertrophy of Tonsils & Adenoids | 1 2 | 1.10 |
| VII. Diseases of Digestive System | | |
| Appendicitis (Acute & Chronic) | 10 9 | 0 71 |
| Hernia of Abdominal Cavity | 3.4 | 1.17 |
| Cholelithiasis & Cholecystitis | 2.6 | 0 72 |
| Hemorrhoids | 1.0 | 0 09 |
| VIII. Diseases of the Genitourinary System | | |
| Inflammatory Diseases of Female Genitals | 4 9 | 0.31 |
| Cystitis | 1.2 | 0 08 |
| Disorders of Menstruation | 1.1 | 0 52 |
| Prostatitis & Prostatic Hypertrophy | 1 1 | 0 68 |
| IX. Complications of Pregnancy, Child Birth & the Puerperium | | |
| Complications of Pregnancy & Delivery | 1 2 | 2.23 |
| Uterine Prolapse & Malposition | 3.6 | 0 08 |
| X. Diseases of the Skin & Subcutaneous Tissue | | |
| Skin Infections | 1 6 | 0 82 |
| XI. Diseases of the Musculoskeletal System & Connective Tissue | | |
| Osteomyelitis & Other Diseases of Bone & Joint | 1.6 | 2.94 |
| Arthritis and Rheumatism | 1.0 | 1 35 |
| XII. Accidents, Poisoning & Violent Injuries | | |
| Fractures, Dislocations and Sprains | 3.2 | 2.42 |
| Other Accidents and Injuries | 3.1 | 6.88 |

TABLE 3
Leading Malignant Neoplasms Seen at
University of Virginia Hospital

| 1907-11 (277 inpatients) | | 1973 (1,203 inpatients and outpatients) | |
|---|---|---|---|
| Rank | Percentage of Total | Rank | Percentage of Total |
| 1. Breast | 13.0 | 1. Lung, Bronchi, Trachea & Pleura | 13.6 |
| 2. Cervical | 11.2 | 2. Skin | 9.8 |
| 3. Gastric | 10.5 | 3. Breast | 7.3 |
| 4. Unknown Primary Site | 8.3 | 4. Cervical | 6.3 |
| 5. Skin | 6.9 | 5. Unknown Primary Site | 6.2 |
| 6. Intestinal | 5.4 | 6. Prostatic | 4.7 |
| 7. Prostatic | 5.1 | 7. Leukemia | 4.4 |
| 8. Bone | 5.1 | 8. Intestinal | 3.8 |
| 9. Leukemia | 1.8 | 9. Gastric | 1.3 |
| 10. Lung, Bronchi, Trachea & Pleura | 0.0 | 10. Bone | 0.6 |

Causes of Death did not separately list cancer of the lung or pleura, or even cancer of the respiratory system as a whole, until the Fourth Revision which was released in 1930.[6] Prior to 1930, the term "bronchogenic carcinoma" was lumped with 90 other cancers of various sites. Lung cancer was first recognized in 1810 and, by 1910, some 100 cases could be found in the literature among both males and females. The disorder was difficult to diagnose clinically. Rigdon found that by 1900 the percentage of correct diagnoses was only 5%.[7] By 1930, the percentage of correct diagnoses had reached 50%. An attempt was made in 1914 to establish a death rate for lung cancer.[8] This was estimated at 0.6 per 100,000 population and in 1924 the rate had increased to 1.6. This was a significant increase in 10 years. Adler reported in 1912 that "even the diagnosis made on the autopsy table was not always reliable."[9] Thus it would appear that the absence of lung carcinoma as a hospital diagnosis in Table 3 can be explained both on the paucity of the disorder as well as the difficulty in establishing a correct diagnosis. It is

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[6] R. H. Rigdon and H. Kirchoff, "Cancer of the lung from 1900 to 1930," *Internat Abstracts Surg.*, 1958, *107* 105-108

[7] *Ibid*

[8] *Ibid*

[9] I. Adler, *Primary Malignant Growths of the Lungs and Bronchi A Pathological and Clinical Study* (London, Bombay and Calcutta Longmans Green, 1912)

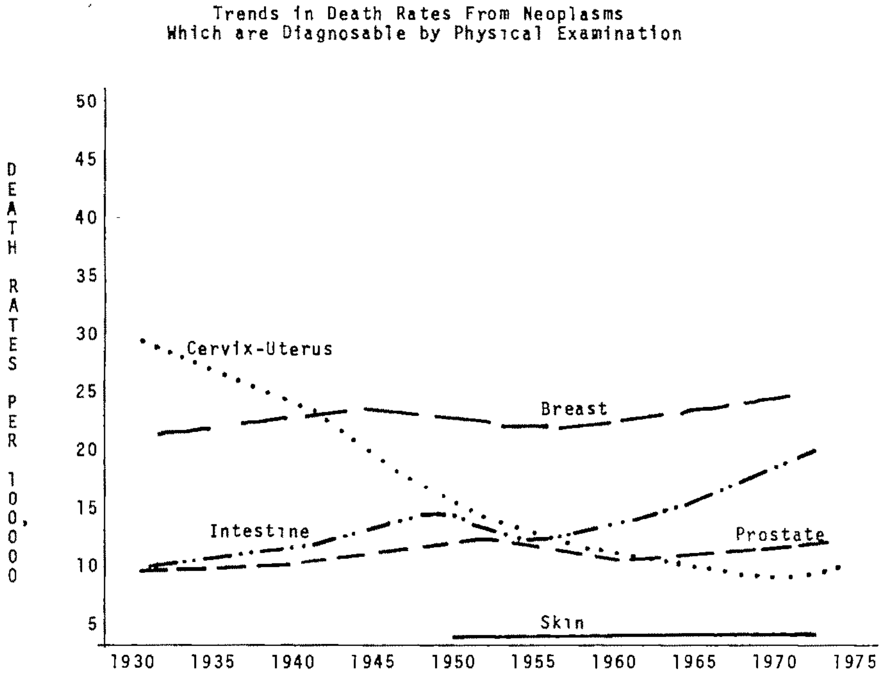

Figure 1 Trends in Death Rates From Neoplasms Which are Diagnosable by Physical Examination

obvious that the more recent increase in lung carcinoma is the result of etiological factors rather than improved diagnostic methodology.

The percentage of unknown primary site malignancies over the past 60 years as shown in Table 3 has remained surprisingly constant. It would appear that diagnostic techniques still are imperfect and require more research. Carcinoma of the breast, cervix, skin, rectum and prostate continue to be diagnosable by gross physical examination. A comparison of hospital admissions for these malignancies over 60 years indicates that these disorders are still prevalent. The breast cancer death rate seems to have stabilized for at least 50 years.[10] Skin, prostate and colon-rectum death rates have also shown little change (Figure 1). Apparent progress for the control of these malignancies has not occurred despite rather easily made diagnoses and improved methods of treatment. Uterine death rates have decreased strikingly over the past forty years mostly as a result of the Pap Test, first

[10] *Third National Cancer Survey* (n. 5 above)

introduced in 1943. The steady decrease in stomach and bone carcinoma over 60 years deserves epidemiologic study.

## *Conclusions*

Hospital admission data gleaned from the first patient record book of the University of Virginia Hospital for the period 1907-1911 were compared with those of 1973. The findings present a remarkable picture of changing hospital practices and disease patterns seen at a University teaching hospital in a small, southern community.

Among the many noteworthy findings is the fact that the hospital has continued to serve patients from approximately the same geographic area for over sixty years; that length of hospitalization stays decreased by almost 44%; and, that the hospital death rate has been reduced by half. During this period the number of patients seen by the surgical services declined from 70% to 42%. An analysis of the most commonly treated diseases and conditions follows generally established patterns. That the 1907-1911 data did not register a single admission for lung neoplasm, which in 1973 headed the list of neoplasms seen at the Hospital, is a salient observation.

A comparison of the teaching hospital of 1910 with its modern-day resemblance indicates that many changes have taken place, affecting providers and public alike. Hospital admission records thus deserve study and periodic review by epidemiologists and others concerned with solution of health care problems. Observations on admissions for malignancies, for example, suggest that an historical review of morbidity data may be more significant than a study of mortality rates for one community of interest.

# AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE, INC.

## BYLAWS*

### ARTICLE I: NAME AND PURPOSE

*Section 1.* The name of this corporation shall be American Association for the History of Medicine, Inc.

*Section 2* The purposes of the Association shall be to promote, stimulate, and encourage research, study, interest, and writing in the history of medicine, including the history of public health, dentistry, pharmacy, nursing, and allied arts, sciences, and professions, to lend its support to and to cooperate with local, national, or international organizations having similar purposes; to organize and arrange meetings, forums and discussion group lectures, present awards, and issue publications; and for such other purposes as are set forth in its Certificate of Incorporation.

*Section 3:* The Association shall conduct its activities solely for educational, scientific, and literary purposes, and not for pecuniary profit.

### ARTICLE II: MEMBERSHIP

*Section 1:* There shall be five classes of members: Active Members, Nonresident Members, Honorary Members, Emeritus Members, and Constituent Societies Active Members may be designated as Contributing, Sustaining, or Life Members by the payment of additional dues, as established in accordance with Article VI, in all other respects the rights and duties of Contributing, Sustaining, and Life Members shall be identical with those of other Active Members.

*Section 2.* Any resident of the United States of America, its territories and possessions, or Canada who is interested in the history of medicine shall be eligible for Active Membership upon submitting an application to the Secretary and acceptance thereof by one or more of the officers.

*Section 3.* Any person interested in the history of medicine who is not a resident of the United States of America, its territories and possessions, or Canada shall be eligible for Nonresident Membership upon submitting an application to the Secretary and acceptance thereof by one or more of the officers. Nonresident Members shall not be entitled to vote at the annual or other meetings.

*Section 4·* Honorary Membership may be conferred upon any person who has made outstanding contributions to the history of medicine or science upon the nomination of the Council and election by the Active Members at the annual meeting, provided that names of proposed candidates for Honorary Membership shall be submitted in writing to the members of the Council at least thirty days before the

* Adopted May 24, 1958, and incorporating revisions made up to May 12, 1978.

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annual meeting. Honorary Members shall have all the privileges of Active Members if they reside within the United States of America, its territories and possession, or Canada, and of Nonresident Members if they do not.

*Section 5.* Anyone who has been a member for 20 years and has reached the age of 70 may on request become an Emeritus Member and be excused from the payment of dues.

*Section 6:* Any organization in the United States of America, its territories and possessions, or Canada, such as a society, club, institute, or library, having purposes similar to those of the Association, shall be eligible for membership as a Constituent Society upon nomination in writing by an Active Member and election by the Council. Each Constituent Society may choose one delegate to attend the annual meeting of the members, who, on that occasion, shall have the same voting rights as Active Members. Each Constituent Society shall submit a brief written report on its activities to the Secretary at the time of the annual meeting of the Association.

*Section 7·* Membership shall not become effective until the Treasurer has received the first year's dues.

*Section 8* If any person or organization being a member or Constituent Society of the Association is believed to be guilty of an act prejudicial to the Association or its purposes, such person or organization shall be notified to appear personally or in the case of a Constituent Society through an officer or other representative for a hearing before the Council at a designated time and place not less than thirty days after such notification. The membership of such person or organization may be terminated by a two-thirds vote of the members of the Council present at the hearing and voting.

## ARTICLE III: OFFICERS

*Section 1.* The officers of the Association shall be a President, Vice-President, Secretary, and Treasurer, all of whom shall be Active Members of the Association. All officers shall be elected by the Active Members at an annual meeting for two-year terms by plurality of members voting, in person or by proxy, the President and Vice-President in even years, and the Secretary and Treasurer in odd years. Their terms of office shall begin at the close of the meeting at which they are elected and terminate at the close of the meeting at which their successors are elected. One person may hold the offices of both Secretary and Treasurer.

*Section 2:* The officers shall have the powers and duties customarily incident to their respective offices in similar organizations and also such as shall specifically be delegated to them by the Council.

*Section 3·* Honorary Officers may be nominated and elected from among Active Members by the same procedures that is provided for the nomination and election of Honorary Members.

## ARTICLE IV: COUNCIL

*Section 1·* Except as otherwise required by law or provided in these Bylaws, management of the affairs of the Association shall be vested in a Board of Directors, which shall be known as the Council.

*Section 2:* The Council shall consist of officers of the Association, the immediate past President, and twelve Active Members elected for three-year terms at an annual meeting by plurality of Active Members voting. The President shall act as Chairman of the Council. Six shall constitute a quorum of the Council.

*Section 3:* The members of the Council other than the officers and immediate past President shall be divided into three classes of four members each. At each annual meeting of the Association there shall be an election of four members of the Council other than the officers and immediate past President to hold office for three years or until the third annual meeting after their election. At the first annual meeting of the Association at which these Bylaws are adopted there shall be elected four members of the Council other than the officers and the immediate past President to hold office for one year, four to hold office for two years and four to hold office for three years.

*Section 4:* In the election of members of the Council, due regard shall be had to representative geographical distribution, provided that so long as the Association is incorporated in the State of New York, at least one member of the Council shall be a legal resident of that State.

*Section 5:* The Council shall hold an annual meeting not more than one week preceding the annual meeting of the Association, at a time and place designated by the President. Special meetings may be called at any time by the President or by any five members of the Council upon at least two weeks' written notice to each member of the Council.

*Section 6:* The Council shall provide for filling the unexpired term of any vacancy that may occur in any office or in the Council.

*Section 7.* Except as otherwise provided by law and these Bylaws, decisions of the Council shall be by majority vote of those members of the Council present and voting.

## ARTICLE V: MEETINGS

*Section 1:* The annual meeting of the Association at which officers and members of the Council shall be elected shall be held at a time and place designated by the Council. The President shall arrange the order of business. Twenty members shall constitute a quorum.

*Section 2.* The Secretary shall mail notice of the annual meeting of the Association to each member entitled to vote not less than thirty nor more than forty days before the meeting. Such notification shall state the time, place, and general purposes of the meeting, together with the nominations made in accordance with Article XI, Section 2, and any amendments proposed in accordance with Article XII, Section 1, of these Bylaws.

*Section 3:* The Council of the Association shall present at the annual meeting a report dated as of the close of the last complete fiscal year, verified by the President and Treasurer, or by a majority of the members of the Council, showing the whole amount of real and personal property owned by it, where located, and where and how invested, the amount and nature of the property acquired during the year immediately preceding the date of the report and the manner of acquisition; the amount applied, appropriated or expended during the year immediately preceding such date, and the purposes, objects or persons to or for which such applications, appropriations or expenditures have been made. This report shall also show the names and addresses of the persons and organizations who have been admitted to membership during such year, and the number of members in each class of membership at the beginning and end of such period. This report shall be filed with the records of the Association and an abstract thereof entered in the minutes of the proceedings of the annual meeting at which the report is presented. The President shall appoint two Active Members of the Association, neither of whom shall be members of the council, to act as auditors, who shall examine the annual report referred to in this section prior to the annual meeting and express their opinion thereon at the annual meeting.

## ARTICLE VI: DUES AND FINANCES

*Section 1*· The amount of the annual dues for all classes of membership shall be determined by vote of the Active Members, except that Honorary Members shall pay no dues.

*Section 2* All dues shall be payable annually by January first. If the dues of any member remain unpaid on March first, the Treasurer shall mail a notice of delinquency. If the dues of any member are not paid within sixty days after the mailing of such notice, his membership shall automatically terminate, unless the Council shall, in individual cases, waive this provision.

*Section 3*· Members who have been dropped for nonpayment of dues may be reinstated at any time before the close of the year in which they were dropped upon payment of the dues for that year.

*Section 4:* The fiscal year of the Association shall begin on November 1 and end on the following October 31.

## ARTICLE VII: PUBLICATIONS

*Section 1:* The Association may issue its transactions and other publications either independently or with an established journal within the terms of an agreement made between the Association and the proprietor of this journal.

*Section 2:* All members, except Emeritus Members, shall receive regular subscriptions to the journal or other publication that prints the official transactions of the Association.

## ARTICLE VIII: AWARDS

*Section 1.* The Association may invite any person distinguished for contributions to medical history or other fields of science and learning to present the Fielding H. Garrison Lecture at the annual meeting. The lecture should be the result of original research and must not have been previously published. The lecturer shall be selected by the Garrison Lecture Committee.

*Section 2:* The Association may award the William H. Welch Medal for particular contributions of outstanding scholarly merit in the field of medical history published during the five years preceding the award. The medal shall not be awarded more often than once each year. Final decision on the award of the medal shall be made by the Council upon the recommendation of the Welch Medal Committee.

*Section 3.* The Association may award the William Osler Medal to students of medicine in the medical schools of the United States and Canada for unpublished essays that either are the result of original research or show an unusual appreciation and understanding of historical problems. The medal shall not be awarded more often than once each year. Final decision on the award of the medal shall be made by the Council upon the recommendation of the Osler Medal Committee.

## ARTICLE IX: RELATIONS WITH THE INTERNATIONAL SOCIETY

*Section 1.* The Association will maintain close liaison and cooperate with the International Society of the History of Medicine as the United States of America section thereof.

*Section 2.* Members of the Association who wish to join the International Society should apply to the President of the Association who, with the approval of the Council, will act upon recommending them for election to membership by the Administrative Council of the International Society. Applicants should include information about their medico-historical activities and publications which will be transmitted to the Secretary General of the International Society.

*Section 3:* In every odd year at the time of the annual meeting of the Council the President shall appoint, subject to confirmation by the Council, the national delegate and an alternate national delegate for the United States to the International Society, to serve for the ensuing two years or until their successors are qualified. The delegate and the alternate delegate must be citizens of the United States, Active Members of the Association, and also members of the International Society. The President shall forward their names to the Secretary General of the International Society.

*Section 4:* In conformity with the regulations of the International Society, the national delegate shall be a member of its Permanent Committee. He shall keep the President and the Secretary General of the International Society informed about medico-historical activities in the United States. He shall attend the meetings of the Permanent Committee and represent the Association at International Congresses. He shall function as the Intermediary between the International Society and its members in the United States.

*Section 5·* In case both the delegate and the alternate delegate are unable to attend the annual meeting of the Permanent Committee and the International Congress, the President may appoint a delegate for the occasion.

*Section 6·* The Secretary-Treasurer of the Association shall collect the annual dues for Association members of the International Society and transmit them to the Treasurer of the International Society.

## ARTICLE X: COMMITTEES

*Section 1:* The President shall appoint annually a Membership Committee consisting of three members of the Council.

*Section 2:* The President shall appoint annually a Garrison Lecture Committee to select the Fielding H. Garrison Lecturer.

*Section 3.* The President shall appoint annually an Osler Medal Committee to make recommendations to the Council on the award of the William Osler Medal.

*Section 4.* The President shall appoint annually a Welch Medal Committee to make recommendations to the Council on the award of the William H. Welch Medal.

*Section 5:* The President and the Vice-President in consultation with the Secretary shall appoint one or more committees to arrange the program and other activities of those annual meetings for which the Council has designated a place and year.

*Section 6:* In every odd year at the time of the annual meeting of the Council the President shall appoint, subject to confirmation by the Council, an Editorial Committee consisting of three Active Members who shall continue in office for the ensuing two years. The Editorial Committee shall perform such work and undertake such negotiations with respect to editorial matters as may be required subject to any agreements or contracts made by the Association regarding its publications.

*Section 7·* The President shall appoint, subject to confirmation by the Council, a Committee on Education consisting of three members, each continuing in office for three years. The Committee on Education shall study all aspects of teaching and education that bear significant relationship to the teaching of medical history and shall advise the President and Council on the development of projects and policies about education in medical history.

The Chairman of the Committee shall submit, in writing, an annual report to the President and Council at least 30 days before the annual meeting. At each annual meeting of the Council, the President shall submit, for its approval, the name of a replacement for the member retiring from the Committee and a designate for Chairman for the coming year. After an individual has completed a designated term, this person shall not be reappointed to the Committee until a period of three years has elapsed.

*Section 8* The President shall appoint such other committees as he deems necessary to carry on the work of the Association.

## ARTICLE XI· NOMINATIONS AND ELECTIONS

*Section 1* In every odd year at the time of the annual meeting of the Council the President shall appoint subject to confirmation by the Council a Nominating Committee consisting of three Active Members, none of whom shall be members of the Council, who shall continue in office for two years or until their successors are appointed.

*Section 2.* The Nominating Committee shall present a list of nominations to the Secretary for the elections to be held at any annual meeting not less than two months before such meeting, and the Secretary shall mail the list to all members of the Association not less than thirty nor more than forty days before the annual meeting. Additional nominations may be made in writing to the Secretary by any five members at any time previous to the voting.

*Section 3.* The voting in all elections for officers and members of the Council shall be held at the annual meeting called for that purpose and shall be by written ballot, unless in any particular case only one person has been nominated for a particular position.

### ARTICLE XII: AMENDMENTS

*Section 1.* Any five Active Members may propose changes in the Bylaws by presenting their proposals to the Secretary in writing not less than two months prior to any annual meeting. The Secretary shall send them to the Active Members not less than thirty nor more than forty days before the meeting The proposed changes shall then be considered at the meeting and shall become effective by a two-thirds vote of the Active Members voting.

*Section 2* Upon the recommendation of the Council the Bylaws may be changed without prior notice by a two-thirds vote of the Active Members voting at any annual meeting.

### ARTICLE XIII. PARLIAMENTARY AUTHORITY

*Section 1.* The rules of procedure contained in *Robert's Rules of Order, Revised* shall govern meetings of the members of the Association so far as they are applicable and when not inconsistent with these Bylaws.

### ARTICLE XIV: SEAL

*Section 1·* The seal of the Association shall be that adopted by the Council and shall be identical to that adopted for its seal by the American Association of the History of Medicine in 1935, except that the words around the outside shall be AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE, INC. 1958 and there shall be added to the central design, below the pedestal, the words FOUNDED 1925.

# *MEDICO-HISTORICAL NEWS AND ACTIVITIES*

## CORRESPONDENCE AND REPORTS

### THE MEDICAL HISTORY CONGRESS IN PLOVDIV, BULGARIA

From August 20 to 25, 1978, the 26th International Congress of the History of Medicine took place in Plovdiv, the second largest city of Bulgaria and capital of ancient Thrace which flourished under the Romans as Trimontium, named for the three hills which dominate the city. Both Plovdiv and Sofia, the capital, ingeniously incorporate Roman ruins in their cityscapes. For example, in Plovdiv a Roman amphitheater, just below the level of a street intersection, is used for concerts and other gatherings, while in Sofia one walks on a Roman road in the underpass of a busy crossing.

The twelve Americans who attended the Congress represented the largest group from the Western world and were cordially received by our Bulgarian hosts, who worked tirelessly to assure a pleasant visit. We were touched by the kindness and generosity of the Bulgarian people, not only at the Congress but later on trips throughout the country.

The scientific sessions were held in the Trade Union House of Culture, "Stefan Kiradjev," which was pleasantly situated near a beautiful park and the National Library, "Ivan Vazov," where an interesting medico-historical book exhibition was held. In the foyer of the Trade Union House a photographic exhibition on "The Development of Medicine and Public Health Care in Bulgaria" was to be seen. The busy scientific program was interspersed with pleasant social events, such as the performance of the State Folklore Ensemble, "The Rhodopes are Singing," where colorful singing and dancing accompanied by ancient instruments illustrated the rich Bulgarian folk tradition. Another memorable evening was spent in the garden in front of the Ethnographic Museum in Old Plovdiv, listening to the excellent Valtchev Quartet from Sofia. Excursions to the Recreation Park and the Health Zone, where facilities for orthopaedic rehabilitation are adjacent to athletic training areas and the regional stadium, and other excursions to the mountains and interesting old towns in the vicinity gave congressists an opportunity to know Bulgaria. At Hissar we saw not only modern sanatoria where the local mineral waters are administered, but also the ancient Roman baths and town walls dating from the time of Diocletian.

It was not unexpected to experience intense nationalism and politicization of the official plenary sessions of the Congress, which was attended by a large number of participants from Communist countries. At the end of the first day we were taken to the Common Grave in Plovdiv, a well-designed modern monument to those who died to achieve the People's Republic of Bulgaria, and to visit the Hill of the Liberators which is crowned by a huge sculpture of a Soviet soldier. In both instances the delegates were given red carnations by children to lay on the monuments. The helpful official Bulletins reporting the daily events of the Congress were charac-

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terized by the somewhat euphoric spirit typical of official reporting in the Communist world. Politics aside, on a person-to-person level, the Bulgarian people whom we met won our respect and friendship, and we are grateful to them for inviting us to their country.

The scientific program suffered from the International Society's perennial problem of a lack of quality standards as well as a plethora of announced papers which never materialized, so that every session was completely unpredictable. This criticism is not directed to our Bulgarian hosts who only followed precedent in the Society. Conspicuous in their absence were most of the world's major medical historians, whose participation could be assured by more meaningful planning by the Society on an international level.

Details of the official meeting of the International Society were given by Dora B. Weiner in this *Bulletin*, Fall 1978, p. 460.

GENEVIEVE MILLER, Ph.D.
*U.S. Delegate*

## INTERNATIONAL COLLOQUIUM

From September 20 to 22, 1978, scholars from nine countries met in West Berlin to discuss the theme "Men and Health in History." The colloquium was organized by Professor Arthur E. Imhof of the Friedrich-Meinecke-Institut of the Free University of Berlin where the three full-day meetings took place.

In his introductory remarks, Imhof, a leading historical demographer, called for an interdisciplinary approach to a problem which cuts across the spectrum of biological and social sciences. He noted the pioneering work of the *Annales* school in France which continues to give biology and medicine a conspicuous place in its project for an *histoire totale*, and he drew attention to the outstanding demographic studies from Scandinavian sources which provide the earliest vital statistics on a national level. Both traditions were well represented at the Colloquium.

The professional background of the participants aided the interdisciplinary goal. Physicians, demographers, historians, an ethnologist, and a theologian presented papers. It is not possible here to summarize the contents of the papers, virtually all of which were substantive contributions based on unpublished sources (see program below). About one-third concerned historical demography; divergent interpretations of the significance of smallpox mortality (and its prevention) for population growth were put forward. Several papers dealt with medicalization and its impact on public health, politics, and culture. Sources tapped included municipal, parish, and national vital statistics, hospital archives, medical correspondence and literature, and folklore.

Given the diversity of place—France, Germany, Norway, Finland, Sweden, Geneva, Brussels, Goteburg, Paris—and time (17th to 20th centuries), it is not surprising that no *vue d'ensemble* emerged. Despite the uniform biological and epidemiological substrate, which some speakers emphasized, the diversity of historical responses proved more impressive. However, shared assumptions and methods, especially the use of quantification where possible, made for fruitful comparisons and discussion. Most participants agreed that considerable progress is being made toward a social history of disease, and more can be anticipated. The cultural history of health, from the viewpoint of patients as opposed to professionals, seems to be a more elusive problem. Here one must continue to search for new sources and, more important, to devise methods for integrating qualitative materials into the quantitative framework in a way which displays collective mentalities.

*Program*

*1st session.* Rolf Winau (Berlin) presiding.

Jean-Noël Biraben (Paris): "Histoire de la classification de décès et de maladies aux 18e et 19e siècles."

Vincent B. Comiti (Paris): "Importance des connaissances médicobiologiques actuelles pour une étude critique de pathocenose historique."

Øivind Larsen (Oslo): "Krankheitsauffassungen in den norwegischen Medizinalberichten des 19. Jahrhunderts."

*2nd session.* Øivind Larsen (Oslo) presiding.

Jean-Pierre Goubert (Paris): "L'hygiène de l'eau en France au XIXe siecle. Aperçus et hypothèses."

Robert William Lee (Liverpool): "Medicalisation and mortality trends in South Germany in the 19th century."

Heinrich Pompey (Wurzburg). "Pastoralmedizin—der Beitrag der Seelsorge zur psycho-physischen Gesundheit."

*3rd session.* Jean-Pierre Goubert (Paris) presiding

Oiva Turpeinen (Helsinki): "Mortality from smallpox, measles and whooping-cough in Finland 1751-1865."

Bengt I. Lindskog (Lund): "Mortalitatsanalyse einer sudschwedischen Bevolkerung 1749-1820."

Alfred Perrenoud (Genève): "Deux cents ans de variole à Genève (XVIIe-XVIIIe siècle)"

*4th session.* Jean-Noel Biraben (Paris) presiding.

Alain Bideau (Lyon-Paris). "Mortalité et morbidité infantiles à Thoissey en Dombes à la fin du 18e siècle. Caractéristiques familiales et cycles épidémiques."

Reinhard Spree (Berlin): "Die Entwicklung der differentiellen Sauglingssterblichkeit in Deutschland seit der Mitte des 19 Jahrhunderts."

Jacques Gélis (Paris): "Regard sur l'Europe médicale des Lumières: la collaboration internationale des accoucheurs et la formation des sagesfemmes au XVIIIe siècle."

Marie-France Morel (Paris): "Mère, enfant, médecin: la médicalisation de la petite enfance en France (XVIIIe-XIXe siècles)."

Françoise Loux (Paris): "Recours convergents à la médecine officielle et à la médecine parallèle en matière des soins aux enfants en France, 19e-20e siècles."

*5th session.* Robert William Lee (Liverpool) presiding.

Inger Wikstrom-Haugen (Goteborg): "Patienten im Sahlgren'schen Krankenhaus Goteborg 1782-1822."

Toby Gelfand (Ottawa): "Les caractères originaux d'un hospice parisien à la fin de l'Ancien Régime."

*Closing discussion.* Arthur E. Imhof (Berlin) presiding.

The proceedings of the colloquium are scheduled to be published in the near future. Financial support was provided by the Volkswagen Foundation.

Toby Gelfand, Ph.D.
Program in History of Medicine
Faculty of Health Sciences
University of Ottawa

## AMERICAN HISTORICAL ASSOCIATION

The history of medicine occupied a prominent place on the program of the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 28-30, 1978. Four sessions, each attracting large audiences, dealt specifically with the history of medicine and public health. On Friday morning Barbara G. Rosenkrantz (Harvard University) chaired a session on comparative medical history entitled "Filth and Politics in the Age of Sanitary Reform." The three papers presented examined the politics of public health in nineteenth-century Paris, Milwaukee, and Moscow: "Working Class Housing in Paris, 1850-1902," by Ann-Louise Shapiro (Brown University); "The Wasteland: Garbage in the Nineteenth-Century American City," by Judith Walzer Leavitt (University of Wisconsin-Madison); and "The Moscow Zemstvo and Workers' Health in the 1880s," by Nancy M. Frieden (Marymount Manhattan College). One of the most interesting points to come out of the session concerned the varying influence of physicians as public-health reformers. In Milwaukee, the medical profession acting alone proved impotent; in Moscow, a single doctor accomplished much. James H. Cassedy (National Library of Medicine) provided the commentary.

The afternoon program featured two parallel sessions on medical history. One, chaired by Alfred W. Crosby (University of Texas, Austin), dealt with "Disease Discrimination by Sex and Race: The Impact on Females and Blacks." In a paper on "Women's Diseases before 1900" Edward Shorter (University of Toronto) argued that women in the eighteenth and nineteenth centuries felt uneasy about sex largely because of "the grim physical consequences." In an equally provocative essay—"The African Connection: Slavery, Disease, and Racism"—Kenneth F. Kiple (Bowling Green State University) summarized his forthcoming book, which stresses the social consequences of blacks being medically different from whites. Ronald L. Numbers (University of Wisconsin-Madison) led the discussion.

The second afternoon session, chaired by David B. Tyack (Stanford University), looked at "Social Deviance in Historical Perspective." Richard W. Fox (Yale University) examined the reasons for California's high sterilization rate in "Detaining the 'Mentally' Deviant: California and the Insane, 1870-1930," concluding that vigorous sterilization programs in two of the six state hospitals were a contributing factor. After exploring "Psychotic Delusions as a Key to Historical Cultures: Tasmania, 1830-1940," John C. Burnham (Ohio State University) doubted that one could really say much about a culture from a study of delusional contents, because they reflect unreality rather than reality. Nathan G. Hale, Jr. (University of California, Riverside) and Gert H. Brieger (University of California, San Francisco) commented.

Saturday morning the AHA experimented with a "theme session" on the practice of comparative history, using William H. McNeill's *Plagues and Peoples* as a case study. In a format reminiscent of the 1975 AAHM session in Philadelphia, Frederick F. Cartwright (King's College Hospital Medical School, London) presided over a panel consisting of Philip D. Curtin (Johns Hopkins University), Charles E. Rosenberg (University of Pennsylvania), and David Musto (Yale University). McNeill responded to his critics and answered questions from the audience.

The AHA meetings—and similar ones sponsored by the Organization of American Historians—give evidence of the rapidly growing popularity of medical history among general historians and suggest the possibilities open to medical historians interested in reaching a nonmedical audience.

RONALD L. NUMBERS, Ph.D.
Department of the History of Medicine
University of Wisconsin—Madison

## OSLER LIBRARY

On May 29, 1979, McGill University will celebrate the 50th anniversary of its reception of the bequest of William Osler's collection of books and manuscripts in the history of medicine and of the establishment of the Osler Library. A symposium on "Books, Manuscripts, and the History of Medicine," will be held to commemorate the event and papers will be presented on Osler's bibliographic activities, 20th-century developments in physical and reference bibliography and their importance for history of medicine collections, and developments in the study of medieval and renaissance manuscripts. Participants in the program, for which there are no registration fees, include Estelle Brodman (Washington University School of Medicine, St. Louis), Richard J. Durling (Christian-Albrechts-Universitat, Kiel), Eric Freeman (Wellcome Institute, London), Charles G. Roland (McMaster University, Hamilton), and G. Thomas Tanselle, (Guggenheim Memorial Foundation, New York). For further information write to Philip M. Teigen, Osler Library, 3655 Drummond St., Montreal, Que. H3G 1Y6.

On the eve of the Osler Library's semicentennial, the dean of the medical faculty of McGill University, on behalf of the Board of Curators of the Osler Library, has announced two appointments. Dr. Edward H. Bensley, Professor Emeritus of Experimental Medicine, has been named Honorary Osler Librarian in recognition of the many services rendered to the library, and his years of competent editorship of the *Osler Library Newsletter* and contributions to it and other publications about the library. The only previous holder of this post was Dr. Wilder Penfield. Dr. Philip Teigen, head of the Osler Library for the past five years, has been designated Osler Librarian in recognition of his accomplishments in both history and librarianship. He is the second holder of this title—the first was Dr. W. W. Francis.

## APPOINTMENT IN THE HISTORY OF MEDICINE

Dr. J. K. Crellin has been appointed Director of the Medical History Program of Duke University. Educated at London University in pharmacy, history and philosophy of science, and in medicine, Dr. Crellin was on the staff of the Wellcome Institute for the History of Medicine from 1966 to 1976. Immediately prior to arriving at Duke he was in the Department of Medical Humanities at Southern Illinois University School of Medicine. His research interests, which have been wide, currently focus on the history of therapy in Britain and on medicine in 19th-century America.

Since its opening in 1930, Duke University Medical Center's commitment to medical history has been growing steadily. Much impetus came from the Trent Collection, the important medical history library of the late Josiah C. Trent. The graduate and undergraduate program was expanded by Dr. Gert Brieger (now Chairman, History of Health Sciences, University of California, San Francisco), and recently an M.A. in the history of medicine has been added to the program.

# ANNOUNCEMENTS

## NATIONAL NEWS

*American Society for Microbiology Archives Collection*

November 20, 1978, marked the official opening of the Archives Collection of the American Society for Microbiology at the University of Maryland Baltimore County, Catonsville, Maryland. The opening ceremonies included four symposia devoted to "Infectious Diseases," "Nitrogen Fixation," "Cloning and Recombinant DNA," and "Industrial Microbiology." The Microbiology Archives are housed with the Special Collections of the UMBC Library and are described in detail in L. S. McClung's article, "The ASM Archives Collection of Books, Laboratory Manuals, Theses, and Reprints," published in *ASM News*, 1978, *44:* 451-458.

*Boston*

"Leaders in American Medicine," a multi-institional film and discussion program about distinguished physicians, is being sponsored jointly by Boston University School of Medicine, Benjamin Waterhouse Medical History Society, Boston Medical Library, Brown University Medical School, Harvard Medical School and Tufts Medical School. The program, which is under the chairmanship of George E. Gifford, Jr., is made possible by a grant from the Josiah Macy, Jr. Foundation to the Section on History of Medicine, Boston University School of Medicine. The program for 1978-79 featured films of Varaztad H. Kazanjian (Oct. 8), Hans Selye (Nov. 8), William Dameshek (Feb. 7), and Charles H. Best (March 7). Discussants included Bradford Cannon, John M. Converse and Walter C. Guralnick; Hans Selye, George W. Thorn and Sanford I Cohen; William C. Moloney, Robert S. Schwartz and Jane F. Desforges; and Alexander Marble and George F. Cahill, Jr.

*Johns Hopkins Medical Institutions*

The first symposium in a humanities series sponsored by Hopkins and the Peabody Conservatory of Music, was held on November 14, 1978. The symposium, "Music and Medicine," featured Jacques Barzun whose address, "In Praise of Apollo: Musician and Medico," was followed by a panel commentary. Panelists included Maestro Sergiu Comissiona, conductor of the Baltimore Symphony, composer Robert Hall Lewis, and music critic Irving Lowens. Physician panelists were Ernest Bueding (pharmacology), Bertram S. Bronn (mental health), George Udvarhelyi (neurosurgery), and Owsei Temkin (medical history). Elliott Galkin, director of the Peabody Institute, served as moderator. A concert combining talents of the Johns Hopkins Medical Institutions and the Peabody Conservatory of Music completed the program. Future symposia in the series will focus on the relationship between medicine and disciplines such as literature and philosophy.

*Symposium on Philosophy and Medicine*

The Ninth Symposium, "Ethical Issues in Clinical Diagnosis," will be held in Madrid, Spain, March 21, 22 and 23, 1979. It will be held through the support of the

Fundacion Juan March and D. Reidel Publishing Company and under the aegis of the Instituto "Arnau de Vilanova." Principal papers will be presented by Professors Pedro Laín Entralgo, Ignacio Ellacuria, H. Tristram Engelhardt, Jr., Edmund Pellegrino, Kenneth Schaffner, Heinrich Schipperges, E. Seidler, Stuart F. Spicker, Dietrich von Engelhardt and Marx W. Wartofsky. The Symposium will serve to convene physicians, nurses and philosophers of medicine in Europe and the United States. Application for AMA Category I credit for physicians (Continuing Medical Education) has been submitted. The edited proceedings of this Symposium will be published as volume XI in the series *Philosophy and Medicine* by D. Reidel Publishing Company, Dordrecht, Holland/Boston, U.S.A.

*University of Chicago*

The 35th D. J. Davis Lecture on Medical History followed by the dedication of the D. J. Davis Center for Medical History will take place in the Special Collections Department of the Library of the Health Sciences, University of Illinois at the Medical Center, on April 25, 1979. The lecturer, Doctor Joseph H. Kiefer, Professor Emeritus of Urology of The Abraham Lincoln School of Medicine, University of Illinois College of Medicine will speak on "The Doctor's Image Reflected in the Matula."

*University of California, San Francisco*

The Department of Pharmacology and Experimental Therapeutics and the Department of the History of Health Sciences, have chosen as the Third Chauncey D. Leake Lecturer, Professor Erwin H. Ackerknecht, M.D. A world-renowned medical historian and the recipient of numerous honors and awards, Professor Ackerknecht is Professor Emeritus of the History of Medicine, University of Zurich. The lecture will take place on May 23, 1979, in Cole Hall, at the University of California, San Francisco Medical Center.

*University of Wisconsin*

Dr. Guenter B. Risse, Department of the History of Medicine, University of Wisconsin-Madison, has recently returned from an extensive trip to Brazil and Argentina, where he was invited to give several lectures. While there he was elected an honorary member of the Ateneo de Historia de la Medicina de Buenos Aires and the Sociedad Argentina de Humanismo Médico. In September, 1978, Dr. Risse was also elected a corresponding member of the newly created Asociación Peruana de Etnologia Medica e Historia de la Medicina in Lima, Peru. In addition, he has received a W.H.O. fellowship to study the historical background of Latin American medical systems in Mexico, Guatemala, Venezuela, and Peru. During the summer of 1979 he intends to establish personal contacts with a number of medical historians and evaluate archival sources to be used in future research.

*Webster Society Prize*

The Webster Society, a group composed of the former residents of Dr. Jerome Pierce Webster and the trainees of the Columbia Presbyterian Medical Center Plastic Surgery Service, announces an essay contest in the History of Plastic Surgery The Webster Society seeks to honor Dr. Webster's commitment and contribution to medical historical studies by establishing an essay contest open to plastic surgeons and all others interested in medical history. A $1000 cash prize will be given biennially with the first award to be made in 1980. The deadline for submission of essays is April 15, 1980. All essays should be sent to and further information obtained from Dr. Bard Cosman, Columbia Presbyterian Medical Center, 161 Fort Washington Ave., New York, N.Y. 10032

## PROGRAMS OF MEETINGS

*College of Physicians of Philadelphia*

September 20, 1978: "Early Toolmakers to Physicians—Hippocrates to Laennec," Palmer H. Futcher.

October 18, 1978: "The Development of Radiologic Imaging, 1895-1978," William J. Tuddenham and Roderick L. Tondreau.

November 15, 1978: "Electrocardiography—Some Morsels of Its History," Francis C. Wood.

January 17, 1979L "Presentation on Pulse and Circulation," Orville Horwitz.

*Handerson Medical History Society*

January 22, 1979: "Art and Medicine," Ralph I. Fried.

*Josiah C. Trent Society for the History of Medicine, Durham, N.C.*

November 9, 1978: "The Medical Poster: An Illustrated Survey," William Helfand.

November 30, 1978: "The Worshipful Society of Apothecaries of London—An Illustrated History," T. Douglas Whittet.

*Washington Society for the History of Medicine, Washington, D.C.*

January 18, 1979: "Personal and Professional Influences on My Career as a Scientist," Julius Axelrod; "The Apprenticeship of Philippe Pinel: A Brief Encounter between Theory and Empiricism," Dora B. Weiner.

## NEWS FROM ABROAD

*Italy*

Professor Raffaele Bernabeo has been appointed to the Chair of History of Medicine at the University of Bologna. In this post he succeeds his former teacher, Professor Vincenzo Busacchi, who has recently retired.

*Switzerland*

The 1978 award of the Henry E. Sigerist Prize, established by the biochemist Markus Guggenheim-Schnurr and his wife in 1968, has been made to Dr. Rudolf Schmid. Dr. Schmid's prize-winning doctoral thesis was directed by Professor Huldrych M. Koelbing, head of the Institute of the History of Medicine at the University of Zurich, and is entitled, *Die Medizin im Oberhalfstein bis zum Beginn des 20.*

*Jahrhunderts*. The study was published by Sauerlander AG, Aarau, in the series "Veroffentlichungen der Medizin und der Naturwissenschaften," Nr. 31.

## EDITORIAL JOB OPENING

Part-time Assistant in the Johns Hopkins Institute of the History of Medicine to be Assistant Editor of the *Bulletin of the History of Medicine*, beginning July 1, 1979. Opportunity to work in an academic environment with a scholarly journal. Strong background in English and writing essential. Acquaintance with other languages important as well, French, German and Latin in particular. Background in history, science or journalism. Some acquaintance with medical terminology indispensable. Familiarity with use of library for research. Scholarly and editorial experience. Salary to be arranged in accordance with previous experience. Send resume to Dr. Lloyd G. Stevenson, Editor, *Bulletin of the History of Medicine*, 1900 East Monument Street, Baltimore, Maryland. Equal Opportunity Employer M/F.

# BOOK REVIEWS

HENRY K. BEECHER and MARK D. ALTSCHULE. *Medicine at Harvard: The First Three Hundred Years.* Hanover, N.H.: University Press of New England, 1977. xiv + 587 pp. Ill. $27.50.

This comprehensive history of the Harvard Medical School is divided into four unequal parts. Part I entitled "The Earliest Stages" takes up 84 pages. Part II, "The Eliot Years, 1869-1909," encompasses 85 pages. Part III, "The Great White Quadrangle: From 1906," is by far the largest section covering 283 pages. Part IV considers non-curricular factors in the growth of the school (63 pages) and there is a short epilogue.

The Harvard Medical School was the third medical school to be established and the first after the conclusion of the Revolutionary War. In 1781, at about the time of the surrender of Cornwallis, the President and Fellows of Harvard College requested Dr. John Warren (1753-1815) of Boston to prepare the outlines of a plan for a medical institution. Warren's plans were accepted and the formal opening of the school took place in 1783. For the first 25 years of its existence the school's faculty consisted of three people: John Warren, Professor of Anatomy and Surgery; Benjamin Waterhouse, Professor of the Theory and Practice of Physic; and Aaron Dexter, Professor of Chemistry and Materia Medica. When the medical school opened there was no hospital in Boston and the facilities for clinical instruction remained quite inadequate until the opening of the Massachusetts General Hospital in 1821. Thus, the subtitle of the book is somewhat misleading as the school opened its doors less than two centuries ago and, until the advent of Charles Eliot as president of the University, it remained largely autonomous and essentially proprietary.

Part II deals with the period of Charles Eliot's presidency of Harvard University. Eliot revised the entrance requirements, arranged for the University to collect the fees, established a three-year graded curriculum, and won the loyal support of the famous Oliver Wendell Holmes after a vigorous confrontation with many of the faculty. It was during this important confrontation that Gilman invited Eliot, among others, to advise him about the organization of the new medical school in Baltimore. What is not pointed out is that Eliot, in contrast to Gilman at Johns Hopkins, placed no real emphasis on time for research. Unlike President Gilman, whose eye was always focused on scholarly achievement and research ability, Eliot was primarily interested in pedagogy. The following anecdote related by his biographer, Henry James, clearly reveals the order of priorities in Eliot's mind: "Professor C. L. Jackson relates that when he was a young teacher of chemistry in the seventies he asked Eliot if he might be relieved of the duty of teaching one class in order to prosecute certain investigations. The President, in his stately manner, propounded a question to which an answer can seldom be given—'What will be the result of these investigations?' They would be published, was the reply. The President wanted to know where Mr Jackson named a German chemical journal. 'I can't see that that will serve any useful purpose here,' said Eliot, and therewith dismissed the matter."

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Here one finds an important reason for Flexner's choice of Johns Hopkins as his model—not because of any lack of appreciation of what Eliot had done at Harvard. Flexner was well aware of the fact that disease was the price of ignorance, that medicine would not progress until the methods of science were applied to the study of disease in man. Edsall's appointment at Harvard was very important as he appreciated what was necessary to advance clinical science. Edsall immediately embraced Harvard's belated drive in this direction. But it was not Eliot who was responsible for Edsall's appointment. It was E. A. Locke who provided the stimulus and gained the support of Shattuck and Cabot to bring about this momentous event in the history of Harvard medicine.

In spite of the authors' concerns, the foresighted contribution Eliot made to medical education was quickly recognized and schools like the University of Pennsylvania (1877) and the University of Michigan (1880) soon lost their hesitancy in establishing a three-year graded curriculum. Each of these schools had succeeded in acquiring a new hospital under university control. This deficiency at Harvard had greatly concerned Eliot: "More than once this limitation of choice has proved unfortunate. More than once the school and the community have lost an important medical reinforcement because the school was not in a position to offer the desired person an adequate hospital appointment as well as a professorship."

In Part III entitled "The Great White Quadrangle," a section which comprises more than half of the volume, the authors relate how this problem was finally solved when David Linn Edsall of Philadelphia (1912) was appointed Jackson Professor of Clinical Medicine and chief of the Medical Service at the Massachusetts General Hospital. Under his guidance, clinical science had a rapid growth at Harvard. When the Peter Bent Brigham Hospital opened in 1912, Henry A. Christian was professor of medicine and physician-in-chief of the hospital and Harvey Cushing professor of surgery and surgeon-in-chief. Thus, Harvard achieved in 1912 what had been accomplished at Johns Hopkins in 1889. The clinical professors had responsibility for both the medical school and hospital activities related to their department.

In their appraisal of the Flexner Report, the authors fail to put these events in their proper perspective and historical objectivity seems to give way to personal opinion sometimes based on incomplete knowledge. One need only look at the situation at Harvard in 1910 to understand Flexner's choice. Harvard was still a local school, two-thirds of its students coming from Boston, no clinical professor had ever been chosen from outside Boston, clinical teaching was largely by precept and lecture and there was little in the way of clinical science. The full-time system in clinical departments is presented by the authors as a black and white proposition rather than as a variable mixture as it has evolved in almost every school. Eliot does not need to be defended in relation to what he did for medical education. He made enormous contributions. Flexner does not deserve to be degraded—he did too by stirring the pot vigorously at a most propitious moment in the historical evolution of American medical schools. Medical science, medical education and medical practice would not have achieved the high plane on which they now stand without a standing army of full-time clinical scientists who could devote in principle their full-time to the teaching of clinical medicine and to the application of the methods of science to the solution of the problems presented by human disease. Their many successes which resulted in a fantastic accumulation of medical knowledge with the sprouting up of specialties within specialties, major technological advances, and the many extracurricular duties now imposed on them have created many of the present problems. True, the present full-time faculty, both preclinical and clinical, are no longer full-time in the most desirable sense. But changes in the fabric of medicine and medical administration are responsible for this and not the basic principle of full-time academic medicine. The young faculty members today are just as devoted to the well being of their patients as those in the past. The hospital substrate in which they must work is vastly different.

Rather than pursuing the objective "to correct distortions in accounts of the role played by the Johns Hopkins Medical School between 1893 and 1925" (indeed, the authors do not seem to be aware of the many important accomplishments in Baltimore from 1876 to 1893), it would have been far better to have emphasized the important and fruitful migrations of faculty from one school to the other. Johns Hopkins will always be grateful to William S. Thayer and J. M. T. Finney who left Boston to pursue their careers in Baltimore, while the many prominent members of the Harvard faculty who received a significant part of their training in Baltimore (Councilman, Wright, Christian, Pratt, Peabody, Shattuck, Albright, Cushing, Berry, Blackfan, Gamble, Janeway, Ropes, Thorn, Cobb, Avery, Eisenberg, Wislocki, Ball, Keefer, Minot, Avery, Campbell, Kuffler, Hunt, Hay, and Braunwald, among others) have added lustre to the outstanding accomplishments of that school of medicine.

The authors' judgment of the importance of various events in Harvard history is open to question. For example, far too much space is devoted to a catalogue of those who served in a number of minor departments and who made no very notable contributions. On the other hand, too little emphasis is placed on the outstanding contributions of Francis Peabody, George Minot, William Castle, Maxwell Finland and the many outstanding colleagues in the Thorndike Memorial Laboratory. There is no critical examination of the relationship of the medical school to its major teaching hospitals and to many of the outstanding contributions to basic and clinical science that have been made by members of these institutions.

The authors highlight the years of Edsall's deanship while his successors receive inadequate treatment in view of their major contributions. The opinion expressed of the stewardship of Robert Ebert would not be shared by most Harvard alumni and this evaluation illustrates the lack of historical objectivity which emerges so often in this volume. The authors do not provide a carefully documented brief in support of their conclusion that "the Harvard Medical School today is being run for the benefit of the faculty and not the students."

The proofreading leaves something to be desired as repeatedly cross references to pages are not filled in and appear as (p. 000).

In summary, this volume is a rather slanted view of the Harvard Medical School which might be characterized as the authors labelled Oliver Wendell Holmes' lecture at the 100th anniversary of the school—"altogether a pedestrian account." Holmes' remarks, however, were much more valuable than the authors indicate, and, despite the controversial aspects of the present account of medicine at Harvard, it is a welcome addition to the other historical accounts of this outstanding school of medicine.

Reviewed by A. McGehee Harvey, M.D., Director Emeritus and Professor Emeritus, Department of Medicine, The Johns Hopkins University, School of Medicine, Baltimore, Md.

Charles E. Rosenberg. *No Other Gods; On Science and American Social Thought.* Baltimore and London: The Johns Hopkins University Press, 1976 xiii + 273 pp. $13.00.

Charles Rosenberg is a stimulating and influential historian who here offers a collection of insights "on science and American social thought." In one essay after another, he interprets the complex relationships between the scientific enterprise and the matrix of social concepts and assumptions. He demonstrates, beyond argument, the fundamental social motivations behind selected scientific efforts and achievements and the sometimes controlling influence of science upon the beliefs of society.

Yet the book is unnecessarily difficult. The problem is only partly a result of the fact that the book is a composite of previously published articles, rewritten for this format. That does, however, have something to do with the question which confronts the reader of what one episode may have to do with the preceding one. More central are the patches of slow and obtuse writing. This is the more difficult to explain, since in earlier books, especially in the *The Cholera Years,* Rosenberg wrote in clear, direct sentences. Here, by contrast his nouns are sometimes modified by two adjectives, each of which carries its own modifiers! Paragraphs are logical but transitions sometimes require retracing. The writing is difficult, not the ideas.

The book can be divided into two halves. In the first, he presents a series of case studies, all relating to the intrusion of scientific concepts into the texture of man's understanding of important social questions. The questions are ones Rosenberg has been probing for several years, all included within medicine, public health, and social physiology. Most of the topics, such as heredity and disease, eugenics, sexuality, woman's role, and nervousness, represent fields in the process of developing standards of inquiry and rigor. Some of them, for example eugenics and phrenology, may be denigrated as pseudosciences, but all existed in an in-between realm where cultural tradition, social need, approaches which had been effective in other scientific fields, and subjective judgement interacted. Several involve appeals to science for emotional assurance. Ideals are pursued through different scenes: the scientific ideal in society in the case of Arrowsmith, the ideals carried by Charles B. Davenport in his hereditarian crusade, and the idealized woman in conflict with change. New perceptions abound but there is no predicting when or where they are going to appear.

The second half of the book is centered upon the agricultural experiment stations movement, a story examined carefully in terms of the development of an American scientific community. Here the economic needs of society and the scientists' need for institutional support were brought together by government action. None of this just happened, and Rosenberg is at his best in tracing the thoughts and actions of the actors in the drama. He makes the results, which differed from the aims of the projectors, not merely understandable but almost inevitable.

His final chapter provides a satisfying conclusion, within the promise of an already influential introduction which asserts the necessity of studying science and social thought within the same context. After discussing hybrid corn and tracing the discovery of vitamin A, an unpredictable scientific result of practical studies, he turns to scientific genetics of T. H Morgan. If much of the rest of the book concerns in-between worlds, Morgan's genetics represented significant achievement based upon model scientific inquiry. But, Rosenberg shows, this did not just happen, either. It rested upon the institutions and the scientific community produced by the kinds of interplay discussed throughout the book.

It is to be hoped that all those who need these insights will find their way to it, for example, agricultural historians who do not usually follow intellectual and cultural history. The book, despite its difficulties, will repay careful reading in many ways and in many fields.

Reviewed by BROOKE HINDLE, Ph.D., Senior Historian, National Museum of History and Technology, Smithsonian Institution, Washington, D.C.

S. V. HUMPHRIES. *The Life of Hamilton Bailey, Surgeon, Author and Teacher of Surgery*. Beckenham, Kent, England: Ravenswood Publications, 1973 (Distributed by British Book Center, Elmsford, N.Y.). xiii + 72 pp. Ill.

Hamilton Bailey deserved this short, balanced biography. His lucid, superbly illustrated textbooks were read by students all over the world. He revised his *Physical Signs in Clinical Surgery* to reach thirteen editions; his *Short Practice of Surgery*, written with McNeill Love, needed a new edition every two and a half years during a quarter of a century; while his biographer considers his *Emergency Surgery* (1930, 7th edition 1958) the best gift ever made by a busy city surgeon for his colleagues in the outback who use it as a "Bible." Bailey wrote thirteen books, several of them for nurses, and did much editorial and journal writing as well.

Why was this great teacher never a professor in a medical school, and this brilliant surgeon never prominent in any college or society? Mr. Humphries, himself a surgeon who has worked in well-equipped places and undeveloped countries, contacted many people who knew Bailey and learned the faults of temperament which made him seem overbearingly impatient to some, while others "loved him on this side of idolatry." Bailey was a loner, who drove himself harder than his helpers. This "Life" shows how he deployed his vast knowledge of current surgical literature in his large practice and in his books. He had few recreations, no social life except to welcome students at his home, and never went to a meeting. He was generous to helpers and patients, valuing money only as a means for working harder and more efficiently. His able and imperturbable wife helped with the production of his books, smoothed his path among people, and rescued him from a long mental breakdown after the tragic death of their schoolboy son in a train accident. His life was a triumph of courage through many adversities.

Reviewed by WILLIAM LEFANU, M.A., formerly Librarian, Royal College of Surgeons of England, London.

RICHARD D. FRENCH, *Antivivisection and Medical Science in Victorian Society.* Princeton: Princeton University Press, 1975. ix + 425 pp. $25.00 (cloth), $9.95 (paper).

Richard French's book is an enormous piece of careful, detailed, *complete* history-writing. He has read every possible primary and secondary source, its very totalness makes it hard for the reader, or reviewer, to grasp whole.

To place it historiographically, it may help to compare it with an earlier account of the anti-vivisection movement, Bretschneider's *Der Streit um die Vivisektion im 19 Jahrhundert*[1] which appeared in 1962 Bretschneider starts off with a sketch of the growth of scientific medicine as it defeated and replaced the older medicine of *Naturphilosophie*, and he casts the antivivisectionists as the *Nachfahren*, the heirs and successors of *Naturphilosophie*. He quotes one of the antivivisectionists as saying that it is degrading to regard organisms as physico-chemical machines. For Bretschneider, to whom organisms *are* physico-chemical machines, and *Naturphilosophie* is ridiculous, the mistakes in the antivivisectionists' writing about physiology are even more absurd now than they were then.

French sets out on a completely different tack historiographically. His "influences" among the historians include, among others, Charles Webster in Oxford, R. M. Young in Cambridge, and R. M. MacLeod in Sussex. For these historians, and for French, a structure which consists, like Bretschneider's, entirely of a conflict of ideas, with God on the side of positivism, is not an acceptable analysis. As he says (p. 408), his choice of material "has been predicated upon the idea that the historical interest attaching to the movement involves not cruelty to animals, so much as the tensions surrounding the roles of science and medicine in society "

And as an example of what he means by "society" (p. 144), "What is the historian to make of the events leading up to the passage of the Cruelty to Animals Act of 1876? Here, it will be convenient to consider this question from the point of view of the adoption by the government of a new administrative responsibility, and second, from the point of view of the response of the medical and scientific communities to political pressure."

Both French and Bretschneider begin with a discussion of the development of experimental medicine in Britain, separating from medical practice from around 1870. But where Bretschneider is more deeply concerned with the literary and stylistic analysis, and the validity of the arguments of vivisectionists and antivivisectionists, French moves immediately from scientific medicine *per se* to the political scene: the working of the antivivisectionist's pressure group, the Victoria Street Society, and the mechanics of political lobbying both before and after the 1876 Act to regulate vivisection. He contrasts the political activists with the more politically naive biological sciences; their organization, the Physiological Society, though it was called into existence as a reply to antivivisectionism, soon became a means of publishing new research within a group of colleagues, rather than a political pressure group. He quotes R. M. MacLeod's remark that "the academic expert still inclined towards the tragic hero rather than the statesman of science."[2] The political pressure applied to the government to persuade it to regulate vivisection is paralleled by the pressure put on it by the societies advocating government action on the question of chronic alcoholism, and those pressing the state into accepting its role as a provider of medical care. These problems have been treated by R. M. MacLeod[3] as case studies of the gradual extension of state responsibility, as demanded by the pressure groups and resisted by the government itself in the rôle of *Nachfahrer* to *laisser-faire* economics and J. S. Mill's politics of personal liberty. Intertwined with this socio-political struggle is the chronicle of the slow change from politically naïve "experimental medicine" to physiology as a profession, that is, to a group with enough autonomy, to quote Eliot Freidson,[4] to regulate its own relationship with the state.

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[1] H Bretschneider, *Der Streit um die Vivisektion im 19 Jahrhundert* (Stuttgart Fischer, 1962), no 2 in the series "Medizin in Geschichte und Kultur" edited by R Herrlinger and K E Rothschuh.

French has attempted the immensely difficult task of conducting at least two interrelated analyses at the same time, as well as making use of many of the newer explanatory ideas at present being discussed by historians of medicine and science. As a comparison with the historiography of the sixties, exemplified by Bretschneider, makes plain, French's book could only have been written in the seventies. The history of ideas approach, which succeeded the positivistic history of discovery, is itself succeeded by the sociology of science. It is noticeable that when ideas are mentioned by this school, they tend to be those at a lower level of sophistication, the pamphleteers, rather than the *Naturphilosophers*. The assumption that the key to the thought of an era is to be found in its high culture has given way to a search for it in the social and political pressures which mould the self-consciousness of groups: philology and philosophy are replaced by sociology as the basic science of history, the history of ideas becomes the history of ideologies.

The diminution of belief in high culture which is so typical of the historiography of the seventies may itself be seen as a reflection of the diminishing funds to support it. As French points out, the literature on the relation of science and society often at present shows them to be hostile to each other, with scientists struggling for both support and autonomy in a community which questions the value of their work. As a contemporary historian, French speaks as much to his contemporaries in the medical and scientific professions, as to other historians. This study, which is the product of a great deal of detailed research, is one of those which serve to deepen the self-consciousness of the members of today's profession in discussing the predicament of the profession in the past.

Reviewed by PAULINE M. H. MAZUMDAR, M.B., Ph.D., Hannah Professor of the History of Medicine, University of Toronto, Toronto, Ont.

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[2] R. M MacLeod, "Science and government in Victorian England: Lighthouse illumination and the Board of Trade 1866-1886," *ISIS*, 1969, *60* 5-38 (p 38—cited by French on p. 151).

[3] R. M. MacLeod, "The edge of hope: social policy and chronic alcoholism, 1870-1900," *J. Hist. Med.*, 1967, *22* 215-245.

[4] E. Freidson, *Profession of Medicine. A Study of the Sociology of Applied Knowledge* (New York: Dodd, Mead, 1975), pp. 23-24 (cited by French, p. 150)

## THE BULLETIN RECENTLY RECEIVED*

*Albrecht von Haller, 1708-1777.* Zehn Vortrage gehalten am Berner Haller-Symposion von 6. bis 8. Oktober 1977. Sonderdruck aus des *Verhandlungen der Schweizerlichen naturforschenden Gesellschaft* (SNG), wissenschaftlicher Teil, Bd. 1977. Bern: Stadt-und Universitatsbibliothek [1977]. x + 182 pp. Ill. Swiss Fr. 40.

Garland E. Allen. *Thomas Hunt Morgan: The Man and His Science.* Princeton, N J.: Princeton University Press, 1978. xvii + 447 pp. Ill. $25.00.

Christian Andree. *Rudolf Virchow als Prähistoriker.* Band 1: Rudolf Virchow als Begrunder der neueren Deutschen Ur- und Fruhgeschichtswissenschaft. Band 2: Briefe Virchows und seiner Zeitgenossen. Koln: Bohlau-Verlag, 1976. 267, 541 pp. Ill. Bd. 1, D.M. 68; Bd. 2, D.M. 120.

Kenneth Dewhurst and Nigel Reeves. *Friedrich Schiller: Medicine, Psychology and Literature.* With the first edition of his complete medical and psychological writings. Berkeley & Los Angeles: University of California Press, 1978. xii + 413 pp. Ill. $22.50.

Markus Fierz. *Girolamo Cardano (1501-1576): Arzt, Naturphilosoph, Mathematiker, Astronom und Traumdeuter.* Basel & Stuttgart: Birkhauser Verlag, 1977. 140 pp. Swiss Fr. 1980.

Luis Garcia-Ballester, Guillermo Olagüe and Miguel Cigas. *Classics in Modern Otology.* Granada: at the University Press, 1978. 354 pp. Ill. $18.00.

Luis Garcia-Ballester. *Historia social de la medicina en la España de los siglos XIII al XVI.* Vol. 1: La minoria Musulmana y Morisca. Madrid: Akal Editor, 1976. 217 pp.

Maurice Bear Gordon. *Naval and Maritime Medicine during the American Revolution.* Ventnor, N.J.: Ventnor Publishers, 1978. ix + 134 pp. Ill. $17.50.

Barbara J. Harris. *Beyond Her Sphere: Women and the Professions in American History.* Westport, Conn.; London, Engl.: Greenwood Press, 1978 (Contributions to Women's Studies, No. 4). x + 212 pp. $15.95.

Gerald W. Hartwig and K. David Patterson (Editors). *Disease in African History. An Introductory Survey and Case Studies.* Durham, N.C.: Duke University Press, 1978 (Duke University Center for Commonwealth and Comparative Studies, 44) xiv + 258 pp. $13.75.

A. McGehee Harvey. *The Interurban Clinical Club (1905-1976).* A Record of Achievement in Clinical Science. Philadelphia: W. B. Saunders, 1978. xiii + 573 pp. Ill.

P. Laín Entralgo. *Historia de la Medicina.* Barcelona: Salvat Editores, 1978 (Biblioteca Médica de Bolsillo). xxxi + 721 pp.

Laurence B. McCullough and James Polk Morris, III (Editors). *Implications of His-*

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* The *Bulletin* reserves freedom of decision as to the publications to be included in this section Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

*tory and Ethics to Medicine—Veterinary and Human*. College Station, Tex.: Texas A & M University, 1978. xvi + 158 pp. $5.00.

Francisco Méndez Alvaro. *Historia del periodismo médico y farmacéutico en España*. Introducción, editión e indices de J. Riera. Valladolid, 1978 (Acta Historico-Médico Vallisoletana, Monografías, VIII). 179 pp.

Axel Hinrich Murken. *Das Bild des deutschen Krankenhauses*. Münster: Verlag Murken-Altrogge, 1978 (Studien zur Geschichte des Krankenhauswesens, Bd. 12). 235 pp. Ill.

Darryl B. Podell and Walter W. Walker. *Archives Procedural Manual*. 2nd ed. rev. St. Louis: Washington University School of Medicine Library, 1978. v + 134 pp. $7.25.

David L. Ransel (Editor). *The Family in Imperial Russia: New Lines of Historical Research*. Chicago-Urbana-London: University of Illinois Press, 1978. 342 pp. $16.50.

James Reed. *From Private Vice to Public Virtue: The Birth Control Movement and American Society since 1830*. New York: Basic Books, 1978. xvi+ 456 pp. Ill. $17.95.

Victor Richards. *The Wayward Cell: Cancer, Its Origins, Nature, and Treatment*. Berkeley-Los Angeles-London: University of California Press, 1978. 2nd ed. xix + 407 pp. Ill. $12.50.

Hertha Riese (Editor). *Historical Explorations in Medicine and Psychiatry*. New York: Springer Publishing Co., 1978. xxiii + 232 pp. Ill. $22.95.

Eucharius Rösslin, the Younger. *On Minerals and Mineral Products*. Chapters on Minerals from his "Kreutterbüch." Critical Text, English Translation, and Commentary by Johanna Schwind Belkin and Earle Radcliffe Caley. Berlin-New York: Walter de Gruyter, 1978 (Ars Medica, IV. Abt., Bd. 1). xxxvii + 418 pp. Ill. D.M. 240.

Karl Ed. Rothschuh. *Iatromagie: Begriff, Merkmale, Motive*. Opladen: Westdeutscher Verlag, 1978 (Rheinische- Westfälische Akademie der Wissenschaften. Geisteswissenschaften, Vortrage G 225). 60 pp. Ill. D.M. 15.80.

Gwendolyn Safier. *Contemporary American Leaders in Nursing: An Oral History*. New York: McGraw Hill Book Co., 1977. vii + 392 pp. Ill.

Rudolf Schmid. *Die Medizin in Oberhalbstein bis zum Beginn des 20. Jahrhunderts*. Aarau-Frankfurt a.M.-Salzburg: Verlag Sauerlander, 1978 (Veroffentlichungen der Schweizerischen Gesellschaft für Geschichte der Medizin und der Naturwissenschaften, 31). 108 pp. Swiss Fr. 25.

Lazzaro Spallanzani. *I giornali della sperienze e osservazioni relativi alla fisiologia della generazione e alla embriologia sperimentale*. Manoscritti inediti a cura di C. Castellani e V. G. Leone. Milano: Episteme Editrico, 1978. 483 pp. Ill.

Edward H. Spicer (Editor). *Ethnic Medicine in the Southwest*. Tucson: University of Arizona Press, 1977. viii + 291 pp. $16.50 (cloth); $6.95 (paper).

Brian Watkin. *The National Health Service: The First Phase, 1948-1974 and after*. London-Boston-Sydney: George Allen & Unwin, 1978. $18.75 (cloth); $8.75 (paper).

A. Windorfer and R. Schlenk. *Die Deutsche Gesellschaft für Kinderheilkunde: Ihre Entstehung und historische Entwicklung*. Berlin-Heidelberg-New York: Springer-Verlag, 1978. 199 pp. Ill. $19.00.

## CONTRIBUTORS TO THIS NUMBER

THOMAS M. ADAMS, Ph.D., is Assistant Professor, University Honors Program, 1153 Patterson Office Tower, University of Kentucky, Lexington, Ky. 40506

W. BRUCE FYE, M.D., Adjunct Assistant Professor of the History of Medicine, University of Wisconsin-Madison, is Associate Cardiologist and Director, Cardiographic Laboratory, Marshfield Clinic, 1000 North Oak Ave., Marshfield, Wis. 54449

DAVID E. LILIENFELD, B.A., is a graduate student in the Mathematical Science Department, The Johns Hopkins University, Charles and 34th St., Baltimore, Md. 21218

WILHELM MOLL, J.D., is Executive Director, Health Sciences Communications Center, University of Virginia, Charlottesville, Va. 22908

GEORGE MOORE, M.D., is Associate Professor, Department of Preventive Medicine, Medical College of Virginia, Richmond, Va. 23298

FRANZ ROSENTHAL, Ph.D., is Sterling Professor of Near Eastern Languages, Graduate School, Yale University, New Haven, Conn. 06520

WILLIAM D. SHARPE, M.D., is Clinical Associate Professor of Pathology, CMDNJ-New Jersey Medical School, and Director of Laboratories, Cabrini Medical Center, 227 E. 19th St., New York, N.Y. 10003

NEIL L. SHUMSKY, Ph.D., is Associate Professor, Department of History, Virginia Polytechnic Institute and State University, Blacksburg, Va. 24061

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